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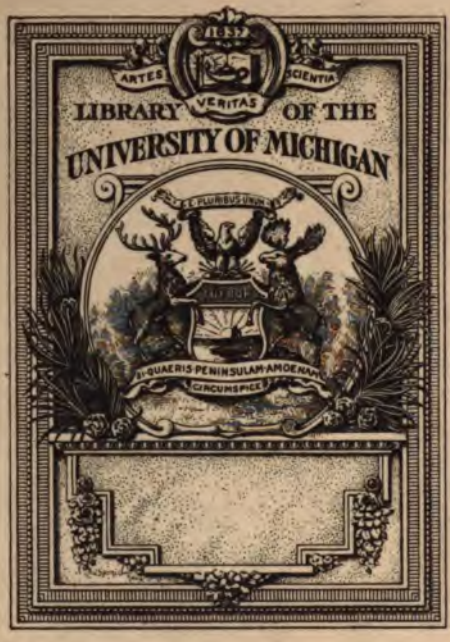
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RETROSPECT

U. of M.

OF

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M. D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

JAMES BRAITHWAITE, M. D., LOND.

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PRACTICAL MEDICINE,

&c. &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

1.—ON TYPHOID FEVER.

By Dr. JENNER, Physician to University College Hospital.

Of all rapid and acute diseases, Dr. Jenner thinks that typhoid is the one which commits the greatest ravages among young people, and that, too, in a guise not very easily detected at all times, for the eruption is far from constant. But there is one clue, which may be often available when no others are. This, says Dr. Jenner, is enlargement of the spleen, which commonly occurs in this form of disease. A case in point fell under his notice not long ago. A boy fell ill of typhoid, but gradually recovered, and was sent on a visit to another part of the country. He did not, however, seem to thrive, and the question arose whether his complaint—for it practically amounted to that—was the result of dietetic irregularity, or was a distinct relapse of the fever. There were no spots, and there was no diarrhœa, but the enlarged spleen enabled Dr. Jenner to make a positive diagnosis of relapse of the fever. Of all the facts he has been able to make out in connexion with this important disease, Dr. Jenner himself looks upon two as being the most valuable—viz., the limitation of the abdominal complication to the glandular system, and the fact that if headache prevail throughout the disease, this is due to cerebral mischief, not to the disease itself. With regard to treatment, nobody now-a-days attempts to avert or to cure a fever, yet there are probably few diseases in which a skilful physician can do more for the benefit of his patient, and this in various ways. In the first place it is known that, provided we can keep a patient alive a certain time, the disorder will pass away; hence, by studying the tendencies to death, and by obviating these, the patient may be enabled to tide over this critical period, and so be restored to health. But it is not with these tendencies alone that the physician has to deal, for throughout the whole of the disease complications of various kinds are liable to spring up, and these also must be combated. At the bottom of the whole matter, however, Dr. Jenner thinks, there is the intense prostration produced by the fever, tending to cut off the patient by pure weakness. This is to be neutralized by the exhibition of stimuli and such food as can be digested. The bowels are also apt to be attacked, as is well known, and as bleeding frequently follows the ulceration of Peyer's patches, the patient may be thus directly weakened, as well as indirectly by the diarrhœa; but the physician steps in, and an enema of a little starch water may save the patient. Opium, as tending to increase the depression of the disease, should be avoided as far as possible, although it may be necessary to exhibit a small quantity of laudanum along with the starch. Bronchitis is another common complication, and here also opium is to be avoided, its effect being to obscure sensibility, to allow the secretion of the mucous membrane to accumulate, and thus still further to deteriorate the blood by impeding respiration; rather should the disorder be treated with stimulants. Again, as the heart is paralysed by the poison in the system, as well as the other organs of the body, its beat is but feeble, and can drive the blood with but little

force; hence, in big-chested people, there may be some difficulty in forcing the blood through the lungs, so that congestions and pneumonia are apt to supervene. Whatever, therefore, will tend towards improving the condition of the heart will tend to obviate this, which is one of the most untoward events likely to occur in the course of the fever. But there is one other complication which Dr. Jenner thinks is more frequently overlooked than most, and which is more than ordinarily fatal—it is congestion of the kidneys, resulting from the impaired strength of the circulation, and from their ordinary dependent position, just as is the case with the skin of the back. This occurs more especially towards the end of the fever, and is to be carefully treated, or albuminuria and uræmia will soon carry off the patient. Dr. Jenner thinks that gentle local stimulation, as by warm poulticing and hot fomentations, not by such powerful agents as cantharides, is the best remedy.—*Med. Times and Gazette, July* 27, 1867, *p.* 90

2.—ON MIXED TYPES OF FEVER; IN RELATION TO THE QUESTION OF THE IDENTITY OR NON-IDENTITY OF THE TYPHUS AND TYPHOID POISONS.

By Dr. Henry Kennedy, A. B., Dublin.

[The question of the identity or dissimilarity of typhus and typhoid fevers seems in England to be quite decided in favor of the latter. Turn to any work lately published on the subject, or to the doctrines taught at the London hospitals, as shown in the published clinical lectures, and this will forcibly appear. Dr. Kennedy, however, believes the two forms of fever to be identical, both owing their origin to the same poison. He observes that the work of Huss on this subject has always been utterly ignored in this country. He saw fevers on a far larger scale than any British physician.]

Surely, in such a widespread disease as fever, no one is justified in asserting that what they have seen is what must have been seen by others. We know that even in the type of fever familiar to us as typhus, very great differences exist, and may be constantly seen when the disease attacks several members of the same family. In one, the head symptoms are all in the ascendant; in a second, the chest will be the part attacked; whilst in a third, it will be the stomach in the form known as gastric fever. Or, again, as regards the spots, the husband will present them and the wife not, or the parents will have them and the children not; or it may be the converse of this. In a family named Bright, of whom eight were in the hospital at the same time, and who were sent in by Dr. Carte of the Royal Hospital, the children were all spotted, whilst the mother had none, though she had a very severe attack of fever. Again, in three sisters, all adults, who were recently in hospital, only one had the regular spots of the disease; in the other two anything of rash was most indistinct; one of these latter died. Further still, in the great epidemic of 1847–48, the fever was what is known, and had been described previously, as the relapsing fever; that is, it was made up of two parts. There was a sharp attack of fever running on for five or seven days; then a lull of one, two, or three days; and again an onset of fever, usually much severer than the first, and, in very many cases, attended by spots. No one, I think, could have any doubt but that it was one and the same poison that caused the two attacks; and yet in all the recent and standard works on fever, the relapsing type is described as if it were a totally different fever from typhus, and caused by a different poison. I cannot give in my adhesion to this opinion, for I have as strong a conviction as the nature of the subject admits that the poison of typhus generates not only the type of fever known as relapsing, but other types, such as nervous, gastric, cerebral, &c., as also fever, both without and with spots, and presenting all the variety which they are capable of exhibiting; and if this view of the typhus poison be not held, insuperable difficulties, as it appears to me, must arise when we come to con-

sider analogous diseases to fever, as, for instance, scarlatina. Here every one must have seen the great variety—I might also say contrasts—which this disease often presents in the same family and at the same time. Yet no one ever thought of setting down these differences to different poisons; and why it should be necessary as regards fever it is not easy to understand. I must leave this point to others to settle.

From the tenor of these remarks it will be understood what are the views I hold on the question more immediately under discussion. I believe that the typhus poison is capable of engendering the type of fever known as typhoid or enteric, and that this particular type must be due to some other cause rather than a specific poison. On the other hand, I hold that the two types can, in the great majority of instances, be distinguished, the one from the other. When I brought the subject first before the Royal Medico-Chirurgical Society of London, in 1860, one of my arguments consisted in the detail of a few cases which were directly opposed to the views of the London physicians. In a later paper, published in the Dublin Quarterly Journal, a still larger number of cases were given, and I cannot, I believe, do better now than by giving the briefest sketch of some which have come under my notice within the last two years. But, in truth, I may say the difficulty now consists in selecting the cases, they have become so numerous. So I shall take such as bear most directly on the disputed point.

Case 1.—McKeown, aged 17, having a fine skin, passed through a very severe attack of enteric fever. Every symptom was present, and during its progress the brain was much engaged, and the tongue and lips covered with sordes. He made a good though slow discovery.

Case 2.—His brother, aged 12, from same room, was admitted under a severe attack of typhus. He had the well-marked and copious rash of the disease, and his face was quite characteristic. He had a sharp attack of diarrhœa, calling for special treatment. My friend, Dr. Hudson, was kind enough to come and confirm my diagnosis of this case. It is but right to state there was an interval of a week between the admission of these two brothers.

These two cases have been given as affording an example of the two types of fever, each well marked, coming from the same room. Others, I know, have met similar examples, and Dr. Croly of Harcourt-street, has informed me of a very striking one.*

Case 3.—McCauley, aged 18, fine skin, was handed over to my care by my friend Dr. Moore. The patient laboured under fever, and had the spots of the enteric type very well marked on abdomen and sides of the thorax, but there was no other sign whatever of this kind of fever. His illness ran on for many days, the chest becoming engaged, and when he left hospital there were signs about him as if phthisis might supervene.

Cases 4 and 5 were of a similar character to the one just given—that is, with fever, the spots were those of the enteric type, but no other symptom of that kind of fever. As they were published, however, in the Lancet for December, 1864, I shall say nothing more of them here.

Case 6.—Podesta, an Italian, 14 years of age, and of a very fine skin, admitted into hospital in September, 1865. In the course of his fever he presented a very good example of the spots said to mark enteric fever. They were few in number, and appeared on the sides of the chest and abdomen. Neither in this case was there any other sign whatever of the enteric type of the disease.

Case 7.—Develin, a young man of 17, admitted into hospital during the present month, April, 1866. He had fever, but not of a severe kind, marked by the usual symptoms, and the tongue red and furred. When he was now six days ill the spots of enteric fever appeared on the chest and abdomen,

* After the reading of the paper Dr. Croly detailed three cases of fever which occurred in the one room. The first was a case of enteric fever, the second a case of typhus, whilst the third was a mixture of the two types.

and in an unusually well-marked form. On the second day of their appearance this patient was seen by the Drs. Martin, from Berlin, who happened to be visiting the hospital. On the third day, however, the number of spots had greatly increased, and become more those of typhus, and finally the case, beginning with the spots of enteric fever, became one of irregular typhus.

Case 8.—Keegan, a man of 27, admitted March, 1865. He was labouring under heavy spotted fever. Some of the spots were large and dark, some were unusually well defined and red, and disappeared on pressure. The case, however, was one of regular typhus and the man made a good recovery.

Case 9.—Murphy, girl of 19, whilst passing through a severe attack of fever, with typhus spots, got a very sharp attack of diarrhœa, attended by tympany, and pain on pressing the ilioccecal region. Nothing checked this diarrhœa till special treatment was adopted.

Case 10.—A dumb girl, aged 24, sent into hospital from the South Union. She had bad fever, being all covered with a copius mealy rash, whilst the tongue, face, and eyes were those which mark typhus. In the progress of the attack she has got severe diarrhœa, attended by tympany, and distinct pain when pressure was made on ilio-cœcal region, and only here. This complication required specific treatment, and she got steadily but slowly well.

Case 11.—Dixon, man of 25, of a very fine skin, and thin, admitted into the Cork-street hospital, labouring under fever, and with a copious rash of typhus spots over him. His general aspect that of the same type of fever. As the disease went on he got severe diarrhœa, the discharges being a light yellow colour, and attended by distinct pain in right iliac region, and tympany. This man also required specific treatment, and the attack was one of unusual severity, marked by great distress and restlessness. His recovery, too, was much prolonged by the occurrence of several abscesses.

Case 12.—In February, 1865, Kelly, a man of 19, was admitted into hospital. He was evidently very ill; but the symptoms of typhus and enteric fever were so mixed up that I was quite unable to say to which type of the disease the case ought to be referred. He had a copious rash over the body, and his expression was that of a man in typhus. But he had also slight though marked tympany, distinct pain on pressure over the ilio-cœcal region, and a very severe diarrhœa, the discharges being of a light yellow colour. He made a very slow recovery. *This man's sister was in hospital at the same time. She had typhus.*

Case 13.—Woods, a man of 20, came in with a kind of spurious fever on him. He then went out for some days, but returned in a week with every sign of enteric fever on him except the rash. He had, however, spots on him, which to my surprise turned out to be variola in the discrete form. Whilst still in bed from this he seemed one day to get suddenly worse, and then typhus in a very severe form declared itself. During all this time he had sharp diarrhœa, and the discharges were those which I believe to be most characteristic of enteric fever, being of a light yellow colour. This patient's life was in the balance for many days, but he finally recovered.

Case 14.—Burn, a girl of 16, admitted in July, 1865. She then laboured under a severe attack of typhus, being well spotted. She was so far advanced as to be sent to the Convalescent House, when she again sickened, complaining of her head, and this again followed by great raving and high fever. When now a week ill, the spots of enteric fever made their appearance. These were unusually well marked, being few in number, and confined to the sides of the chest and abdomen; but there was no other symptom whatever of enteric fever, and they were looked for, I need scarcely say, with the greatest minuteness, nor did any such appear. At this stage of her illness the patient was seen by Dr. Murchison of London, who was visiting Dublin *at the time, but who,* I regret to say, I was not fortunate enough to meet.

Such is the series of cases which I wish to bring under the notice of the Association this evening. When added to those already given in the two former papers—and did time permit, I could have given other similar cases —they appear to me to afford the strongest proof the question is capable of eliciting, that we must consider the two types of fever known as typhus and enteric as the result of one poison. If this be not the correct view to take of the matter, I confess myself quite unable to explain the cases of the mixed types detailed this evening; for it must have been observed, as each was given, how the symptoms of each type of fever were mixed up together. As there is not time, however, to go over each symptom in detail, I shall notice but one, on which most, if not all, who hold different views from my own, seemed to have placed the greatest weight of their argument. I mean the spots said to be characteristic of enteric fever. On this point, I think I may say with certainty that these lenticular red spots, and few in number, have not the value which has been given them; for I have seen them now in many instances, and some have been given this evening where, whilst they existed, there was not another symptom of the ileum being engaged—at least I could make out no evidence of such a lesion, though looking specially for it. Here, then, were cases where the particular spots existed, but not the lesion of which they are said to be diagnostic. But, further still, I have given cases to-night where, with the enteric spots, there was also a typhus rash. As bearing on this particular point, I would just recall the case of the man Develin, where the enteric spots first appeared, then the typhus rash, and as this latter disappeared the enteric spots were again visible. If this be not a case in point, I know not what is; and I shall be glad to hear some explanation of this from any gentleman who differs from me. As regards the spots of typhus fever generally, I have got an impression that a good deal of misconception exists. I have heard some speak of the bright and the dark spots, as if there were a difference between them. On this point I can state with certainty that it is very common to see the two on the same individual, and at the same time. This may be seen on the body itself, but it is more common to have them dark on the body and a bright red on the arms. Again, the spots of enteric fever are described as recurring again and again, and this is quite true. But it does not seem to be so generally known that the same may be seen in typhus, for I have witnessed cases where a distinct second crop of eruption appeared; nor is the observation original, as I have read of it in one of the olden authors, though I cannot at this moment give his name. So also of the statement that petechiæ are never seen on the face. This is positively incorrect, as I have noted several cases where they were quite distinct. But these points are only mentioned here as bearing indirectly on the point under discussion. Still I think they are enough to show that any positive statements about the rash in fever must be received with caution, as the variety is truly very great. I cannot, however, pursue the subject further here.

In the course of these observations it has been stated that the enteric type of fever must be due to a something else rather than a particular poison; and if asked what that is, I would state my impression that it only occurs in persons of a peculiar constitution, most probably closely connected, if not identical, with the strumous. This idea I have stated before; but every year is increasing my conviction on the point, and if it should turn out to be correct, I need scarcely say how important it would be. I know not whether the idea has struck any one else, but it is not stated in any of the works on the subject that I have seen. My reasons for holding this view are the following:— The enteric fever is very constantly indeed met in persons of a fine skin, and I have now seen several instances where scars, evidently strumous, existed in the neck of persons who had this type of fever. Again, it is much more common under 30 years of age—that is, when the tendency to struma is known to be strongest. I am aware that this remark may be objected to, inasmuch as every type of fever is more frequent under 30; but what I would convey is this, that whilst typhus is common after 30, 40, and 50, enteric fever is exceedingly rare. *I myself have not* met it in any instance above 35, though

it has, I know, been seen later; but, further still, every one is aware that in the course of enteric fever the lungs are very apt to become engaged. But in place of this affection passing off with the fever, as it does in typhus, it is by no means uncommon to meet cases where signs like phthisis declare themselves. The pulse keeps up, sweating occurs, and the cough is very troublesome and hard to relieve. I have said that such is common after enteric fever, and I have been forced to send several out of hospital in this state with the hope that change of air would benefit them, and in some I know that I heard of subsequently it had proved successful. That the idea I would put forward is not without some surer foundation than mere impression, I may cite the following instance:—

Case 15.—C., a girl of 16, was admitted into the Cork-street Hospital in January of the present year. She had a very fine skin, with light eyes and hair, and laboured under enteric fever in a very well-marked form. The diarrhœa proved most obstinate; but as the abdominal symptoms yielded the lungs got very much engaged from general bronchitis of the minute tubes, and for more than ten days the dyspnœa was of the most urgent character, the lips being quite livid and the distress very great. Though the urgency of this state lessened, the pulse still kept up, and the patient began to have regular sweats, and, finally, I was able to observe that the upper part of the right lung was becoming solidified. Nor did the disease stop here, for in a period of about seven weeks I was able to trace weekly the process of softening going on, till at last a cavity formed. In this state the patient left hospital, the physical signs in the top of the lung being those of a cavity, but the rest of the lungs being apparently quite sound, and as the patient's passage had been taken for America, it is just possible the predisposition to tubercle, which seemed so strong in this girl, may be averted, and she might yet live to old age.

Lastly, on this question of the connexion, or supposed connexion, between enteric fever and the strumous diathesis, I would just advert to the great similarity which obtains between the lesion found in the fever and that which so often exists in ordinary phthisis. For my own part, I must say that I have seen many specimens where I could not distinguish them, and I shall be glad to hear any gentleman express his opinion on the point.

The general question brought before the Association this evening is not, as some think, one of mere curiosity. It is of every importance that it should be settled. The diagnosis, prognosis, and treatment of the disease all hinge upon it. For if typhus be the specific fever which some think it, it is obvious that the treatment will differ from what it would do were the enteric lesion present at the same time, and the danger of allowing such a lesion to pursue its course unchecked would indeed be very great. On the other hand, those who hold with myself that the two types of fever may arise from the one poison and coexist, will always be on the look-out for such a complication, and will act accordingly. For myself, I believe I have often had to deal with such cases, and to alter or modify the treatment as the case required, and that this is not a mere belief I have reserved for this part of my remarks the details of the following cases, which have, however, been on a former occasion detailed.

Case 16.—A girl of 20 years of age was attacked with fever of a severe kind. Raving occurred and petechiæ very early, and these latter spread over the entire body. With these symptoms there was also severe diarrhœa and tympanitis. Matters went from bad to worse, and the patient died about the fourteenth day of the fever. On post-mortem examination, the lower portion of the ileum was found extensively ulcerated, Peyer's patches being the parts engaged.

Case 17.—A boy of 14, who had already learned to drink, was attacked with fever. He had much stupor and moaning, both night and day, and he presented a copious petechial rash over the body. With this state he had also tympany and diarrhœa, and, finally, involuntary stools and death. On dination extensive ulceration in patches was found in the lower portion

of the ileum. The brain presented the usual appearances found in cases of fever, but in a lesser degree than is common. I should say at the time this case occurred I was much surprised at the result of the post-mortem, for I then believed the enteric lesion could not exist with regular typhus, which the boy otherwise presented.

Case 18.—Hill, a girl of 18, fine skin, was admitted into hospital after being nine days ill of fever, which presented all the signs of the enteric type, including the spots, which appeared the day after admission. These did not, however, go through the usual course of such spots. They gradually increased in numbers, spreading to the chest, arms, and, finally, the face, and in this state many of them could not be distinguished from regular petechiæ, being large, dark, and ill-defined. My colleague, as he was then, Dr. Aquilla Smith, saw the patient at this period. By the fourteenth day of the fever all the signs of enteric fever seemed to have subsided, but there was no corresponding change in the state of the patient. Her nights became restless, she shortly lay on her back, sordes formed on the nostrils, lips, and tongue, and she got great tremor of the upper extremities—in fact, she presented all the signs of well-marked-typhus, and died on the twenty-first day of her illness. Except in the lower portion of the ileum nothing abnormal was found, and here the signs of disease were slight, but well marked. Peyer's patches were much plainer than natural, and this became more apparent as the valve was approached, for here one of an inch in length and a third in breadth was prominent and brought out in strong relief, but it had not ulcerated. The impression given by the inspection was, that irritation had recently been going on in the part, but had somewhat subsided. The specimen was exhibited before the Pathological Society.

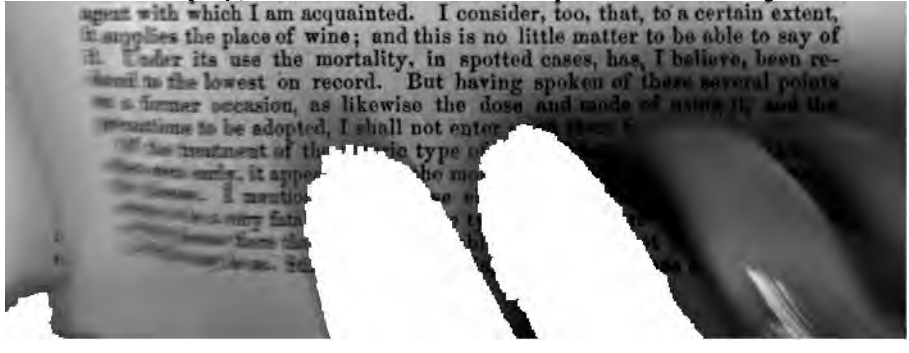
Case 19.—Bellew, a servant, aged 45, of tall stature and thin, admitted in May, 1862, with all the signs of fever in a very severe form. He had to be supported into the hospital, and though only one week ill was already densely spotted; his tongue dry and brown; eyes very much injected and expression heavy. There was also severe diarrhœa, which seemed to cease suddenly within forty-eight hours—that is, about the eighth day of the fever. From this out the attack was as genuine typhus as it is possible to describe. The spots became of the darkest, the mind very confused, with constant rambling, and passing under him. There was difficulty in putting out the tongue, and, late in the illness, hiccup. By the eighteenth day the symptoms had materially improved. The spots were gone, the tongue had expanded, and was put out better, and he took support well. It was evident, however, the fever had not resolved itself. The pulse had not fallen in proportion, nor the tongue cleaned, and he still remained heavy and at times would ramble. In this state he went on till the twenty-fifth day, when he died. There was no effort at crisis at any time nor any tympany. I was only able to examine the abdomen. The ileum had no ulceration in it, but it was very red in patches, and the more so the nearer we got to the cœcum. In this last organ the chief lesion was found, for it was ulcerated in patches, one as large as a shilling. The ascending colon had a number of small and distinct ulcers in it. The glands of the mesentery were not enlarged. It is scarcely necessary to observe that Louis' observations prove that the colon is often engaged in enteric fever, similar to what has been just described.

It appears to me these cases afford as strong a proof as the nature of the subject admits that the enteric lesion may coexist with a petechial rash, or, in other words, with typhus fever. On my own mind there now exists not a shadow of doubt of the fact, and if this be not the proper view to take of the matter, I must ask those who differ from me to explain it otherwise. What has been advanced are facts, put what interpretation on them we may. Nor would the slightest difficulty exist in giving other cases, and some striking ones have occurred within the last month; but I prefer now to glance at what others have seen; for if no one else had met similar cases to my own there would indeed be strong grounds for questioning my powers of observa-

tion, and necessarily the correctness of what I have stated. I refer, then, my hearers to the lectures of the late Dr. Todd, in which they will find some cases exactly like those given this evening—that is, the enteric type of fever attended by a copious measly rash. Some of these, too, died, and the specific lesion of the intestine was found. Again, in Chambers' "Clinical Lectures" may be found cases of exactly the same kind, and also examples of the two types of fever coming from the same room. Here, then, are two London physicians who fully bear out what has been advanced this evening, and I quote them the more readily, as they have managed to see a class of cases, which, by some strange fatality, never seem to have come under the notice of Dr. Jenner and those who agree with him; but, further, I observe that Dr. Lyons when in the Crimea met the two types of fever in the combined form, and states, especially in his work, that whilst the rash was genuine typhus the lesion often found was ulceration of Peyer's patches. In a paper, too, which has just appeared by Dr. Law on "Fever," one of the cases given is described as a typhoid case, as I believe it was, and yet the rash was a copious measly one. Lastly, the Drs. Martin, from Berlin, whose names I have already mentioned, told me the two types of fever were commonly looked on as the same disease, and that the enteric type was there called abdominal typhus. I have not the least doubt that had more time been given I could have got further evidence in the same direction; but enough, it appears to me, has been advanced for my present purpose. I do not, for a moment, assert that the question is settled on my side; but I do maintain that enough has been stated this evening to show gentlemen who differ from me the need of a cautious reserve on this question, and in not allowing themselves to come to a decided conclusion till all the facts of the case are clearly before them.

Before concluding these remarks, I would advert for a moment to one other symptom which some have thought was characteristic of the enteric type of fever—I mean hemorrhage, whether from the nose or the bowels. The London physicians especially look on them in this light, but it certainly is not correct as regards Dublin. With us typhus often exhibits epistaxis, both in its earlier and more advanced stages. In the summer it is very common, particularly when the temperature ranges high; but it is much more frequent in some years than others. And, again, as regards bleeding from the intestines, I myself have put on record some thirty cases—most of them regular typhus—in which bleedings, more or less severe, occurred, and in some that proved fatal and were examined not a trace of ulceration was found. So that bleedings cannot in any way be considered as specially diagnostic of enteric fever, and I do believe the same may be said of any other symptom that might be chosen. I would repeat, however, that it is quite another matter distinguishing between the several types of fever. This can very usually be done, and ought, of course, always be attempted; but that the types of fever will often be found united I cannot doubt, and I think the time will come when the natural history of fevers—for this is really the question at issue—will be looked on in a very different light from what it at present is.

On the treatment of fever I have here little to say. As a single remedy, and in the ordinary typhus, I find balm still the best. It seems to me to act as an antiseptic, and to fulfil the indications required better than any other



effect on it than any other type of fever. For myself, I use astringents, and from an early stage of the attack, and it is the dilute sulphuric acid on which I chiefly rely. This is the medicine recommended by Huss, and in the proportion of one, two, or three drachms to an eight-ounce mixture I have found it most useful. Two or three drops of laudanum are added to each ounce of the mixture, which is repeated according to the urgency of the case. It is, however, to be observed that the diarrhœa is only to be moderated, not directly checked; and this rule is the more important the earlier the disease is seen. If the diarrhœa be stopped too soon or too suddenly mischief elsewhere than in the intestines will arise. It may be in the chest, or the brain may be the organ that suffers. Several such cases have come under my notice; but though some of these were severe, none proved fatal. One, however, was so remarkable that I must give it here; for the checking the diarrhœa had, or seemed to have, the effect of altering the type of fever under my very eye.

Case 20.—Kelly, a man of 19, having a fine skin, was admitted into hospital, labouring under the enteric type of fever in a well-marked form. He presented the characteristic diarrhœa, and also the spots, and had been nine days ill. After three doses of the acid mixture the diarrhœa suddenly ceased, and was at once succeeded by symptoms referred to the head. His eyes, which before had been quite clear, became deeply injected; he complained of headache, his face flushed, he began to rave, and in the course of two days he presented the countenance of a well-marked typhus case, his tongue and lips being then covered with sordes. In this state, and when now about twelve days ill, his nose began to bleed, and this was repeated daily three times, so that he bled in all on four occasions. The first of these the bleeding was much the most, and they were all so obviously beneficial that they were not interfered with. The patient made a good though very slow recovery. There was no recurrence of symptoms referable to the intestines. I have seen several instances like the one just given, but none so striking, and none which proved fatal. When, however, any similar instance occurs, it may be assumed that the case is quite within our control.—*Medical Press and Circular, June* 20, 1866, *p.* 647.

3.—ON THE USE OF STIMULANTS IN THE TREATMENT OF TYPHUS FEVER.

By Dr. T. J. Maclagan, late Superintendent Dundee Royal Infirmary.

The question of stimulation is one which should be inquired into less in the spirit of an economist than in that of a physician. The rule which should guide us in the administration of stimulants should be to increase as much as possible, by every available means, the patient's chance of recovery. If, by the free administration of wine or spirits, we think that this (however hopeless the case may seem) is increased by one iota, the benefit of that chance we are bound to give him. The real question at issue is the quantity which it is proper to administer. To give on paper an exact or even fair account of the extent to which stimulants have been used in a given number of cases is exceedingly difficult. To say simply that a case was stimulated, or that so many out of every hundred were stimulated, is to convey the idea that the writer has been in the habit of using stimulants in the treatment of his cases, but conveys no information regarding the extent to which they may have been employed. Stimulation may mean two ounces of wine or twenty ounces of [illegible] Neither does it serve any useful purpose to say that so many patients and that such and such a quantity of wine was consumed, being [illegible]ach of so many ounces, when in the calculation are included [illegible]t, as well as those who were, stimulated. The ages of the [illegible] also be taken into account, old people requiring a much [illegible]ne and spirits than young. It seemed to me, also, that [illegible]ntity of stimulants consumed by each patient was a

method likely to lead to inaccuracy, as many cases might have the period of stimulation much prolonged in consequence of the existence of some other weakening cause. To take the average daily allowance seemed also open to objection, as being likely to lead to an under-statement of the extent to which stimulation was carried, besides being more difficult of application than the plan which has been adopted of taking the largest quantity taken continuously for twenty-four hours by each of the cases, adding these together, and dividing the sum total by the number of cases stimulated, and so getting the average of the maximum daily allowance. Of course, in making such a calculation, the cases not stimulated are entirely left out. This method possesses the advantage of showing better than any other, I think, the extent to which stimulation has been carried; at all events, the average is not understated. The result will be more readily seen by dividing the ages into fewer periods than by adopting the division into quinquenniads. I shall, therefore, divide them into three classes, those under 25, those from 25 to 50, and those from 50 upwards.*

It seems that of 2476 cases, 1107, or 44·7 per cent., were stimulated; the average of the maximum quantity given to each case *per diem* being 3·19 ounces of wine, and 2·6 ounces of spirits. Of those under 25 years of age, 25·8 per cent. were stimulated. the average for each being 3·5 ounces of wine, and 1·09 ounces of spirit; of those from 25 to 50, 73·7 per cent. got an average of 3·2 ounces of wine, and 3·07 ounces of spirits; whilst of those above 50, 95·3 per cent. were stimulated, the average maximum quantity being 2·1 ounces of wine, and 5·7 ounces of spirits.

It will be observed that during the last three years, but more markedly during the last two, the percentage of cases stimulated under 25 years of age is considerably less than during any of the years which precede; the quantity of stimulant given is, at the same time, just about the average; the death-rate being slightly below it. It will also be seen that in the other two periods of those years, there is a considerable increase in the quantity of spirits administered, whilst the amount of wine given is below the general average for the same periods; both the percentage of cases stimulated and the death-rate being very near it.

These two facts indicate the principles on which treatment has been conducted. In young people, as a rule, stimulation is not called for to any great extent; whilst in old people, the contrary holds good. In the young, the danger is not so much from failure of the cardiac action as from the head-symptoms; death in them is due less to asthenia than to coma. It is comparatively rare to find in a young patient the almost imperceptible cardiac impulse and inaudible systole which form so frequent and anxious a feature in the cases of old people, into the mode of production of whose death asthenia enters largely. True it is that every now and then a case occurs in a young person demanding a pretty liberal administration of stimulants, just as one occasionally meets cases advanced in life characterized by unusual mildness; these are exceptional, but serve well to show the fallacy of laying down any general rule of treatment founded on the ages of the patient, and making no allowance for individual peculiarities. When free stimulation is required, spirits are more convenient than wine; an ounce of spirits represents a greater amount of real stimulant than double the quantity of wine. It was not a very uncommon thing, in treating the above cases, to order 12 ounces of spirits *per diem*, half-an-ounce every hour; seldom has stimulation been carried beyond this for more than a few hours at a time. The mode of treatment to which the cases included in these statistics, at least those admitted during the last three years, were subjected, may be characterized as mainly dietetic in the early periods of life, and freely stimulant in the more advanced periods. The success which attended it may be, and very likely is due in part to some of the agencies noted in an earlier part of this paper; but the fact remains that, with this treatment, there is a death-rate considerably below that of most other hospitals. A good deal has of late been said about the harm likely to

* *Dr. Maclagan here* introduces a Table which shows the amount of stimulant given in his cases.

accrue from over-stimulation in typhus. My own very strong impression is, that whilst a deal of harm may result to young people from the indiscriminate adoption of this method of treatment in all cases, a great deal more injury would result to elderly patients from its abandonment, and the substitution of the non-stimulant plan. The only truly rational plan is to do with typhus as with every other disease,—treat each case on its own merits, but have constantly before us the lessons taught by experience and observation, that age increases immensely the fatality of the disease, and that in elderly persons death is threatened by failure of the heart's action to a much greater extent than in young people, in whom the head-symptoms must be looked to as the chief source of danger.—*Edinburgh Medical Journal, August*, 1867, *p.* 155.

4.—ON THE USE OF OPIATES IN CONTINUED FEVER.

By Dr. N. G. Merger, formerly House-Surgeon to the Clayton Hospital and Wakefield Dispensary.

[There occasionally occur cases of fever in which the administration of an opiate becomes absolutely necessary, in which the narcotic has a remarkable therapeutic power, and gives to the sinking energies of life their last chance of rallying. Hence it becomes necessary to define the cases in which its use is admissible. With this view the following case is related. The patient was a pensioner, 47 years of age, one of many in a large Irish lodging house, which was crowded and unhealthy to the last degree.]

His case was from the first attended with the gravest typhoid phenomena. I had attended him for nearly two weeks, and the symptoms were growing daily more alarming, although an exception must be made in favour of an attack of bronchitis, which had been severe from the beginning, but had undergone partial improvement. The delirium had not abated for three days, and its active character frequently necessitated restraint to keep the patient in bed. The pulse-rate had gradually risen in frequency, and had reached 144 per minute. Great wakefulness had existed from the first, but in the middle of the second week had become so extreme, that out of twenty-four hours scarcely a whole one of sleep was obtained as the aggregate of many short-lived snatches. The bowels had been alternately loose and constipated, the evacuations invariably presenting the peasoup characters. The following were the signs and symptoms present on the thirteenth day of the fever, on which, as determined by treatment, the turning point seemed to take place:—The patient was almost completely deaf, and, when asked in a loud tone to show his tongue, seemed to comprehend nothing. A view of it was, however, obtained by mechanically separating the jaws, and it was seen to be black-brown in colour and perfectly parched, while the teeth were covered with sordes. The respiration was hurried from the bronchitis, although this did not exist to the same extent as a few days before. The abdomen was literally blown out with tympanitis; the patient was continually fumbling with the bedclothes, and muttering inarticulate sentences. The evacuations were and had been involuntary for three days. During two entire days and nights, at the least, his wife declared that the wakefulness had been without a moment's intermission. All the symptoms present in general, although this last and the existing character of the pulse in particular, indicated and warranted the experiment of an opiate. That character of pulse is hardly to be described; once felt it is perceived to be remarkable, and is not forgotten. In this case it was not possible to estimate the number of beats per minute, on account of its extreme rapidity, but chiefly—thus completely conforming to that description of pulse where opium is highly serviceable—because of its weak, fluttering, confused, and indistinct character. The beats appeared to run into one another, without that appreciable interval present in all other variations of the pulse, whether in health or disease. Such are the circumstances under which we are culpable in withholding an opiate, as the last chance of saving our patient's life. A very mild one in this case was found sufficient to compass the

object in view. A pill containing half a grain of opium was to be given at bedtime, and if this seemed to have the effect of slightly lessening the delirium and restlessness, a pill of like strength was to be administered in two hours. If a tendency to sleep happily supervened, the second pill was to be omitted.

From the very hopeless nature of the symptoms present on the afternoon when this treatment was adopted, the prognosis I gave was to the effect that the patient would certainly die; and at my visit on the following day I did not dare to expect an improvement in the symptoms, although in cases less severe I was well acquainted with the beneficial influence of opium. My disappointment, however, was as great as it was agreeable. The patient's wife informed me that after giving him one pill only he showed a strong disposition to sleep in less than half an hour, and that accordingly the other pill had not been administered. Soon after he fell into a profound slumber, and at my visit he was calmly asleep. At intervals he was disturbed, in accordance with my instructions, for the purpose of giving nourishment. Under the influence of the half grain of opium, the pulse, too, had undergone a most notable change, having fallen to 132 per minute, and its beats having grown perfectly regular. Although I still gave a cautious prognosis, the symptoms had become, in fact, a little hopeful, while the day before they were desperate.

The subsequent history, which might be termed one of uninterrupted improvement, may be summed up in a few words. Ever since the eventful night when the soporific was given, the patient enjoyed nightly a few hours of refreshing sleep. Delirium thereafter formed a very slight feature in the case, and his deafness went also gradually away. What tended chiefly to retard recovery was an immense sloughing bedsore, which burrowed amid the muscles of the nates, and exposed the crest of the ilium. Under suitable applications, however, this was towards the close of the fourth week converted into a healing, granulating ulcer. Ammonia and bark were early and freely given after the active symptoms of fever had abated, and our Dispensary contributed towards enriching his by no means over-liberal diet, for he took daily after the third week three ounces of cod-liver oil with a keen relish, and to his great benefit. Soon after I lost sight of him, having been told at my visit that he had gone some miles into the country, where, no doubt, he finds the change of air, lodging, and surroundings of much advantage to his weakly condition.

In conclusion, I have only to observe that this case, like many others of the same fever, refused to conform to that rule of limitation as to time which systematic authors speak of in this disease, while there could be no doubt whatever of its nature. Small opiates only were hazarded on account of the degree of bronchitis present; but even these served to illustrate the salutary and beneficent agency of the narcotic, controlling irritability, adding force to the action of the failing heart, and well establishing the medicine's claim to be called, as it has been by Dr. Christison, the "best drug of all."—*Medical Times and Gazette, Sept.* 28, 1867, *p.* 346.

5.—CEREBRO-SPINAL FEVER: ITS TREATMENT BY MEANS OF ICE.

By Dr. John Chapman, Physician to the Farringdon Dispensary.

Before we can act scientifically for the removal of the proximate cause of the group of symptoms, consisting of trismus, retraction of the head, opisthotonos, pleurothotonos, rigidity of the recti muscles, emprosthotonos, contracclassifying the symptoms of cerebro-spinal fever, I have assumed it to be tion of the muscles of the extremities, convulsions, and subsultus tendinum, we must first assure ourselves, of course, what the precise nature of that cause is. *In hyperæmia, proceeding* possibly to the extent of inflammation, of the spinal

cord, but I am aware of the existence of a doctrine concerning the proximate cause of muscular contraction which is wholly inconsistent with that generally held on this subject, as well as with the practical application of it which I am now about to advocate. I refer, of course, to the peculiar views concerning the proximate cause of muscular contraction, and of pain, which have been elaborately and ingeniously expounded by Dr. Bland Radcliffe. In a paper entitled—"The Treatment of Epilepsy: Principles and Practice," read to the Medical Society of London, and afterwards published in this Journal (March 27th, April 4th), I briefly explained Dr. Radcliffe's views, and adduced what appears to me conclusive reasons for believing them to be confuted by a large number of physiological, pathological, and therapeutical facts. On this occasion, therefore, while referring students interested in the subject to that paper, I shall assume, without further discussion, that all excessive muscular contraction is due to excessive energy in those nervous centres which preside over the muscular system. Now, this being so, it is obvious that the only rational way of treating lock-jaw, retraction of the head, and other tetanoid states, as well as convulsions and subsultus tendinum, is by lessening the energy of the nervous centres producing those phenomena. Can we do this? I affirm that we can, and, notwithstanding the difficulty of many thoughtful men in believing it possible, by applying ice along the spine, to influence the circulation of the spinal cord at all, and the fact that, up to the present time, this method has, with a few exceptions, been wholly disregarded or unknown, I believe that the time is not far distant when it will be counted on by medical practitioners as one of the most powerful remedial agents at their command for subduing hyperæmia or inflammation of the spinal cord.

Some physicians who are disposed to recognise the reality of the results which I have obtained by the application of heat and cold along the spine, and who cannot believe those results are due to the direct action of these agents on the spinal and sympathetic nervous centres, are inclined vaguely to regard them as phenomena of reflex action. (One of the most eminent physicians recently asked me if I had tried whether applying ice over the stomach would not produce the same effect as that produced by it when applied along the spine!) I am satisfied, however, that this view is untenable and quite inconsistent with many ascertained facts. But I cannot here enter on a discussion of this subject, and must content myself with observing that whatever mode of accounting for the results in question may be most scientific, the existence of them is indubitable, and, as in a practical point of view, the question of their reality is the all-important one, I will, before proceeding further, adduce evidence that various morbid conditions of the muscular system analogous to those commonly associated with cerebro-spinal fever, may be most effectually abolished by the application of ice along the spine.

I have already cited the evidence of Dr. Todd, and I may observe in passing that he was one of the few physicians who never doubted that the application of ice along the spine acts directly on the spinal cord. In the Medical Times and Gazette, March 12th, 1864, Dr. Edmunds reports a case of Emprosthotonos (?) cured by the use of ice to the spine. The facts of the case are briefly summarised as follows:—An intelligent married woman, aged 28, mother of two children, and who was "altogether free from flightiness or hysteria," went to bed, January 21st, quite well, but on rising next morning at 7:30, found her fingers were slightly benumbed; they shortly afterwards "turned in." The "turning in" quickly extended to the hands, rapidly increased, and caused a cramp-like pain in the arms. Her legs became affected in a similar way but less severely. The morbid muscular contractions steadily increased during the day. At ten p. m., "her fore-arms were hard and rigid, and the wrists were so fully flexed that her hands were forced open," and "the backs of her wrists were fixed close to her mouth. . . . Her neck became turgid and the respiration somewhat constrained. . . . Her spine was bent forward, and her knees were bent and drawn upwards. . . . She could not stand, and her legs were about as much cramped as in a bad

case of cholera. . . . There was an expression of great anxiety and pain upon the face, and the lips were sufficiently retracted to show the teeth. . . . She could swallow by making a great effort to ignore the tearing cramp-like pain which occupied her, and which forced from her a continual moaning scream." Between eleven and twelve a. m., Dr. Griffith ordered her some aperient medicine; at one p. m., Dr. Webber, who saw her in consultation, prescribed "sulphuric æther and antispasmodics at once, and a dose of morphia to be taken a few hours later if the pain should not abate." She grew worse; the morphia was given, and, afterwards, "all her symptoms were aggravated." At ten p. m., Dr. Edmunds was called in. He ordered the administration of a purge consisting of croton-oil and calomel, an enema of turpentine and castor-oil, and the application of ice along the whole length of the spine. Cold was thus applied continuously during "seven hours," when all her severe symptoms had disappeared. The screaming and moans were at once mitigated by the ice, and in about half-an-hour ceased entirely. In two hours her feet became flexible. About eight a. m. on the 23rd the spasms had so far subsided that she could open her hands, but the numbness remained. On the morning of the 24th the numbness entirely disappeared, and she had no return of cramps or spasms. . . . The patient herself ascribes the relief of her dreadful pain and cramps entirely to the use of the ice," says Dr. Edmunds, who adds,—"and although it is difficult to distinguish between the mere *post hoc* and the *propter hoc*, yet I cannot but believe, if ever I saw a life saved by a therapeutical application, that this patient's life was saved by the use of ice in this way to the spine."

The Australian Medical Journal for March, 1866, contains a report of a case of tetanus treated successfully by the application of ice along the spine. In this case, chloroform and atropine, three doses of each, were given; but no one reading the report will fail to be convinced that it was the ice along the spine which saved the patient. "In two hours after its application," says the report, "the spasms had considerably and decidedly diminished. . . . Our entering the room caused such an excitement in the patient as to bring on all the symptoms that had been in abeyance for four hours and a-half. The application of the ice was renewed, and in twenty minutes the spasms had disappeared."

On the 24th of February, 1866, I received from Dr. Roberts, of Northallerton, Yorkshire, the following letter:—"As an old fellow-student, I have watched with more than ordinary interest your treatment of diseases by variations in temperature. Have you ever tried it in tetanus? I have a case of traumatic tetanus which has remained almost stationary for a fortnight, and I have a notion that one of your ice-bags applied to the spine would be of service. If you have any experience, I should be glad to avail myself of it, or if you have any theory on the subject I should be glad to test it. I have given the patient belladonna and morphia, but to no purpose. The tetanus has supervened on a large scalp-wound over the occiput, exposing a large-ISH surface of the bone." I sent immediately to Dr. Roberts a spinal ice-bag of suitable length, also a "lumbar ice-bag," and recommended him to apply ice along the whole spine, and at the same time, by means of the lumbar-bag across the occiput—from ear to ear. I begged him not to let the presence of the wound deter him from placing the lumbar-bag as advised, and to keep both bags well supplied with ice, and applied continuously until the disease should be subdued. In reply to my inquiry as to the results of the treatment, Dr. Roberts wrote me (May 20, 1866)—"I am glad to say my case of tetanus got quite well. I have not had time to put my notes of the case into shape and send them to the Lancet, but I hope to be able to do so very shortly." I regret to say this "hope" still remains unfulfilled.

In the Medical Times and Gazette of June 16, 1866, Dr. John W. Ogle reported a case of "tetanoid convulsions overcome for a time by application of ice to the spine." A boy two years old was brought to St. George's Hospital, at eleven a. m., February 16, in a state of general convulsions. Dr. Ogle "ordered an enema with castor-oil and turpentine to be given at once, *and the gums to be examined* for the purpose of seeing if they required

lancing. It was found impossible, however, to open the mouth. He then ordered an ice-bag to be applied, and kept in close contact with the back, the entire length of the spine. . . . Gradually the convulsions abated, and the child was considered by the apothecary well enough to be allowed to go home in the evening." On the following day, the child being "evidently not so well," was again brought to the hospital, but was placed under the care of Dr. Pitman, who prescribed grey powder, antimony and salines, and did *not* use ice: convulsions recurred, and proved fatal, February 19. Dr. Ogle, commenting on this case, observed:—"The substantial relief from the ice application was not a little interesting, and speculation upon the probability of a greater and more permanent benefit from a longer application of the ice could not be resisted."

Under the head of "Gleanings from Allopathic Literature," The British Journal of Homœopathy, for April, 1866, says:—"Falconer related the case of a boy, aged 11, attacked with severe tetanus, who recovered under the constant application of ice to the spine for thirty-two days. Howard records a case of opisthotonos in a man, aged 25, cured by ice to the spine." I have not ascertained what "Allopathic" journals these cases were gleaned from.

In the Medical Press and Circular of March 27th last, Mr. Hargrave reports a case of tetanus, in which great relief was given by ice applied to the spine, and in which a cure would probably have been effected had a properly made spine-bag been used. The ice was applied in an ox-gullet, and during its application the patient was singularly benefited. Unfortunately, the gullet burst in the night, and ice was not afterwards applied, Mr. Hargrave being evidently ignorant that india-rubber spinal ice-bags, made under my direction, are readily procurable in Dublin. During the application of the ice, "the muscles of the lower jaw were so relaxed, that the patient could open her mouth and take some nourishment and medicine, from which," says Mr. Hargrave, "I augured favourably."

In the number of this journal for May the 8th of this year, I published a case of what I called "Tetanoid Paralysis of the Four Extremities," accompanied with swelling, ecchymoses on the feet, and extremely heightened sensibility, which was completely cured by the continuous use of spinal ice-bags.

On the 12th ult., at the Farringdon Dispensary, I prescribed for a patient, Margareth Macgrath, aged 14, who was suffering from scarlet fever, with inflammation of the throat. The fever subsided, but on the 19th the patient came to me with the head violently twisted round, so that the face looked over the left shoulder; the cervical muscles were tense, extremely tender and sore, the tenderness extending to the shoulders, and were seemingly so swollen, that I thought, at first sight, the appearance was due to glandular enlargement. Being satisfied that the case was simply one of persistent spasm—a sequela of the influence of the fever on the spinal-cord, I ordered ice to be applied along the cervical and upper part of the dorsal spine. Within thirty minutes of the beginning of the application, which was continued two hours, the patient could move her head in any direction, and the next day all tenderness was gone.

The above statement of facts justifies, I think, my conviction that in the present state of medical knowledge our greatest chance of curing tetanus and tetanoid affections consists in the proper application of ice. I am aware that a number of cases were published in the Lancet a short time ago, which seemed to prove that ice to the spine is of no avail against this deadly malady. But the writer gives no particulars of his method of applying the ice, and, in the absence of these, I can attach but little importance to his conclusions. It will be observed that in the case cited above, under the care of Dr. Roberts, I directed that while ice was applied along the spine, it should also be applied in a separate bag across the occiput, notwithstanding even that it had to be placed directly across a wound in which the bone was exposed. I consider the adoption of this plan in all cases of tetanus of the very utmost importance, and think it probable that in those fatal cases in which ice may have been applied continuously along

the spine, it was not applied across the occiput. When ice is applied along the spine, and especially over what is called the cilio-spinal region, the vaso-motor nerve centres controlling the cerebral arteries are subject to its sedative influence, and, hence, those arteries become dilated, and are thus enabled to supply the encephalon with more blood than before. In this manner, as I conceive, the medulla oblongata is maintained in a hyperæmic state, at the same time that the spinal cord is subject to the sedative influence of cold. Such being the case, it is obvious that that influence may be partly neutralized by the preternatural energy of the medulla-oblongata propagated downwards; while, of course, the nerves arising from it, including the motor divisions of the fifth and seventh, and the muscles they supply, if not kept as active as they were before ice was applied at all, are, at any rate, scarcely likely to be rendered quiescent. Much, however, will depend upon the way in which the ice is applied. If, for example, it be restricted to the lower cervical and upper dorsal vertebræ (the cilio-spinal region), there will probably be a maximum afflux of blood to the head; but if the ice be continued upwards over each cervical vertebra to the base of the skull, the sedative influence exerted on the spinal cord is likely to extend in some degree to the medulla-oblongata. The considerations here advanced afford an explanation, which is perhaps the true one, why in certain cases of tetanus ice applied along the spine has been found useless, and, at the same time, indicate that, as a general rule, when ice is applied along the spine in such cases it should be carefully kept in contact with the uppermost cervical vertebræ, and that it is always indispensably necessary to apply it in a separate bag across the occiput *simultaneously*.

The practical application of the foregoing facts and observations to the treatment of cerebro-spinal fever scarcely needs indicating: trismus and retraction of the cervical muscles, when occurring in the course of that disease, should be treated in the manner just described, care being taken by incessant watchfulness that air (a bad conductor of heat) is not allowed to accumulate in the upper end of a spine-bag, or in the side next the head of the lumbar-bag placed across the occiput, and that both bags be kept well supplied with ice. If they are allowed to remain on the patient when they cease to be cold they will do positive harm; for, as india-rubber, like air, is a bad conductor of heat when it is applied to the skin, it prevents, of course, that passage of heat from it which would otherwise take place. This method of treating the tetanoid symptoms frequently associated with cerebro-spinal fever is fortunately calculated to subdue, at the same time, that morbid state of the cerebro-spinal axis which constitutes, so far as our positive knowledge extends, the very essence of the disease.

In those cases in which convulsions, partial or general, supervene the treatment should be the same; for though convulsive paroxysms apart from cerebro-spinal fever may be effectually treated by ice along the spine only, the action of the ice is always more complete and certain if, *when used during the fits*, it be applied across the occiput also; and even though tetanus or tonic spasms of the cervical muscles may not occur in certain cases of the disease in question, there will probably exist strong reasons against so using ice along the spine as to facilitate the flow of blood to the brain in fuller streams than before without taking care to keep it under the sedative influence of cold at the same time. I need not enlarge here upon the efficacy of the spinal ice-bag in the treatment of convulsions, as I adduced ample evidence of its power in this respect in the paper on Epilepsy, already mentioned. I may, however, appeal to the testimony of Dr. Routh and Dr. Rogers, that in cases of both infantile and puerperal convulsions, they have found that the spinal ice-bag arrests them, and may add that I am in possession of a large number of facts proving, not only that these maladies, as well as epilepsy, are amenable to this method of using cold, but that laryngismus stridulus, ordinary night-cramps, and the cramps of cholera, and those occurring in some cases of sea-sickness, may be completely subdued in the same way. I conclude, therefore, that all spasmodic, convulsive, or epileptoid affections incidental to cerebro-spinal fever may be most surely controlled by precisely the same

treatment as is necessary, not only for the tetanoid group of symptoms, but for those to be now immediately referred to, and which I regard as no less significant of morbidly increased activity of the spinal cord.

In my pamphlet on Sea-sickness, and in a paper on the Sickness of Pregnancy—read to the Medical Society of London, I have developed my views concerning the pathology of nausea and vomiting, and in my work on Diarrhœa and Cholera, the pathology of those diseases, and the mode of production of an abundance of pale urine of low specific gravity, are also fully discussed, and, as experience emboldens me to believe, correctly explained. I shall, therefore, content myself here with the statement of my conviction that I have proved that nausea, vomiting, diarrhœa, and preternatural abundance of pale urine *can only occur* by virtue of a preternaturally vigorous action of the spino-ganglionic nervous centres presiding over the organs implicated, and that that excessive activity can be restrained most scientifically and most satisfactorily without the evil influence of narcotics, stimulants, or other drugs, by the use of the spinal ice-bag.

In the group of symptoms of inflammation of the spinal cord and its membranes, I have classed, besides those just referred to, *erotic desires*, *priapism*, *subsultus tendinum*, *swelling of the tongue*, *petechiæ*, and *vibices*. It will probably be admitted at once, by each of my readers, that the first three of these disorders are expressions of disorder of the spinal cord, and I shall, therefore, only observe respecting them, that I am assured by experience that they may be treated successfully by the spinal ice-bag, and that, if in cases when subsultus tendinum is present, the physician will apply the bag, and watch the results, he will generally be surprised and gratified, not only by the rapidity with which the affected muscles will be soothed into quiescence, but by the generally refreshing and vivifying influence which the ice thus applied exerts.—*Medical Press and Circular*, *July* 31 1867, *p.* 97.

6.—SULPHUROUS ACID FUMIGATION.

By Dr. James Dewar.

The following cases, as illustrating the advantages of this mode of treatment, will no doubt prove so interesting as to induce others to submit the system to the test of intelligent experiment, and so judge for themselves.

The Rev. —— for fifteen years had suffered from disease of his throat, rendering preaching laborious. General health "below par;" obliged to change his "underclothing" between services, so uncomfortable was he from perspiration; had tried everything, without good result; began the sulphurous acid inhalation, (besides "fumigating" twice or thrice a day); improvement immediately commenced, and a cure was speedily effected. General health has since greatly mended, and finds his duties now no burden.

Rev. —— called for me to inquire about sulphurous treatment of throats. He had suffered from his ailment for many years, which made him quite pluckless, and afraid to undertake duty, from a feeling of nervousness. I examined his throat, and, without any remark, applied the spray. He instantaneously felt relief, and said that he could command his voice in a way he had not done for long. He asked me what I had used, and was surprised to learn that it was sulphurous acid, as he had believed that the remedy of which he was in search was sulphur *pur et simple*. The last accounts I had from him were satisfactory.

Rev. —— had been a victim to this malady for twenty years; found himself very ill able to discharge his duties, from want of vigour and unconquerable nervousness, which especially laid hold of him when he had occasion to pronounce words containing certain consonants. Was in a state of agitation at the idea of having the spray applied, but, nevertheless, he at once clapped his hands, and exclaimed "Delightful!" At the end of a month he returned to London, and is now in perfect health, and fit for any duty. He

conjoined fumigation with the use of the spray. It is interesting to note that each of these gentlemen used the same figure to illustrate the feeling of relief which they had obtained, which was, "that something which for years had been loose in the windpipe had suddenly been 'braced up,'" which, doubtless, was no inaccurate explanation of the real change which was effected upon the vocal chords.

Miss —— came home from London last season, changed somewhat in her appearance, and showing symptoms which naturally made her parents anxious. She had lost flesh, had a tickling cough, and a tendency to flush. Upon examination, the chest proved all right, but the condition of the mucous membrane of the pharynx and larynx was evidently sufficient to account for the cough at least. The spray was applied a few times, when the disease was quickly eradicated, and a return to her wonted health has been the satisfactory issue.

Mrs. ——. This lady had for several years been obliged to winter in a southern climate, on account of liability to catch cold, and general delicacy of health. I saw her early last summer, and, from the satisfactory state of her chest, was led to believe that she was likely to be benefited by "sulphurous acid medication." Both processes were adopted with immediate advantage. She rapidly regained her health, and when I last heard of her, she was taking on flesh, and felt strong and well. She had persistently put in practice the plan recommended, and was to continue it during the present winter.

Miss ——. Last autumn I was asked to see a young lady from Edinburgh, whose general condition had recently given her friends much anxiety, and when I visited her it seemed to me that her appearance, coupled with what I was told were her symptoms, fully justified the apprehensions entertained concerning her. The family history being somewhat ambiguous, I was fully prepared to find something wrong in the chest, but in this I was very agreeably disappointed. The appearance of the fauces, however, was such as to account for everything, as I thought, and accordingly the spray was applied with the usual good effects, and under the influence of this and fumigation the patient has progressed marvellously. I have not seen her again, but the last report said that she was so well as to think confinement to the house at night unnecessary and very irksome.

A very interesting case of cure of an obstinate laryngeal affection of many years' standing has lately passed under my observation. Mrs. —— had long suffered from this disease, which had baffled all attempts at remedy, although she had consulted the most eminent medical men in different parts of Europe. Some weeks ago I had occasion to see her, and took the opportunity of recommending and applying the spray. I left the apparatus with her, enjoining its use four times a day, and she writes me to say that her throat is quite well, and that she enjoys a sense of returning vigour which she had not hoped to attain.

Last week a young lady, who used to sing very nicely, called for me to ask whether I thought the remedy in question would benefit her, as for months she had been unable to sing as was her wont, and besides had suffered from uneasiness in the chest, which naturally gave her some concern. Her case appearing to be a suitable one, the spray was applied at short intervals during a *séance* of five minutes, and she arranged to come again each morning to have the process repeated a few times. When I next saw her, she joyously said that she was quite relieved of every feeling of discomfort, and "that she had sung as well last night as ever she had done!"

Chronic Bronchitis and Emphysema.—In this class of diseases sulphurous acid medication is attended by speedy and signal benefit. In whatever stage I find my patient, I at once proceed to apply the spray, which by loosening the expectoration, facilitates its expulsion, so that the chest can be wonderfully cleared of it in a few minutes. Cough is no doubt excited, but this does not deter patients from its use, finding as they do, that they are more than repaid for the temporary inconvenience.

— F., now a post-runner, formerly a miner, has been asthmatic for many *years. In January last* his condition became much aggravated, and, after

ten weeks' struggle, he was obliged to abandon his duty. I found him terribly oppressed, his attempts at expectoration being distressing because unsuccessful. The spray enabled him at the outset to begin the process of clearance; his improvement under the conjoined remedies was progressive, and in the course of five days there was scarcely a râle to be heard in his chest. The spray was applied twice a day, and his room was filled with "fumes" every four or six hours. He has long since gone back to his work, and he told me yesterday (April 10, 1867) that he is in better "wind" than he has been for ten years.

J. P. had been ill for weeks before I saw him, his breathing difficult, and his efforts at expectoration almost fruitless. I persevered, pumping in the spray at intervals for some twenty minutes, until his chest was much relieved and almost noiseless. Next day this was repeated, which completed the cure.

I could easily multiply the number of cases such as those just detailed, but this is quite unnecessary, as like favourable advantages have many times accrued in individuals *otherwise* abandoned to hopeless suffering. That this agent exercises a *really curative* influence upon the pulmonary mucous membrane, the rapid improvement in the health, and the comparative immunity from other attacks of bronchitis in those so operated upon, sufficiently testify, while the simplicity and perfect safety with which the process can be employed render it peculiarly suitable for the desired purpose.—*Medical Times and Gazette, May* 25, 1867, *p.* 548.

7.—REMARKS ON THE ACETIC ACID CURE FOR CANCER.

By Dr M'Gregor, Medical Superintendent, Barnhill Hospital.

[The first indication of the presence of cancer in the case which forms the subject of the following article, was an obscure hardened swelling deep in the substance of the left gland. There was an occasional lancinating pain. When first seen by Dr. M'Gregor there was no retraction of the nipple, no discoloration, no history, hereditary or otherwise, to lead to the diagnosis of malignant disease. In a few weeks after the case was first seen by Dr. M'Gregor the swelling had increased considerably in size, had become harder, and the lancinating pain more frequent and severe. There being then no doubt as to the malignant nature of the case, it was determined to give Dr. Broadbent's method of cure a fair trial.]

I began the injections by means of a Wood's syringe, using a dram of the acid, of the strength one part of strong acid to three of water, or one in four. This was repeated several times, every second day, the dram being generally divided equally into two portions and injected in two distinct parts of the tumour. After a week's trial in this manner, I found that the disease, if not aggravated by the remedy, was certainly not improved by it, for now it seemed to be rapidly spreading, the tumour becoming larger, and the pain increasing in severity. Under these circumstances, I increased the quantity of injection to two drams, and also the strength of it to one of acid to two of water, or one in three, instead of one in four as before. So hard and dense was the tumour (scirrhus) that I found great difficulty in injecting so much as two drams into it by Wood's syringe. I therefore employed a small trocar and canula, generally giving the patient, who was a little nervous, chloroform during the operation, the acid being injected as before into different parts of the tumour on each occasion. In this way I persevered for a fortnight more, but only to meet with disappointment. The disease spread rapidly, soon involving the whole breast, and ulcerating through the skin at one or two points of it, so as to leave me much too little skin to cover the wound, when, after the failure of the injection, I felt compelled to remove the tumour. This I did (with the able co-operation of my assistant, Dr. Bain,) along with a couple of swollen and suspicious glands in the axilla. She made a good recovery, the wound healing up satisfactorily, notwithstanding the lack of skin (*owing to* previus ulceration) to cover it, with the

exception of a small point which, instead of closing up like the rest, remained in an angry and irritable condition. I tried every means at my command to make this heal. On the contrary, it soon gave signs of spreading and becoming larger. It was soon apparent that the cicatrix was opening up rather than healing firmly, as it should have done, and that the dread disease was not going to yield to all the measures employed for its extirpation.

To this small ulcerated surface clearly relapsing into a diseased state, I now began to apply dressing of lint soaked in the *acetic acid*, of the strength of one in three as before, thinking that if of any use at all in battling with this disease, a better opportunity of noticing its effects could not present itself. For here was an ulcerated surface of the size of a penny piece, evidently relapsing into a diseased state, notwithstanding the removal of the entire breast; and if the acid has the power of effecting that metamorphosis of tissue attributed to it by injection into a cancerous tumour, it ought to influence a raw surface like this affected by the same disease, when directly applied to it, in a similar manner. Yet, notwithstanding its constant application in the manner indicated, the disease continued to spread till the whole cicatrix was involved in it. Unfortunately the same disease now began to show symptoms of having attacked the liver and intestines, and if I had any hope before of saving the patient it now disappeared. At this date, I am sorry to say, she is rapidly sinking.

On examining the excised breast, by making incisions into various parts of it, it was noticed that at the points where the acid had been injected, the cellular tissue had a dark, broken-down, sloughy appearance for about a quarter of an inch on either side of the points of introduction of the acid. Under the misroscope, the portion of the breast unaffected by the acid presented the usual indications of scirrhus. Where affected by the acid, there was nothing made out but a mass of disintegrated, broken-down, cellular tissues that presented nothing remarkable.

During the application of the acid on the lint dressing to the ulcerating surface, there was noticed on each removal of it, a thin layer of a dirty gray, cheesey-looking matter underneath the dressing. This could be scraped off by the nail in small soft scales or rolls, leaving bare patches of the ulcer exposed, and was certainly due to the action of the acid, for where it was not applied, this cheesey layer did not exist. It did not seem to be due either to its action on the discharge, which an ulcerated surface will always more or less produce, but rather to the action of the acid on the tissue it came in contact with. This made me hope at first that the diseased tissues might be broken up and got rid of in this way, so I continued assiduously to apply the acid to the affected parts, clearing away this cheesey matter and dressing the sore afresh three times a-day, so that the acid might, on each application, have a new surface to act upon. In spite of all this my efforts proved unavailing and the disease went on its rapid course, seemingly unchecked by this treatment.

The apparent extent of the effect of the injections, as noticed in the breast after its removal, seemed to me so trifling in comparison to the mass it had to contend with, that I could not help doubting its efficacy when the disease had assumed anything like considerable proportions; even the size of a walnut, unless, indeed, it progressed more slowly than malignant disease usually does. The injection of the acid will always have a tendency, as in this case, to produce irritation and pain, if used of such strength as to affect the tissues, and this must *hasten* the progress of the disease when it fails to check it. I am confident that had I removed the breast in this case a few weeks sooner, instead of putting off time trusting to the injection, there would have been a much greater likelihood of the breast healing thoroughly after the operation, and keeping well, though the disease having attacked internal organs, the result to the patient would ultimately have been the same.—*Glasgow Med. Journal, June* 1867, *p.* 52.

8.—ON THE USE OF ALCOHOL IN DISEASE.

By Dr. Samuel Wilks, Physician to and Lecturer on Medicine at Guy's Hospital.

I am not going to enter upon the much vexed question as to the necessity for the daily use of wine or spirit; but I will say that the usual test for the advantages of its use is one which I conceive to be in most cases utterly valueless. Alcohol, remember, although an excitant, is a sedative to the nervous system—is, in fact, an anæsthetic. A drunken man may be injured in such a way as to have all his teeth knocked out in a brawl, yet apparently not perceive the injury, and be utterly unconscious of the occurrence when he has returned to soberness. The argument, therefore, that a man feels better after his glass or two of grog would be equally applicable to the case of the Turk who feels better for his opium. His feeling better simply means that he has got rid of his unpleasant sensations, whether these be moral or physical; he "drowns his troubles in the bowl." If a man engaged in the practice of his profession, mercantile business, or even pleasure, such as boating, cricketing, or shooting, intends to assert that he can pursue these respective objects with more success after he has taken a stimulant, then he may have an argument in favour of its use. If, however, it be admitted that during the active pursuits of the day a stimulation to the nervous system is injurious, but that after the fatigues are over the body must be recruited, and that a proportion of alcohol is beneficial, I have nothing to say against it, should experience speak in its favour. I repeat, however, that in the majority of instances where a man's reason for taking his wine or spirits is of no better kind than that he feels better for it, the reason is utterly valueless; indeed, it may generally be assumed that whilst his feelings are benumbed his organisation is being injured. The argument is no better in favour of the use of wine or other stimulant in disease—as, for example, that it must do good, since the patient craves for it. The question of the advantages attending the daily use of beer, wine, or spirits, although a difficult one to solve, is one which you cannot evade considering, since the health and welfare of families may depend upon your judicious decision. You may recommend wine with advantage to members of certain families having peculiar temperaments, whilst should you advise it for others you may unwittingly be sowing the seeds of ruin of mind, body, and estate.

The subject of the different temperaments of your patients and their mode of life is one which is well worth your study; but the matter I wish now to strongly enforce upon you is that you are as thoroughly to consider the propriety of the administration of alcohol as you would any drug in the Pharmacopœia. Endeavour, if you can, to erase from your minds that it is a proven fact that alcohol is a tonic or a necessary part of every one's beverage. This is assumed by a large mass of people; and the meaning of the question which your patient puts to you when he says "What shall I drink?" is not "Shall I take a stimulant or leave it alone?" but "Shall I drink beer, wine, or spirits?" He often confesses that he is in a great difficulty; he finds none of them agree with him; but that he must take "something" appears as necessary as eating his daily bread; the alternative never having formed part of his calculation. I say it is assumed that a strength-giving property lies in these drinks—that just in proportion to a man's feeling of weakness so will he require one of them; in ordinary health he may only want his beer; but if ill, his wine; and if very ill, his spirits. Now this popular opinion is shared in, I am sorry to say, by many in the profession; if the patient is weak he wants "support," this term carrying too frequently with it the necessary idea of wine or spirits. I should be sorry to say that the doctor panders to the public taste, since he is too often already in accord with it; but the consequence of such agreement between patient and medical man resolves itself into this, that an extra stimulant is prescribed. You might ask to what complaints do I refer when I speak of this too common advice; but I need only repeat the word "patient," for it matters little what

is the nature of the disease, since the reasons for the treatment are applicable to all complaints, and are founded on this simple proposition: all persons who are ill are weak; they have lost strength; they require it to be restored; alcohol is a supporter and a tonic, therefore alcohol is a remedy for all diseases. This is no parody, for I have heard the argument set forth in some such words; and practically it is adopted by many, for I constantly hear medical men say they give brandy to all their patients, for they always find them "low." Brandy indeed becomes with some as much a universal remedy as revalenta, chlorodyne, Morison's pills, or any other quack medicine. Moreover, it is a medicine of which the patients approve, assuming as they also do its supporting and strength-giving powers. You therefore cannot do better, if you fear no compunctions in converting your profession into a mere trade, to say to all your patients, after feeling their pulse, that they are very low—that you are sure they do not take enough; and order them several glasses of wine daily. Should they be exceedingly ill with some desperate organic complaint, then you must turn your remarks to the friends, and speak of the necessity of supporting the patient by giving him as much brandy as can be poured down his throat. By this method you are sure to give "satisfaction": for should the patient die without such treatment, you may have the credit of letting him slip "through your fingers"; whilst, if he die with it, you have done your best. If you kill a dozen patients with brandy, you need have no fear—"you have done your best." This, I say, would be a very comfortable and lucrative mode of practice.

It may very fairly be asked, If alcohol be so very potent a remedy that it can supersede all drugs in so many several cases of disease, is not this a reason why brandy treatment should be adopted, with some consideration? The want of caution in its use is owing, I have no doubt, to its entering so frequently into the daily diet; and thus alcohol is not reckoned amongst the same class of agents as that of medicines. On the bed-cards in this hospital there is one column for the medicines, and another for the diet. Before filling up the one, we discuss the benefits of giving our patients a few drops of henbane or ether; and in the other column we often write down any number of ounces of brandy with very little thought of its effect. If alcohol were transferred to the medicine side of the card, we should be more likely to discuss its value in any given case in the same manner as we do the various drugs in the Pharmacopœia.

It would require a whole course of lectures to dwell upon the beneficial or baneful effects of alcohol in all forms of disease; and, therefore, I will simply state, as a result of my own experience, that like other drugs, it may be beneficial, useless, or harmful. I may remind you of what you yourselves have witnessed—that fevers will do well without this remedy. So wedded, however, are some to the idea of the absolute necessity of stimulants, that they have expressed almost incredulity when they have heard it stated that fevers will terminate favourably without them. Of course stimulants are often needed; but young persons with typhus and typhoid do far better, I believe, without them. That they make good recoveries on simple milk diet is a fact which my hospital cases prove, and which no arguments can gainsay; and, on the other hand, I have seen a marked improvement take place in some cases where a stimulus has been left off. It is also a fact that in bronchitis I have repeatedly seen improvement after stimulants have been omitted; and, as regards heart-disease, I am convinced that the amount of mischief done by stimulants is immense. In the case of fevers and bronchitis, the weak pulse is often but an indication of extreme capillary congestion, and a stimulus to the heart only aggravates the evil; and in the case of a diseased and weak heart, where repose is indicated a constant stimulation by alcohol adds immensely to its trouble.

It causes me daily surprise to observe how the effects of stimulation are overlooked. Often have I been called to see a patient apparently dying, sometimes of a nervous disorder, at another time of a liver complaint, and at *another of heart disease*. He is lying in bed, where he (or she) has been for *some time, and kept alive* (as it is said) by brandy; the breath is abominably

fetid; the heart's action is so rapid that it is impossible to say whether the organ is diseased or not; the patient refuses food, or if this be taken, it is rejected, and so he is plied with brandy to keep him alive; the body is, in fact, saturated with spirit, or its elements. My first remark on seeing such a case is, that a man cannot live on alcohol; he must take some food or he will die. The correctness of such common-sense remarks is admitted, but qualified with the statement that no solids can be taken, and that if stimulants be omitted it is feared the patient will sink. It is assumed that the constant administration of brandy is necessary for the temporary maintenance of life, and the idea seems never to have been conceived that the stimulation of the heart causes the weak, fluttering pulse, and the stimulation of the stomach a subacute gastritis. Do you ask me what method I adopt? The simplest possible. I withdraw every drop of the stimulant, and in a few hours the irritated stomach is partly restored to its normal condition, the nervous excitement abates, the patient takes a little food, and begins to mend. Do you ask, again, whether I do not fear any frightful results from the sudden withdrawal of the stimulus? I say, not the least; I have no fear of the consequences. Not of delirium tremens? Not in the least. This is a disease not induced by the withdrawal of stimulants, but, on the contrary, is produced by a recent debauch. For the production of delirium tremens the patient must have been such an habitual tippler as to have weakened his brain, and must then have had an overdose of the stimulant to set up the disease. There are no facts to show that the withdrawal of the accustomed drink is attended with any evil results, although I know that an imaginary fear of this kind leads to an erroneous and vicious method of treatment—the plying the patient with a stimulant during the violence of the attack, the effect of which is to prevent or prolong the cure. Rest and repose, with the avoidance of stimulation, is the treatment which the patient requires. The success of digitalis may be mentioned in corroboration of this view. I repeat that there are no facts to show that delirium tremens is produced by the withdrawal of stimulants, whilst it is a fact, as I could illustrate by many cases, that nothing but good results from its absolute discontinuance in the desperate cases to which I have alluded.

That many cases of disease of various kinds would do far better without stimulants I am perfectly confident. But lately I have seen the case of a gentleman, sixty years of age, who passed through a most severe attack of pneumonia without the use of stimulants. He had been a tolerably free liver, and would not have been called a good subject; but having before me the case of another gentleman of the same age, who had just died of pneumonia, and who had taken a large quantity of brandy, I readily acquiesced in the patient's own view, that none should be given. It is very remarkable what extremes we have reached, and on how slight a scientific basis is founded the treatment of pneumonia. Not many years ago the antiphlogistic method was adopted, including bleeding, antimony, calomel, &c.; then came the "let alone" method; and now we have the brandy treatment. What the need of this can be with Professor Hughes Bennett's statistics before us, I do not comprehend. My own opinion is (but of course this is only an opinion), that in any given number of cases a larger majority would recover under the old antiphlogistic treatment than by the more modern method by brandy. As regards heart-disease, the utmost discrimination is required in the use of stimulants. There are cases where an undoubted benefit is produced by them; but there are others, and these I have seen repeatedly, where alcohol has produced palpitation, fluttering, great distress, and constant sleepless nights, but where, on the other hand, the withdrawal of the spirit, and the substitution of a dose of digitalis or henbane, has been of the most essential service. The administration of a stimulus in the attempt to overcome disease, in lieu of good and well-tried remedies evinces the very worst form of medical scepticism with which I am acquainted.

It is not only in these severe cases of disease, but in lesser troubles, that your recommendation of stimulants may do incalculable mischief. You visit, for example, *an ailing lady*, and she details to you a number of troubles

of a nervous and dyspeptic character. She is sitting in-doors all day, taking no exercise, living well, and consequently drifting into a weak and flabby condition. You place your hand on her pulse, and finding it feeble, condole with her on her state of health, assure her that she does not live well enough, and order her a few extra glasses of wine or a little brandy. You find that she grows no better for the advice; but perhaps you never reflect that you have been adding fuel to the fire. Knowing not what to do in the way of treatment, you order her out of town, and she immediately begins to improve. She goes to Brighton, rides on horseback or walks miles a day on the Parade, regains her appetite, craves less for stimulants, and her health is restored. If, on the contrary, you fail to remove her from her home, she goes on from bad to worse; she takes to her bed, eats less food, drinks more wine and brandy, until, having become one mass of fatty degeneration, life can hold no longer, and death ends the scene. This lady has been killed with kindness. This is no imaginary case; my mind's eye is carrying me to the bedside of more than one such instance. Do not then assume that alcohol is an equivalent to a tonic, and that it must be necessarily administered because your patient is weak. It may be that that very weakness is due to the long-continued pernicious effects of this same stimulant; indeed, as you have often heard me say in the out-patient room, if a man comes into our presence with a tottering gait, bloated face, and his nervous energy all gone, you may be quite sure that he has been taking "strengthening" things all his life.

I will say no more on the subject, as I do not wish to speak condemnatory of alcohol as a remedy, since it is one of the most powerful agents we possess to rouse the dormant nervous power. Moreover, I do not wish to speak too dogmatically of its ill-effects, being fully aware that there are many holding very distinguished positions in the profession whose opinions are not in accordance with those I have expressed. Were it not for this reason, I should have used still stronger language than I have done; for even firm convictions must be restrained when we know that an amount of contrary opinion can be arrayed against us. It is, nevertheless, the duty of every one to express his own conviction when that is based on experience, and thus I shall ever feel bound to withstand the indiscriminate use of stimulants in disease.

Whatever may be thought of the remarks just made, there is one thing which I must insist upon—that is, when treating any malady, and the administration of alcohol is suggested to your mind, that you give the same grave consideration to its recommendation as you would to any other potent drug in the Pharmacopœia; not to sit down and give all your serious thoughts to the question of whether a grain of this or a grain of that drug should be ordered, perhaps twenty or thirty drops of ether, and then at haphazard order any loose number of ounces of brandy. You observe that I say nothing against the potency of alcohol in several states of disease; but I do speak strongly against its indiscriminate use without due consideration of its need or of its results. My arguments would equally apply did I find that opium or any other drug were indiscriminately used as a universal medicine. I should protest against the practice, whilst still possessing great faith in the virtue of the drug. If I can influence you to place alcohol in your list of drugs, so that you may administer it with the same caution as you do the several articles in the Pharmacopœia, then the object of these remarks will be fully answered.—*Lancet, April* 27, 1867, *p.* 505.

9.—AN ANALYTICAL EXAMINATION OF THE CAUSES OF GOUT.

By Dr. CHARCOT, Assistant-Professor at the Paris Faculty of Medicine.

After having briefly surveyed the history and geography of gout, we must now descend from this philosophical and comprehensive point of view to the minute examination of facts, in order to appreciate the conditions which, *under various circumstances*, give rise to that disease. In the course of this

lecture, we shall seize the opportunity of calling your attention to some interesting particulars which, up to the present time, had remained almost entirely unknown in France.

I. Individual Causes.

1. *Spontaneous Development.*—It is certain that gout, in some cases, arises spontaneously. The fact is corroborated by the testimony of all the authors who have written on the subject; and in my own practice I have met with cases of this kind. There exist, therefore, in the constitution of certain individuals, conditions which favour the development of gout; and external circumstances only serve to bring them to light. We need scarcely be astonished at this; for an excess in the production, or a deficiency in the elimination, of uric acid appears to lie at the root of the gouty diathesis. But in the normal state a small proportion of uric acid already exists in the blood: let its amount be increased, and the morbid process will at once begin.

2. *Hereditary Predisposition.*—The definition of gout, as given by modern writers, invariably comprehends the notion of hereditary transmissibility. Hospital Physicians have already ascertained its existence; and in town practice it is still more prominently brought forward. A few figures will better impress the fact upon your minds. The hereditary taint has been noticed.

By Scudamore, in 523 cases of gout, 309 times.
" Patissier, " 80 " 34 "
" Garrod, " 100 " 50 "

Hereditary gout is often premature; it frequently makes its appearance before the age of 30 or 35, the usual period for the spontaneous manifestation of the disease. It often exhibits itself at the same age among all the members of certain families. Garrod informs us that in one of the most illustrious houses of England the eldest son is invariably affected with gout on succeeding to the title; and this singular inheritance has been entailed in the family for the last four hundred years.

3. *Sex.*—The influence of sex on the production of gout is evident. It cannot be denied that females enjoy a peculiar immunity in this respect. Out of the eighty cases collected by Patissier, seventy-eight were observed in men, and only two in women. The change of life is the period at which the disorder chiefly exhibits itself in female patients—a remark which had not escaped the sagacity of Hippocrates.

We discover here an essential difference between gout and chronic rheumatism. This rule suffers, no doubt, a few exceptions, and some ladies are affected with gout early in life; but in such cases we almost invariably recognise an hereditary taint.

It must be added that the low asthenic forms of the disease predominate in female subjects.

4. *Age.*—The classical time of life for gout extends, according to Scudamore, from 30 to 35. It is seldom met with before 20 or after 60. Garrod, however, has seen it once in a boy 9 years old, and once in a lad of 17. He has also observed a few cases in which the first attack occurred in men of 60.

Rheumatism, on the contrary, is met with at an earlier age, and usually makes its first appearance before the patient is 35.

5. *Constitution.*—Attempts have often been made to establish the characteristics of a peculiar constitution which predisposes the subject to this disease; but experience has shown that gout spares no constitution whatever, and assails weak, debilitated patients as well as powerful, athletic men. The type of the disease corresponds, however, to the general condition of the system. The sthenic or acute form is principally observed in strong plethoric subjects; the low asthenic form in women and invalids.

II. Hygienic Causes.

From the causes of gout which are inherent to the patient himself, let us

now turn to those which result from the conditions in which he lives. Let us examine his habits, and more especially his diet; we shall discover facts of the highest importance in prosecuting this study.

1. *Climate.*—Medical geography has already taught us that gout belongs almost exclusively to the temperate zones of the globe, and flies from tropical regions; but, although entirely unknown in Brazil, in Africa, and under the line, it occasionally affects Europeans, who have brought into hot climates the habits of northern countries. For this reason the English in India are sometimes troubled with its symptoms, though less frequently than in their own native country.

2. *Full Diet, and Want of Exercise.*—It has at all times been acknowledged that an easy life and high feeding—two conditions not unfrequently combined in the same individual—constitute a strong predisposition to gout; it predominates, in consequence, among the higher classes of society, and is less prevalent in the lower ranks. In familiar style, gout is often said to be the result of a surplus of receipts. We shall soon see that facts cannot always be so easily interpreted; but it is quite certain that an excess of animal food promotes the development of this complaint, and that voracious eaters are frequently numbered among its tributaries.

3. *Influence of the Nervous System.*—The influence of cerebral activity cannot in this case be denied. Intellectual labour, strong emotions, great mental exertions, have always been numbered among the causes of gout. Sydenham, who was himself a martyr to this affection, attempts to console himself by reflecting that "*divites interemit plures quam pauperes, sapientes quam fatuos.*" The chief politicians of England have been numbered among its victims. We need only mention the two Pitts, father and son. The Earl of Chatham was by no means a votary of Bacchus. It is true that the same can hardly be said of his illustrious son, who never addressed the House of Commons without having previously inflamed his eloquence by freely resorting to the bottle.

4. *Venereal Excesses.*—The depression of the nervous system which results from sexual indulgence creates no doubt a strong predisposition to gout. But a natural explanation here suggests itself. Looseness of morals in general is closely allied to intemperance, which in this case probably plays the chief part in the production of disease.

5. We have now to examine the action of two of the most important causes of gout—I allude to *fermented liquors* and *lead-poisoning*.

Such is the influence of fermented liquors that, according to Garrod, man deprived of these drinks would never have known gout. The action of lead-poisoning is far less extensive, but, as regards the theory of the disease, it is a fact of the utmost importance.

We will now proceed to investigate these two points.

Fermented Liquors.—A broad line of distinction must be drawn between spirits (rum, brandy, whisky, gin, etc.), which contain from 40 to 70 per cent. of alcohol, and ordinary fermented drinks (wine, beer, cider), the alcoholic strength of which varies from 4 to 20 per cent. At first sight it seems natural to suppose that the more potent liquors are by far the most conducive to gout. But you will perhaps be astonished to find that the use of ardent spirits does not appear to be connected in any degree with the production of this disease. In fact, gout is far from being widely diffused among the dram-drinking populations of Europe. It is scarcely ever met with in Sweden, where *alcoholism* is so frequent, according to Professor Magnus Huss. The same is the case in Denmark, Russia, and Poland. In Scotland and Ireland gout is seldom observed among the lower classes. At Edinburgh Bennett and Christison have scarcely met with more than one or two cases in their hospital practice. Now the only strong drink in general use in those two countries is whisky.

In London, on the contrary, gout is exceedingly frequent among work-*people, and is constantly* observed in Hospitals. Now the only important

difference which exists in this respect between the northern and the southern parts of Great Britain is the enormous quantity of *strong beer* absorbed by the inhabitants of the metropolis, under the various names of *ale*, *stout*, and *porter*. The extraordinary influence of these drinks is acknowledged by all English medical writers. Scudamore informs us that since the use of porter has been universally adopted in London, gout has become infinitely more frequent among the labouring classes. Watson, Budd, and Todd corroborate this assertion. According to the last-mentioned author, most of those who usually drink beer, and especially porter, become, sooner or later, victims of gout.

An interesting instance of this is recorded by Budd, whose words we shall here beg leave to quote:—"There is a body of men employed on the Thames whose occupation is to raise ballast from the bottom of the river. As this can only be done when the tide is ebbing, their hours of labour are regulated by that circumstance, and vary through every period of night and day. They work under great exposure to inclemencies of weather: their occupation requires great bodily exertion. . . In consideration of this, their allowance of liquor is very large: each man drinks from two to three gallons of porter daily, and generally a quantity of spirits besides. This immoderate consumption of liquors forms the only exception, as far as relates to food, which these men offer to the general habits of the lower classes in London. Gout is remarkably frequent among them, and although not a numerous body, many of them are every year admitted to the Seaman's Hospital ship affected with that disease." These poor labourers are therefore admitted for once to share in the privileges of the peers of the realm, yet they are almost all derived from the peasantry of Ireland, and can scarcely, therefore, inherit a disposition to gout.

Garrod has also arrived, on this side, at similar conclusions. He states that persons employed in breweries are frequently affected with gout; and the abuse of malt liquors, especially porter, is the only plausible explanation of this morbid tendency.

And yet the various kinds of beer are far from containing any large amount of alcohol. Mulder states that Scotch ale contains 8 per cent., and porter only 5 per cent. The proportion is below that of our French wines, and not superior to that which exists in German beer—a liquor the effects of which are entirely different, notwithstanding the enormous quantity absorbed by our neighbours across the Rhine.

You therefore see that *à priori* reasoning cannot be brought to bear on this subject, and that the influence of fermented liquors on the production of gout is entirely independent of the amount of alcohol which they may happen to contain. Other conditions, our knowledge of which is hitherto very imperfect, appear to take the lead, and it is only from experience that we can learn the real state of things.

Let us now examine the action of wine. The highest rank, as far as our subject is concerned, evidently belongs to the wines, (port, sherry, Madeira, Marsala) which are so generally preferred in England by all classes of society. They all contain a large proportion of alcohol, which varies from 17 to 20 per cent.

Light wines (Rhine, Moselle, champagne, claret) are far from exerting a similar influence.

But the same cannot be said of Burgundy, which, however, does not contain more spirit than the qualities we have just named. Gout is contained in every glass of red Hermitage and Burgundy, especially the latter, if Scudamore is to be trusted.

Cider itself, an apparently inoffensive beverage, is said to favour the development of this disorder. According to Garrod, this injurious property does not belong to *hard cider*, and only exists in the new unfermented liquid.

Having, as we believe, sufficiently illustrated the connexion between gout and fermented liquors, let us now turn to a different subject.

Lead Poisoning.—Garrod had ascertained some time ago that out of eighty-one *gouty patients admitted into his* wards no fewer than sixteen were painters

and plumbers, and ulterior investigations have corroborated this singular result. The introduction of *lead* into the system must therefore be placed in future among the causes of gout.

The fact having been established, documents in its favour came pouring in from all side. Among the authors who had noticed the coincidence before Garrod we will quote Musgrave, who saw gout appear immediately after *colica pictonum*; Falconer, who notes the same fact; Parry, who shows that gout frequently exists in cases of paralysis from lead poisoning; and lastly Todd, who mentions several cases of gout observed under similar circumstances.

Since the appearance of Garrod's excellent work, several English authors have published observations of this kind; we will only mention Burrows and Begbie. But the habits of English workmen, which have just been discussed, exert, in all probability, a powerful influence in such cases. In France, where lead cholic is so frequent, why is gout so seldom met with among the lower classes?

A few gouty patients exist, however, among those who have undergone the process of lead poisoning. I have been so fortunate as to observe the coincidence myself in one most remarkable case, and Dr. Bucquoy has just met with another at La Charité.

The cause of this singular connexion between two very different disorders still remains obscure. Garrod has discovered that lead poisoning produces an undue accumulation of uric acid in the blood, especially in an advanced stage, where paralysis already exists. The fact has been ascertained in patients who were not labouring under gout, and who do not seem to have been affected with Bright's disease. On examining the urine the proportion of uric acid was found to have considerably diminished, but we are not informed whether there existed any degree of albuminuria. Garrod is not able to decide whether there is an excess in the production of uric acid or an insufficient elimination. He leans towards the latter hypothesis, and relies upon the following experiment.

After having analysed every day, during a certain lapse of time, the urine passed by several patients affected with various diseases not connected with the urinary organs, he gave them medicinal doses of acetate of lead. Under the influence of this preparation, the excretion of uric acid was seen gradually to decrease.

If, therefore, the introduction of lead into the system calls forth the manifestation of gout, its action seems principally to bear upon the kidneys. It paralyses their functions as far as the excretion of uric acid is concerned. But can this single cause give rise to the gouty diathesis? It may do so in a few insulated cases; but if there exist other predispositions acting in the same direction, the effects of lead poisoning will appear in their full light.

III. Exciting Causes.

Although incapable of creating gout by themselves, the conditions which we are now going to enumerate are sufficiently powerful to provoke attacks of the disease when it has already taken possession of the system.

1. *Alcoholic Liquors.*—In gouty subjects certain wines (port, champagne, &c.), even if taken in small quantities, will determine an attack of gout, which is sometimes limited to the great toe. According to Garrod, whenever a few glasses of wine produce inflammation of the joints, that inflammation is invariably connected with gout.

2. Indigestion and all other derangements of the stomach act in the same manner.

3. Cold and damp, and suppressed perspiration, produce similar effects.

4. Immoderate intellectual exertions exert on the production of gout an influence to which we have already called your attention.

5. Surgical injuries, operations, fractures act in the same way. I have *myself seen a wound produce* simultaneously trismus and gout.

6. Debilitating causes, such as loss of blood, protracted illness, &c., &c., have also a marked influence in the production of gouty symptoms. This fact deserves notice, gout having often been described as a malady peculiar to plethoric subjects; but Todd has amply proved that it is equally severe in debilitated patients.—*Medical Times and Gazette, May* 4, 1867, *p.* 463.

DISEASES OF THE NERVOUS SYSTEM.

10.—ON THE USE OF STRYCHNINE IN EPILEPSY.

By Walter Tyrrell, Esq., Malvern.

[Mr. Tyrrell published a paper on this subject in May, 1867.]

I stated at the end of my former paper, my belief that in "strychnine we possess a drug which will always control the excitability of the medulla oblongata, and restrain the attacks of convulsion." This opinion will, I think, be found to be remarkably strengthened by a perusal of the following cases. One most important fact is to be gathered from them—viz., that large doses of the drug must be given to produce the favourable results. In some of the cases the doses may appear formidable, but I feel confident that with care and watchfulness no ill effects need follow their administration to the epileptic. In such cases the system appears to lose its susceptibility; and the drug even in large doses produces none of the ordinary signs of disagreement. In no case have I seen it produce any mischievous excitement or irritation; and I may state that in one very severe case, still under treatment, I have carried the dose as high as one-fifth of a grain, taken twice daily, and this continued for nearly three weeks together, not only without its producing the slightest sign of irritation, but on the contrary the most marked diminition in the frequency and violence of the attacks. The following case although the attacks had but recently come on, is interesting as showing how rapidly the beneficial effect of strychnine is often gained, no attacks having supervened after two doses (each of one-twelfth of a grain) had been taken.

H. R., aged 29, has been of late years much exposed to heat in China, Singapore, and Japan; had congestion of the liver in March, 1865; was invalided home in June, 1865; since which time he has been living at home, under treatment for enlargement of the liver, using iodide of potassium and iodine ointment locally. On May 22 of this year—a very cold snowy day—he imprudently stayed out all day fishing, and at dinner that evening was seized with a violent epileptic fit, accompanied with great convulsion; this was followed by other attacks at the following intervals:—May 25, three fits, at intervals one hour and a half; May 30, a fit in the evening; June 1, two fits, with six hours' interval; June 2, one fit in the evening. On June 5 he arrived in Malvern, and I prescribed for him one-twelfth of a grain of strychnia twice daily, allowing him to continue his potash in rather increased quantity. On the morning of the 6th he had three fits, during the first of which I was present; they were very convulsive, and produced an extremely prostrating effect on his mind—so much so, that, even after the ordinary stupor had passed off, he was unable to answer the simplest question without consideration and great hesitation. It is needless to give a daily report of this case. I increased the dose of the strychnine to one-eighth of a grain; he had no further attacks; and his return to health, both bodily and mental, although gradual, was most perfect. He is now at the seaside, and may be considered to all intents and purposes convalescent. In this case it was curious to observe how the inclination to an attack (which occurred several times during the early treatment of the case) yielded at once to a slight increase in the strength of the dose. I may say that in this case I found the use of ice to the nape very useful, insuring quiet sleep, and also allaying a frequent tendency to irritability.

In the following case, where the attacks were dependent on menstrual

irregularity, the utility of combining the strychnine with remedies directed to the removal of the exciting cause will be apparent.

L. A., aged 17, a not unhealthy-looking girl, has never menstruated properly; has been subject to epilepsy for four years, the interval never been longer than one week; the attacks vary in intensity, a slight one being sooner followed by others. In this case I commenced with one-sixteenth of a grain of strychnia twice daily, gradually carried up to the tenth, at the same time giving her aloes and myrrh and assafœdita in pills twice daily. In this case a perfect immunity from attacks commenced with the treatment, and has continued up to the present day, a period of nearly three months. Although the menstrual irregularity has not entirely ceased, it is very much ameliorated. I used also in this case the cold effusion to the nape, coupling it at times with the application of warmth to the feet. This case, although not severe, is a type of a very prevalent form of the disorder, and shows how amenable such cases are to treatment. In another case somewhat similar, which is still under treatment, I have the greatest benefit from the use of the bromide of potassium in combination with strychnia.

E. H., aged 14, a fair girl, partially paralysed on the left side. When two years old had what was called brain fever, during which she was insensible for a length of time; recovered, but had a return about two years ago. Since the first attack she has been subject to continued attacks of *petit mal*, sometimes five and six in the day. She turns slightly to the right; is slightly convulsed; sometimes is partially conscious during them, and tries to talk; sometimes she bites her tongue; her manner is silly, being fond of repeating lines of poetry, for which her memory is good. She has slight tenderness on pressure over the upper cervical vertebræ, and on percussing the atlas with the finger points she complains of pain at the epigastrium. The attacks sometimes come on during sleep. I will give here an extract from the diary kept by the parents. The patient came under my care in May, and I prescribed for her:—℞. Tinct. nucis vomicæ ʒiij., syr. aurantii ℥j. M., cap. ʒj. bis in die ex aquâ.

The following is a diary from May 29 up to the stoppage of the attacks:—

May 29. Four fits in the day; two in the night.
30th. No fits in the day, but eight in the night, two of them being severe.
31st. One fit in the morning; eight again at night, but less severe.
June 1. No fit in the day; four at night.
2nd. No fit in the day; five at night.
3rd. No fit in the day; four at night.
4th. No fit in the day; three at night.
5th. No fit in the day; three slight ones at night.
6th. No fit in the day, and if any at night very slight.
7th. No fit in the day; only one observed at night,
8th. No fits day or night.
9th. No fits day or night.
10th. No fit.
11th. No fit.
12th. No fit.

And so on. Since this date she has continued almost entirely free from attacks, but few having occurred, and those of an altered and much slighter character, which yield readily to a slightly increased dose of the strychnine.

Although the following case is still under treament, yet I think a slight sketch of it cannot fail to be interesting as exhibiting the effects of strychnine in very severe convulsive epilepsy, and as also showing what large doses of the drug may be given with impunity. This patient, who has suffered for some years, is one of the severest cases of the disorder I have ever seen. I commenced treating him on the 15th June last, and as the case is still under treatment, I will merely give a comparative table of the number of his attacks during May of the present year and July. During the former month he was under no treatment of any kind. During July he was taking *strychnine in the doses appended* to the table. The attacks, which occurred *almost exclusively at night*, were most violently convulsive. They were

much influenced by atmospheric changes, heavy thundery weather invariably increasing both their number and severity. Thus July would, under ordinary circumstances, be his most unfavourable month. In addition to the strychnine during part of the month, he was using cold affusion to the nape, and ice to the occiput during the night.

No Treatment.

May 1, 1867	2 fits	May 17, 1867	1 fit
" 2, "	1	" 18, "	0
" 3, "	2	" 19, "	0
" 4, "	3	" 20, "	0
" 5, "	2	" 21, "	1
" 6, "	3	" 22, "	0
" 7, "	2	" 23, "	2
" 8, "	2	" 24, "	2
" 9, "	1	" 25, "	3
" 10, "	0	" 26, "	3
" 11, "	2	" 27, "	2
" 12, "	2	" 28, "	1
" 13, "	4	" 29, "	2
" 14, "	3	" 30, "	0
" 15, "	3	" 31, "	0
" 16, "	3		

Under Strychnine.

	Fits.			Fits.	
July 1, 1867	0	½ gr	July 16, 1867	3	
" 2, "	1	very slight	" 17, "	0	½ gr.
" 3, "	1	very slight, no con.	" 18, "	0	"
			" 19, "	0	
" 4, "	0		" 20, "	0	
" 5, "	0		" 21, "	0	
" 6, "	0		" 22, "	0	
" 7, "	1		" 23, "	0	
" 8, "	2		" 24, "	0	
" 9, "	0	⅓ gr.	" 25, "	0	
" 10, "	0		" 26, "	0	
" 11, "	0		" 27, "	1	
" 12, "	0		" 28, "	1	
" 13, "	0		" 29, "	0	
" 14, "	1	$\frac{1}{12}$ gr.	" 30, "	0	
" 15, "	0		" 31, "	0	

On July 14, owing to a misunderstanding, he only had $\frac{1}{12}$ of a grain instead $\frac{1}{3}$th. It will be seen that four fits followed in rapid succession. I think this table shows the power which strychnine possesses in restraining the epileptic attacks. I may add that, although so remarkably lessened in number, they were not at all increased in severity, but, on the contrary, were less convulsive. The above table gives the following results:—

May.		July.	
No. of attacks.	Nights free.	No. of attacks.	Nights free.
51	7	11	23

It will be observed that during the latter half of the month the dose of strychnine was as high as one-fifth of a grain, taken twice daily, and this without its producing the slightest sign of excitement or irritation. In combination with the strychnine the patient is taking the infusion of digitalis.

In conclusion, I would reiterate the summing up of my last paper—"That

in strychnine we possess a drug which will *always* control the excitability of the medulla oblongata and prevent convulsions, but that to cure the disease we must also remove the exciting cause.—*Medical Times and Gazette, Aug.* 24, 1867, *p.* 201.

11.—ON A CASE OF TRAUMATIC TETANUS SUCCESSFULLY TREATED WITH THE ORDEAL BEAN OF CALABAR.

By Dr. A. Campbell, Navenby, Lincolnshire.

E. L., a delicate-looking girl of 12 years, was chopping meat with a chopping-knife on the 1st of January, 1867, when she cut the dorsal aspect of her left thumb. near the articulation of the metacarpal bone with the trapeza. The wound healed slowly by granulations, leaving an ugly elevated cicatrix. On the 14th of January, when the wound had nearly cicatrised, she felt a stiffness about the jaws which passed off on the application of a mustard poultice and a little medicine. She now continued in her usual state of health till the 1st of March, when she again experienced a sensation of stiffness of the muscles of the lower jaw, and slight twitchings of the muscles of the face. On the 2nd she was worse, and had a difficulty in opening her mouth. On the 3rd she had greater difficulty in opening her mouth, and had occasionally severe spasms in her arms and legs. She now continued getting gradually worse, and on the 8th, when Mr. Smith, my partner, was called to see her, she presented all the appearance of one who had trismus. The jaws were firmly locked, and the lower jaw was drawn forward, so that the teeth of the lower jaw were about a quarter of an inch in front of the teeth of the upper jaw, which enabled him to get fluids easily into her mouth. Her body and limbs were perfectly rigid, and the peculiar sardonic grin was very marked. The slightest noise, or moving her in the least, brought on the most violent spasms. She found comfort in lying on her stomach, with her head and shoulders over the edge of the bed. When the spasms came on she tried hard to get hold of anything in the way, so as to prevent the tendency in the direction of opisthotonos. A dose of castor oil, with turpentine, was now prescribed, and a mixture containing chloroform to be taken every three hours.

On the 19th the bowels had not been moved. To have an enema, which relieved her bowels; and to take a grain of opium at bedtime.

She continued much the same till the 13th, when I saw her for the first time (two weeks from her attack), when she appeared the most pitiable object I have ever seen. She seemed all of apiece, the muscles being as rigid as a board; the slightest noise produced severe opisthotonos, the body being bent into an arch, the girl resting upon the back part of her head and heels. The muscles of the back were so powerfully contracted as to cause angular, or Pott's, curvature of the spine. The scapulæ were drawn backwards forcibly, fixing the chest so that respiration was performed with difficulty. I ordered her an enema of castor oil and turpentine, and to have half a grain of opium every three hours.

I then consulted with my partner as to the advisability of trying the Calabar bean, so highly recommended and so successfully used by my respected teacher, Dr. E. Watson, of Glasgow. We agreed to give her the benefit of it (although my partner was sceptical about new drugs). Not having the tincture recommended by Dr. Watson at hand, I began on the 17th by giving her five drops of a solution of the extract in white wine (twelve grains of the extract to the ounce of wine, five drops of which, therefore, contained about one-eighth of a grain) every half hour.

I may mention that the girl lived four miles from my residence, so that it was impossible for me to see her so often as I could have wished. However, her stepmother, who is a most attentive nurse, and to whom I explained the physiological effects I wished to produce, was most assiduous in her attention. She began with five drops about eleven a. m. of the 17th, and continued it every half hour.

On the morning of the 18th I found her about the same, although the nurse thought the spasms not quite so violent. Pupils widely dilated; pulse 110. I now ordered eight drops every hour. At eleven p. m. (by which time she had taken about six grains, in thirty-six hours) she became half comatose; pupils considerably contracted; breathing more easy; no tendency to opisthotonos. The muscles of the extremities became quite relaxed; and she had a feeling of sinking, as if about to die. The nurse, being aware of the physiological effects anticipated, at once stopped the bean, and gave her two teaspoonfuls of brandy every half hour.

At one a. m. on the 19th she had a natural motion of the bowels. She now became less distressed, and fell into a quiet sleep of three hours' duration—the first she had had since her illness. At the time of my visit she was very much depressed; pulse 120, fuller than it had been; pupils slightly contracted; breathing easier; muscles of the arms and legs quite flaccid; although unable to move them. When moved, she had still severe spasms of the muscles of the back; I was, however, pleased to find that the medicine had so far produced its great work. I ceased giving the bean for a time, and ordered her beef tea and brandy *ad libitum*.

March 20th. I found her decidedly better. Pulse 110, soft and regular; she has more power over her arms and legs; can open her mouth a very little. Spasms in the back still violent when moved, but not otherwise. I now ordered the bean in quarter-grain doses, in pill, every two hours.

21st. Slept a little during the night. Can open her mouth so as to admit the edge of a teaspoon; speaks more distinctly; says she feels hungry. To have an egg beat up in brandy, and to take a dose of castor oil.

22nd. She slept during the night, and did not take her pills regularly. To have an enema.

She continued gradually improving till the 26th, when she could open her mouth freely; eats and sleeps well; bowels still constipated; sits up a little when propped in bed. No spasms of the extremities; but when moved still complains of great pain in her back. The muscles of her back very rigid.

27th. I ordered her half-grain doses every two hours, so as to counteract the violent spasms of the back.

28th. I found her not so well; less power over her extremities; spasms of the back not so painful. She has a nasty cough, which made me feel anxious. Stopped the bean; and to have nourishment and wine, with five grains of Dover's powder every four hours.

30th. Cough less troublesome; can move her limbs freely; pulse stronger. Has had no bean since the 28th.

By the 1st of April she was so well that, the weather being fine, she was taken out in a perambulator. Still considerable rigidity of the muscles of the back. I ordered chloroform and laudanum liniment.

2nd. To have half-grain doses of the bean three times a day.

6th. Can stand upon her feet, although she feels very shaky, and grasps at any thing in her way so as to support her back. To have twenty drops of tincture of muriate of iron three times a-day in water. Takes her food with relish; bowels now regular, &c. The muscles of the back still rigid—more so on the right side of the spine than on the left. When moved suddenly, or when she attempts to walk, she has considerable pain in her back.

12th. Improves slowly; pain not so great in her back when she sits up or stands; muscles of the back softer; still considerable curvature of the spine. To continue the iron, and the pills to be left off.

20th. The muscles of the back much softer, but painful when pressed upon; the spine gradually assuming its original form; feels the muscles of her legs weak when she stands upon them; otherwise quite well, and evidently gaining flesh. I now consider her cured so far as the tetanus is concerned; but, as she still requires great care and good nursing beyond the power of her parents' means to bestow upon her, I have recommended them to remove her to the Convalescent Hospital at Lincoln.

The facts of the case speak for themselves, and I publish an account of it in the hope that others may be induced to give the drug a fair trial in similar

cases. There can be no doubt that it has a wonderful effect in producing relaxation of the tetanically contracted muscles. The muscles of the back, being the most powerfully contracted, were of necessity the most difficult to overcome. The pupils were never contracted to a point, although upon two or three occasions they were perceptibly so. Another physiological effect I am inclined to believe it produces (which has been already hinted at by Dr. Watson) is catharsis (see Lancet, March 2nd). It is satisfactory to think that the cases which have been successfully treated by the bean enable us to look forward with more confidence than hitherto in treating that terrible disease. In future, I would be careful, after having produced a decided effect upon the system, not to carry the effect of the bean too far; for by doing so, in order to overcome the more obstinate muscles, such as those of the back, one might paralyse the already relaxed muscles, and so retard convalescence. —*Lancet, August* 10, 1867, *p.* 157.

12.—THE INFLUENCE OF EXTREME COLD ON THE NERVOUS FUNCTIONS.

By Dr. B. W. Richardson, F.R.S., Senior Physician to the Royal Infirmary for Diseases of the Chest.

The phenomena observed on freezing the animal tissues, depend essentially on a change in the condition of the water of the body. By the cold the water is transferred from a liquid to a solid state. Nerve tissue contains eighty-five per cent. of water. Make this water solid, and we get all the phenomena of negation demonstrated—a sort of temporary death. These phenomena may be produced also by other means. Heat is capable of bringing about a similar state. Abstract the water, dessicate the tissue by any means, and the result is the same. In illustration of this, a solution of brain matter was placed on a piece of glass, and on the spray being played upon the opposite side of the glass, a visible change was brought about. This was owing to the solidification of the water, and the separation of the albumen. The same appearance was produced by dropping on the matter some absolute alcohol. This result was brought about by the great affinity of the alcohol for water, depriving the brain matter of this substance. Heat was next applied by means of a spirit lamp. This agency brings into play similar sets of phenomena.

Thus, by congelation on the one side, and coagulation on the other, we get exactly the same phenomena. The lecturer then went on to refer to some things which are more subtle in their action than these. But these more subtle agents are no less definite in their effects.

Take, for instance, shock, which suddenly produces the phenomena of negation, seen in freezing the nervous centres. Again, a blood change is equally subtle in its operation, and the same may be said of pressure on the brain, which exerts a very definite influence. The lecturer conjectured that probably the effects of certain sounds and odours on the brain, might be explained in a similar manner, and so of certain sights which are known to produce the same phenomena. Moreover, loss of blood leads to the same change—the abstraction of the watery part of the nerve tissue. This leads to a conjecture that some of the phenomena of the collapse stage of cholera may depend on the abstraction of water, and the crystallisation of the nerve matter in the body.

One of the most striking things in all these experiments on animals is the local independency of the several portions of the nervous system. Every part seems to have its own centre, so we can isolate many parts of the cerebrum. We may act on one hemisphere or both, or on the cerebellum, or on the medulla, or spinal cord. But we do not thereby necessarily interfere with the functions of the other parts of the nervous system. This fact explains many things in connection with, or in elucidation of, brain disease. There *is, nevertheless, to* be noticed a kind of balance on one part by another. In

reference to this, it is clear that one hemisphere balances another. The corpora striata balance, and are balanced by, the cerebellum. The medulla is thus in the same relation with some obscure point not yet fully made out. One part of the medulla balances a part of the spinal cord connected with respiration. The medulla presides over the muscles used in inspiration; the part of the cord balanced by it presides over those of expiration. If we freeze the medulla we stop the breathing, but we stop it by arresting the inspiration. In the opposite case, that of freezing the segment of the cord balanced by the medulla, we stop the breathing by arresting the expiratory act. Thus, in the nervous functions there is a balance of opposite powers, just as in the muscular system there is a balance of flexors and extensors. This is of a considerable import in a pathological point of view. In diseases in which there is a tendency to fall forwards, for instance, this propensity may indicate disease of the corpora striata; but it is to be remembered that the disease may be negative while the symptoms only show over-action of the balancing part. The disease may be the result of negation of the function of the corpora striata, while the phenomena of the disease, the symptoms, are those of excessive or unregulated action of the cerebellum. Psychologically, these experiments are also interesting. This is, perhaps, illustrated in the act of going to sleep in the upright position. Sleep seems to begin in the visual organs. As the anterior part of the brain first loses its power, we see the tendency to nod—to fall forwards—caused by that part becoming inactive, while the posterior parts retain their function of propelling. The same may perhaps be said of the shock which sometimes results from looking down great heights. This shock paralyses the anterior or thinking part, and the cerebellum retains its power, and so gives an impulse forward. This question might be put to the test by sportsmen. A bird shot flying will fall at once, rolling over and over *forwards*, while another turns over many times backwards, in its descent. Dr. Richardson believes that in one case the bird will invariably be found shot through the cerebrum, and in the other through the cerebellum, and he suggested that this fact should be noticed by such of his hearers as had the opportunity of joining in the sports of the field.

In diseases, exactly the same reasoning applied. An instance of suicide by hanging was mentioned. The man, however, had on, at the time of his attempt, a soldier's stock, which, by its compression on the jugulars, caused extreme pressure on the brain, the ventricles after death being found full of effused liquid. He was cut down alive, and immediately ran forwards several yards to a wall; afterwards he had the same symptoms. A man at Newcastle-under Lyne lost a portion of the bone of the skull, and on pressure on the brain, it was noticed that he pitched *forwards*. It was also observed that in certain states of the weather a similar tendency was evinced, probably when the atmospheric pressure was increased.

In therapeutics the experiments may be of great value. First of all, they may indicate the mode of action of alcohol. This fluid abstracts water from all the tissues, and most of all from the brain. But there is along with this a liberation of heat, and then a loss of heat, as the alcohol passes away with the water it has abstracted. Thus we have direct loss of water from the brain tissue, and loss of heat from the whole body.

By a subtler action, ether and chloroform modify the relations of water and albumen. It was thought, years ago, that chloroform suspended oxidation. The idea arose with Snow, but it was not clearly shown to be fact. Some have thought that the chloroform was absorbed and picked up caloric in the system, and by expansion in the closed cavity of the skull, caused pressure on the brain. This idea was suggested by finding that if the skull be opened while an animal is under the influence of the drug, the brain does protrude. But it appears that some more direct change takes place. Dr. Richardson had placed animals in a rarified atmosphere, and found the effects of chloroform precisely the same.

The effect of nitrite of amyle, more subtile and more remarkable still, was next adduced in support of this view. Then was adduced amylic alcohol. This suspends all nerve function, but the animal, though apparently dead, will

afterwards recover. A guinea pig, to which this substance had been given, was then passed round, and appeared quite lifeless. The lecturer stated that it would recover the next day. On recovery from this poison, symptoms like strychnine poisoning are observed. It is a spinal poison.

Antiphlogistics.—The mode in which these may act was next referred to. The familiar experiment of the loss of heat on a solution of salt in water, was shown by the thermometer. Then a frog was injected with a solution of chloride of ammonium. This operation produced artificial cataract. The same effect was next produced on another frog by freezing the eye-ball with the ether spray. In each case recovery took place.

The question was now propounded, can cold be applied for the cure of inflammatory symptoms? The spray has been used to subdue acute mania. Cold below zero, for half an hour at a time, to the anterior part of the skull, has been found quite safe. It has been followed by quiet sleep, and by no extreme reaction. In chorea the same agent has been used.

In recovery from rheumatic affections, in which there is excessive vascularity of the membranes of the cord, it has been used in France, and Dr. Richardson looked upon it as likely to cure.

In ganglion, formed on a nerve, it has been successfully employed to stop pain and motion. This led Dr. Richardson to try it in tetanic action. To show how far he had pursued this, he injected a frog with 1-45th part of a grain of strychnine, and placed it under a bell glass on the table. For two or three minutes it tried to escape, and seemed lively enough. Then tetanic rigidity came on. When the effect of the poison had become so fully developed that the least touch caused violent convulsions, the cerebrum-spinal cord were frozen by the ether spray, and the effect of the strychnine was completely subdued. A frog does not eliminate rapidly, but possibly an animal with more effective secreting power might be kept frozen until the poison has passed away.

Could we then in tetanus keep a patient frozen until the tetantic poison could all be eliminated? If indeed that disease depend on a poison at all, and may not rather consist in some molecular change, such as may be caused by a shock or blow:—if we had the hope, there is a great practical difficulty. Over the spinal cord there is so thick a layer of non-conducting tissue that no effect can be produced. In experiments with a delicate thermometer inserted in a portion of the spine, the spray being directed on the outside of the muscles, Dr. Richardson has not been able to make a difference of one degree; though he would not assert that in the living body some effect might not be *indirectly* produced through the skin, or its vascular and nervous system.

In a warm-blooded animal even actual freezing of living structure may be induced without any stage of pre-action, provided always that the heat be abstracted with sufficient energy and rapidity. Let me demonstrate this on my own body.

We have here for our spray apparatus a very light hydrocarbon fluid, called rhigolene. It was sent to me by Professor Donaldson, of Baltimore, and it is used extensively in America for the production of local anæsthesia. It boils at 70 Fahr. Dr. Sedgwick will direct a large spray of this on my arm, and, as you see, the process of freezing is immediate. The part, a moment since flexible, warm, and sensitive, is now hard, cold, and absolutely insensible. There has been no redness, no pre-exalted sensibility, no evolution of heat—in short, no stage of *pre*-action. All the parts have been rendered inert together, and when the cold is withdrawn recovery is equally rapid, and is without *re*-action. Some day we may have a means of extending this rapid inertia so as to make the whole of a limb insensible, and to operate on a part some considerable distance from the point where the application is made. This process of rapid freezing is the best and safest.

When the process of sudden freezing is brought to bear on the whole cerebro-spinal system of a cold-blooded animal, every function of life is immediately suspended. Here is a large powerful frog. I direct the rhigolene *spray on the head and spine* for a few seconds. I place the animal thus pet-

rified in your hands; it is like marble—a hard frozen mass. You would declare it dead; it only sleeps. As it takes up heat from the surrounding air, its pulses recommence; in a short time it leaps; it lives again, and it sustains no injury. If you ask me how long a frog could be kept in the state of temporary death we have here seen, I answer that time seems to me to make no difference as to results, the conditions being sustained. I have every reason to believe that if the animal were kept thus frozen for a century, or any number of centuries, it would live again as well as ever on restoration of warmth. By this freezing we have fixed all the water of its tissues; we have stopped evaporation, we have suspended motion, we have arrested waste; but we have done no injury that may not be removed by the re-solution of the water and by the re-communication of motion, from the act of re-solution, to the particles of water.

We may illustrate the phenomena thus observed by experiment on a lower mechanism; and, although the illustration is necessarily extreme, it is useful, as familiarising the mind to the study of physical effects proceeding from a common cause. I take my watch and find it is in motion; its hands move under the influence of force derived from the main-spring, which main-spring is enclosed in a small box lying at this point. I bring extreme cold to bear on the box holding the main-spring; I change in this manner the condition necessary for the work of the spring, and the watch immediately ceases to go. It is not physically injured, but it is stopped, and it would remain stopped for any length of time, the conditions being precisely the same. I change the conditions; I allow the watch to absorb the surrounding heat, and in time of itself it goes again. In the watch the cold stopped the motion by modifying the molecular condition of the metals. In the frog the water stands in the place of the metals, and the cold stopped the motion by acting upon the water.

Hyoernation.—It is impossible to observe the phenomena of prolonged and profound insensibility produced on the frog by cold, with the entire recovery that follows, without recalling the simple and analogous process brought to bear on animals by Nature herself. In the process of hybernation Nature not only displays to us the same order of phenomena, but uses the same means for the production of the inertia—viz., cold—and the same means—viz., warmth—for the restoration of motion. And as, in nature, some warm-blooded animals are put to sleep by cold (the dormouse, for example) so in these we may predict that art will be able to bring about the same condition. In fact, in a bird, as we saw at last lecture, the thing was done. It will be an interesting study for some future comparative anatomist to discover why animals that hybernate come so readily under the influence of cold. There must either be some peculiarity of construction, which enables the cold to reach and act upon their nervous centres, which special activity, or the nervous centres themselves, must be specially susceptible to the influence of cold.

Practical Inferences.—The observations which these researches on the influence of extreme cold on nervous functions called forth are so varied and so rich in suggestion, it is difficult for the mind rapidly to take them in all their fullness. Vasco Nunez, standing on the height of the Darien, and exclaiming, as he saw for the first time the Pacific Ocean, "The sea! the sea!" could hardly have cause for wonder more. We are brought, as it were, to confines of reality, where before there was obscurity only and doubt and dream. We feel that in time the physiologist will pass by the poet, and, as he proceeds in discovery, will place fact in that high place which, as yet, in the history of the world, fancy has claimed as her especial domain.

Such few points of practical moment as I may on this occasion bring and string together are, of necessity, as minute as the time in which they have been studied has been short. Still they will awaken the attention, as I hope, of many, and fascinate a few.

Influence of the Conditions of the Water of Nervous Matter on Nervous Functions.—The first subject to which I would invite your attention has reference to the *influence of water, under* varying conditions, on nervous function.

You will see by the table before us that water forms above 80 per cent. of nervous structure; albumen forms 7 per cent.; the remainder is fatty matter and salts—mainly phosphatic salts. It seems to me, from the experiments which I have conducted, that all the phenomena of disturbed natural function we have seen depend chiefly on modification in the physical condition of the water; on the transference of the water from the fluid to the solid state. In freezing nerve-matter we take from the water its heat of fluidity, or the force which, holding its molecules apart and giving them motion, supplied the condition for that mobile and active state which is the fluid state of water. We have crystallised the water of the nervous matter in all the cases in which we have frozen it. We have reduced it by this means from activity towards inertia; therewith we have deprived the structure of the power to maintain what is called life. The proof that upon the water having undergone solidification the principal phenomena rest, is proved by the fact that when the crystallised frog is warmed again, when it is allowed to take up heat from the surrounding air, it recovers its powers. In these animals the tissues are so thin, and such fair conductors of heat, that the heat can immediately act upon the nerve-substance, and can set the solid mass at liberty. In this respect frogs and animals, constituted as they are, differ from warm-blooded animals. Warm-blooded animals, evolving more heat from within and requiring more heat, are usually protected naturally with non-conducting skin and fur, or feathers, so that their heat is sustained by being retained in their bodies or conserved. But, because they are thus protected from undue loss of heat, they are also rendered incapable of directly taking up heat in sufficient degree to restore instantly the heat of fluidity of their fluids and tissues when that is withdrawn. In chilling their nervous centres, therefore, we have to be most careful so to limit our operation as not absolutely to stop the respiration and circulation. We may render them insensible in the profoundest degree; we may bring them so nearly to death that they seem dead, and if we permit the merest remains of their fire-producing apparatus to continue in play they will recover. But it is essential that we let the fire remain alight. In hybernating animals, during hybernation, this is the secret of their recovery—the fire never goes quite out.

In speaking of the crystallisation of nerve-matter by cold, I have ventured to insist firstly and chiefly on the solidification of the water; but in nerve-substance there is also a considerable per cent. of fatty matter, which when heated is fluid, like water, and which also, like water, loses its heat of fluidity, crystallises, and becomes solid by cold. When, then, we freeze the brain, we solidify this fat also, and, what is more, we solidify it at a temperature at least thirty degrees higher than the freezing point of the water; and as this fat, solidifying first, becomes a bad conductor, so it impedes and limits the freezing of the whole mass of nerve-substance. In hybernating animals I should think the fatty part of the brain and cord is intensely solidified by the cold.

This form of crystallisation of nerve-matter, whether of water or of fat, is simple; it is, however, not the only form of solidification that can be induced. We can solidify nerve-matter so as to destroy its function by two other means at the least—viz., by withdrawing the water from the albuminous constituent of nerve, and by altering the constitution of the albumen by heat. I can illustrate this subject by a very simple experiment. In this beaker there is a solution physically the same in composition as brain-matter; it is in a state of fluidity. I take up a little of this fluid with a soft brush, and paint a surface of glass plate with it. I now, on the opposite side of the plate, project rhigolene spray, producing intense cold. You see the fluid becomes white and solid, and adherent to the glass. It is crystallised and solidified. I take another portion of the same fluid, paint another glass plate with it, and this time, instead of using cold, I drop on a little absolute alcohol. The alcohol as it intermingles seizes the water with evolution of heat, and again there is whitening and solidification of the fluid, with adhesion to the glass. In this case, the molecular condition being changed, the albumen is precipitated, and assumes, with the water, the solid *state. On a third plate of* glass I put a little more of the fluid, and then

gently apply heat; once, again, observe the same objective phenomena—whitening, solidification, and adhesion to the plate. Here, again, it is the albumen that has undergone change; it, with the water diffused through it, has produced a solid mass—a condition also akin to crystallisation. If I varied this experiment by directing the same processes to living nervous-matter, I should induce similar series of phenomena on the nerve-substance; and in so far as alteration in the function of the nerve is concerned, I should produce disorder of function in every case identical. One difference—and it is an essential difference in some respects—would be presented, and it is this: If the nerve were solidified by cold, it would quickly regain its function under the influence of heat; if the nerve were solidified by alcohol, it would slowly and imperfectly regain in its function; while if the nerve were solidified by heat it would, according to our present knowledge, be destroyed in regard to function altogether.

We cannot leave this part of our subject without being led to the contemplation of the influence of other and more refined agencies acting upon the cerebral and spinal matter. What is that change in nerve produced by a blow or communication of great mechanical force: nervous shock? What is the change produced by pressure? What is the nature of the change induced by loss of blood, by choleraic flux, by the heat of fevers, by what are called powerful odours, by strong external impressions—alarms—acting on the brain? When we see how the molecular condition of nerve can be transformed and the function of the injured part modified by cold and by heat, by abstracting and intensifying force, we gain, I think, a physical reading of these other influences as yet, of course, obscure, but still with light stealing into the obscurity.

Caloric the Form of Force in Nervous Matter.—A point which calls for special remark relates to the nature of the force with which the nervous matter is supplied. This subject has occupied the minds of the first of the thinkers of the world, and I know of no subject more absorbing or more wonderful. The experiments of Galvani and Aldini on the influence of electrical action on muscular motion through nerve, led, in the early part of this century, nearly all the physiological world to the belief that in the natural nervous system electrical force is developed, and that the nerve cords from the centres are the veritable conductors of electric currents. To this day the same view has been maintained with much persistency, and various analogies have been set up between brain force and that form of electrical force called galvanism—analogies, as I humbly think, having no real foundation in fact. In the year 1860, in the course of Lettsomian lectures "On Certain of the Phenomena of Life," I ventured to oppose some of these views, and to maintain that in the animal body there was no arrangement for the generation or liberation of any variety of force except caloric. I knew then none of the very singular experiments with cold which we have seen at the last as well as at the former lecture, but I reasoned that there was only one process in the organism for the production of animal force, and that this force, generated, or rather set free, in the combustion of blood, was an active force in every structure. Proceeding from the general to the particular, I laid down as a proposition that caloric is the primary cause of motion in nature, and is therefore a primary cause of life, in so far as motion represents life. To illustrate the position thus assumed, I was at that time compelled to resort to less direct modes of illustration than at this moment are necessary. I looked at the sun as, by the universal voice of mankind, the prime source of all living power, and I contended that the force by which he manifests himself throughout the living world is the force which in its effects, as sensible effects, we call heat, and which in all its characters, whether sensible, latent, or specific, we include under the more comprehensive term *caloric*.

I said that every observation we make in our natural lives leads to these inferences. We measure the amount of life, animal or vegetable, throughout the universe; and we learn that the proportion of life and of motion goes with the heating power of the central luminary. From the extreme pole to the burning equator, the eye glances in the imagination of absolute

knowledge; and as each degree passes under review, from the sterile centres of thick-ribbed ice to the intensified summers of the tropical gardens, we see the gradational life increasing until it reaches the maximum of luxuriance, and that stage of rapid growth and decay, when life and death play into each other with such force, that the one is, I had almost said, lost in the other; and viewing the scene, philosopher and simplest observer each says, "these are the effects of heat." We turn from this grand panoramic picture to our own smaller doings, and we find ourselves instinctively summoning into play a force to supplement nature—our designs are successful. We bathe our bodies in the rays of the fire when the sun retires, and we live. We wish our flowers to grow, and our fruits to run into maturity before their seasons, that our tastes may be satisfied; we make a furnace in our gardens, and our object is gained; we know here that the sensible caloric, the heat we have elicited, is the supplement of the sun. We enter into the laboratory, and we desire there to tear matter into fragments, or to conjoin materials which before were fragmentary. We dissipate solids into gases, and weld the fragmentary metal into a huge mass, an arch for a bridge, or a battery for war; and again we do it all (for we could not move a step otherwise), we do it all by the agency of caloric. We diffuse this agent through expansible water, and give to every cubic inch the dimensions of a cubic foot; the full expansion gained, we extract this expanding agent, condense the water to its original size, and by the alternation generate motion, thus imitating the great winds which, expanded in the equator, condense towards the pole, and in perpetual circuit make their way; and thus likewise imitating the gentle rain, which distilled from the earth into the colder regions of space, condenses there and returns to its resting place in liquid drops.

We thus see in this caloric a great agent, possessing all the attributes by which the varying and yet universal properties of all matter are developed.

It is observable that in natural thought we never dream of attributing the changes noticed to any other force than caloric. It is only when we enter into philosophical subtleties that we begin to hear of other and correlative forces, as primary to caloric or substitutional for it. When, again, we turn to these profounder though probably not safer contemplations, we learn with equal sense of truth that, after all, caloric is the primary force—the first and last in action, and the universal in presence and production.

Carrying out the argument further in its especial application to living beings, I added what follows:—

"Limbs are flexible and moveable at 96 degrees of heat. If this temperature be reduced a few degrees, the muscles become painfully contracted; if the reduction be made further, the muscles become rigid; but they are relaxed by gentle warmth, and the motion is restored.

"In the animal world, I see different animals of different degrees of motive power and endurance. Examining their construction, I find as an unvarying law that the strength or the power of motion is in exact relationship to the power of the animal to eliminate and apply caloric.

"I see an animal at rest, and I notice that he converts a certain weight of carbon into carbonic acid, and liberates a certain proportion of water. I see this animal in active motion—a horse galloping with his rider, every muscle in full action; and now he pours out a greater measure of carbonic acid and volumes of visible vapour of water. Between the steam evolved by this animal and the steam evolved by a locomotive, will any one define to me the difference in relation either to cause or to effect? I mean, of course, in relation to the physical differences; for with the metaphysical I have nothing to do.

"In so far, then, as motion represents life, caloric is the source of living motion. It may undergo modifications in character; now being latent, now sensible; now being rapidly conducted through metals, or other conducting media; now rapidly evolved in series of concentrate sparks. We may call it in these varied forms by other names—*electrical force*—*galvanic force*—but it is the alpha and omega of them all—the principle of motion."

Lastly, in connexion with this universality of distribution of animal force,

I inferred its unity in the animal organs, conceiving the active healthy living structures to be all stored, as it were, with force. Thus I showed "that, whenever muscular action is called into play by interference with the nervous cords, whether in the branches or in the spinal trunk itself, the excitation produces its effects by interference with the nervous column at the part irritated; not by propagation of a current through the nerve-trunk leading from the excited part to the deranged muscle." For instance, when I passed a continuous current through a portion of the spinal cord of an animal, I for the time produced a paralysis of the muscles which immediately communicate with that section of the cord; but that this is not at effect produced by a transmitted current to the muscles was proved by the fact that, by galvanising the nerves between the cord and the muscles, I could cause contraction of these muscles.

According to the view I then held, I also inferred from all the phenomena observed that the nervous system is in every part a producer of the peculiar force with which it is endowed; not that the brain or ganglia are special producers; not that a current from these centres, intermittent or continuous is traversing the nerve-fibre; but that the nerve-structure, so long as it is supplied with blood, is producing the force wherever there is nervous filament. I looked on the vast area of nerve-fibre in the peripheral surface, and I saw in it a mass equal to that of the brain; I saw this mass supplied with blood everywhere, and built always on the same plan. I assigned to it everywhere the same purpose and labour.

In this way I was led to look on the muscular system as an entire independency, and on the nervous system also as an entire independency. The muscular system, nourished by blood and charged with caloric as caloric; the nervous system nourished everywhere by blood, and charged also with caloric in its electrical modification;—each independent systems. We conjoin the systems, and the result of their equilibrium is a simple passive state, while the result of a disturbance of their equilibrium is motion and sensation.

"Thus," I reasoned, "as every portion of nerve down to the minutest branch possesses producing power, the mass of the force generated is so universally distributed, that interference in any part of the nervous communications is reflected to the whole nervous system. So when our distinguished brother, Dr. Brown Séquard, produces artificial epilepsy, and induces the paroxysm by irritation of some particular external point of nerve, he does, in fact, in that irritation touch at one presenting point the universal fluid pervading the whole body of his subject, and excites, not by special transmission, but by general disturbance of the equilibrium of the forces, a convulsion through the whole muscular organism. So, when with the intermittent current I galvanise a portion of the nervous tract, I produce convulsion, because I induce an alternation of force; at one moment allowing the natural equilibrium to establish itself; at the next moment disturbing it. So, when I continue the current without intermission, I virtually cut off altogether the included nervous tract from its system and cause paralysis of will, because I have cut off also communication with the brain; but I can, nevertheless, call into play at pleasure the excitability of the nerve-trunks below, as long as they continue to summon into their service blood for their nourishment and force-producing faculty.

"If it were possible to entirely remove from the body every muscular fibre, and leaving the nervous system entire, still to supply that system with blood and surround it with those conditions under which its blood could be applied, that nervous system would exist as a motionless intelligence. It might think, feel, and by virtue of its sensual organs appreciate and know the external world surrounding it; yet be incapable alike of act or of expression. On the other hand, if every particle of nerve-matter could be removed, the muscular system being left with its attachments to bone still secure, and its blood-current free; that muscular system would remain an unintelligential mechanism, having in itself its *vis insita*, but being incapable of exerting

movement until brought into action and guided by the intelligential part of a more perfect animal.

"By the combination of the two systems in the perfect organism we obtain, so long as the necessary conditions for life are supplied, the doubly endowed and self-acting body. An excitation of light refracted on the nervous expanse of the retina touches the pervading forces, and the animal sees; but this light must be presented to the nervous expanse, or in other words, to the force that pervades the expanse, in such a way that the absolute physical picture shall be put upon it, or the picture will not be seen. It is not that the picture is to be carried to the brain, but that it is to be looked on at this point of the nervous expanse by the presiding force. A vibration is set up in a mere physical membrane, spread above other distribution of nerves, and the animal hears; it is not that anything is conveyed specially to the brain, but that the equilibrium of the pervading force is disturbed. An impression is made on the skin, and the animal feels; it is not that any current is conveyed to the brain, but that the impression disturbs the balance of the nerve-fluid throughout its universality. The impression made is slight, and it is pleasant, or not painful; it is severe, and it excites the whole animal body, so that the body writhes in agony, and may even die from the reflection of the impression upon the muscular fibre, and the resultant spasm."

The views expressed above, at the time when they were first enunciated, were based confessedly on general inferences. They were not sufficiently exact to satisfy the demands of strict science, and for that reason they were never unduly pressed on the attention of the profession. At the same time they were, in the main, correct, and now, somewhat simplified, they stand firmly, I believe, on fair and rigid experiment.

For what are the facts which have here experimentally been brought to our notice? We have had before us a frog; we deprived it of all sensibility; we deprived it of all power of motion. How did we effect this? Simply and solely by the abstraction of the heat from its cerebro-spinal system. When the animal was thus reduced to inertia—when to all appearances it was dead—we saw its life fully restored. In what manner, under what condition, did this restoration become pronounced? There was only one condition—restoration of caloric. When we put these two great facts together, and when we couple them with the further fact that the animal has no means for producing any force except calorific force, we cannot. I think, escape the conclusion that animal force is caloric. With this we may admit—indeed, we must admit—that other correlatives, when brought to bear on the living body, can stir into temporary motion the organic structures; we must admit that light can set up motion from the eye to the brain; that the passage of an electric current can set up motion in muscle, and motion from any periphery of nerve to brain, that the vibration of air can convey motion by the ear to the sensorium, and that the motion of minute particles of organic matter may be communicated through the olfactory nerves back to the centres of those nerves. But these admissions do not interfere with the general truth that caloric is *the* force of life—that it is true and sufficient to explain all the phenomena of living motion.

Before this curious experiment of producing inertia in a living animal by abstracting the caloric from its nervous centres, is allowed to pass from our thoughts, let me make one more reflection respecting it. The inert animal is either asleep, or it is dead. The terms sleep and death are, I know, relative, but as we must be guided by them, and as we know the animal can be restored, we are bound to say of it that it is not dead, but that it is as we are, when we are shut out, for a time, from the world—asleep. We let back then the force that has been taken away; as the force goes back, it loosens the hardened tissues; it enters the structures; it enters the nervous centres, it recharges them, and thereupon there is renewed motion. We learn, thus, that when these nerve-centres are reduced in force there is a powerless *organism; we learn* that so reduced the centres can again take up and again *lay up force; we gather* the truth that when the centres are once more re-

plete with force they communicate the fact to the rest of the organs, and that the body therewith awakes. Of what is this the counterpart? I think it to be the counterpart of natural sleep. I take it that during sleep the exhausted brain, the exhausted nerve-cord, the exhausted nervous system, everywhere takes up and stores up caloric, and so continues to take up and store up until it is charged to its full capacity. Then, if I may use such expression, it overflows with force, it spontaneously fills the body, and there is presented that phenomenon of motion which we call "the awakening." Send other force, vibration from noise or mechanical motion, at any moment through a sleeping body, and you may, through a dynamic act, excite motion, and the body may awake; but by this you have not primed the body with the force it wants for sustained work; you passed a charge of force through it, but you did not charge it. So when my unwound watch is ceasing, I can stimulate it into movement for a moment or so by a moderate blow or shake; but the force is applied uselessly if the mainspring be not recharged.

The phenomenon of sleep and the phenomenon of awaking from sleep appear thus to me to have a simple and a rational explanation—an explanation purely physical in character, and which may be applied to the correct exposition of many of the phenomena of disease as well as of health.—*Medical Press and Circular, June* 19, 1867, *p.* 575, *and Med. Times and Gazette, July* 20 *and Aug.* 3, 1867, *pp.* 57, 113.

13.—ON A NEW METHOD OF TREATING PARALYSIS BY THE APPLICATION OF COLD OR HEAT ALONG THE SPINE.

By Dr. JOHN CHAPMAN, Physician to the Farringdon General Dispensary.

The term paralysis denotes in reality various diseases, diseases which differ alike from each other in respect to their phenomena, the parts of the nervous system in which they are seated, and the nature of the morbid condition which obtains. In fact, they differ from each other so decidedly that their only common element consists in disorder of some part of the nervous system. disorder most frequently, however, extending to the voluntary or involuntary muscles, or to both, at the same time. The morbid states of the nervous system are also due to widely different, and not seldom indeed to quite opposite, proximate causes—as, for example, anæmia, hyperæmia, congestion, inflammation, red softening, white softening, hemorrhage into the nervous centres, pressure of tumours, tubercle, cancer, and finally, blood-poisoning—as in cases of syphilis, uræmia, plumbism, and the eating of bread containing the ergot of rye. There are also the cases of paralysis resulting from mechanical injuries, and finally the progressive paralysis of the insane. In fact, it seems to me that the nomenclature of so-called paralytic diseases is extremely defective; for at present it is customary to designate morbid states of the muscular system which are exactly opposed to each other in nature by one and the same word. The flaccidity and atrophy characteristic of the muscles in cases of wasting palsy, present a complete contrast to the rigidity and comparatively undiminished bulk of the muscles in other forms of motor paralysis scarcely less notable.

[The writer proceeds to relate a case of paralysis involving the muscles of the tongue, throat, back, and four extremities, and associated with giddiness, headache, profuse dribbling of saliva, constipation, and scantiness of the menses. The patient was a married woman, 42 years of age, and described as "a thin, nervous, and intelligent person."]

When first prescribing for her, I ordered aperient pills to be taken occasionally at bed-time, quinine and sulphuric-acid twice a-day, and requested that the shoulders, arms, back, and legs should be rubbed vigorously as often as practicable, and that towels dipped in iced-water, and so folded lengthwise as to be about four inches broad, should be applied along the whole spine during twenty minutes, three times a-day—the towels being carefully changed *immediately they* became warm. This report would be-

come inconveniently long were I to give a detailed account of the treatment I adopted during the time the patient was under my care. I shall therefore restrict my description of it to a general statement of it as follows:—Besides the medicines just named, the only others used were calumba, citrate of iron and quinine, nitric acid, an aperient draught, and bicarbonate of potash. Galvanism was applied to the muscles—chiefly of the extremities, and was used for the first time, May 15, 1863. I also applied heat freely, by means of hot water, to the extremities; but the main treatment, and that upon which I principally depended, consisted in lowering the temperature of the spinal region, by means sometimes of iced-water, sometimes of the spinal ice-bag. The applications were, at one time, along the whole spine, at another to a part, and for different lengths of time, the variations being made according to indications observable from time to time.

Results of Treatment.—The patient showed indisputable signs of improvement within the first week of treatment, which was continued upwards of two months, during which she progressed most encouragingly. Up to the end of that time the change effected in her may be summed up as follows:—She could hold her head erect; could protrude the tip of the tongue beyond the teeth full half an inch; the dribbling of saliva quite ceased; she could speak much more distinctly; could swallow fluids without difficulty, and drink a cupful of tea or coffee at a draft without becoming choked, or spilling any of it; she could raise the right arm vertically—placing the elbow higher than the head, and the left arm so that the tips of the fingers reached as high as the top of the head; the arms increased in size,—during the first eighteen days of May the diameter of the middle of each forearm increased a quarter of an inch; and the power of each arm was correspondingly increased. Even as early as the 18th of April she said:—"My arms are wonderfully much stronger. I can feed myself; can pull myself up, and can wipe my face," &c., and with joyous pride showed me that she could lift a chair—a feat she could not perform the previous week. The claw-like set of the fingers disappeared. On the afternoon of Sunday, May 3rd, the patient experienced a sudden increase of power in the extensor muscles of the left forearm, where, for the first time, I could distinctly feel their contractions beneath my fingers; and the pulling and grasping power became, during treatment, greater than before. The concavities formed by the wasting of the ball of each thumb were in no perceptible degree filled up; there was, however, a slight though decided increase of motility of each thumb, and the left flexor longus pollicis became markedly stronger, notwithstanding that the concavities just named continued. The metacarpal interosseous spaces of each hand were gradually filled up to a surprising extent, so much so, indeed, that her hands became quite plump. The power of flexing the thighs on the trunk and of separating them from each other gradually returned, so that instead of slipping down from her chair continually, as she had formerly done, she was able to maintain her position, and, finally, to move her thighs about freely in all directions. The feet gradually became susceptible of passive motion, and afterwards she was capable of putting both feet flat to the ground, and of moving them in different directions—outwards as well as inwards—by the mere force of her will. During the latter end of May she stood occasionally for two or three hours at a time, supporting herself sometimes only with one hand, and one day, with the help of her son, she walked into an adjoining room. The muscles of the back, with the exception of a few of the upper fibres of the trapezius, ceased to quiver, and re-acquired a very considerable development. In fact, she gained flesh generally to such an extent as to cause her nurse to complain, during the latter portion of her attendance, of the great additional weight of the patient when moving her about. This increase of flesh was even more remarkable in the face than in the rest of the body, so that, compared with what she had been, she appeared quite plump. The back became permanently much cooler than it had been; *the function* of the bowels healthier; the catamenia abundant; appetite *greatly improved; the head* clear, and not only free from headache, but free

from the great giddiness which, up to the time when my treatment began, she had experienced during the whole period of her illness.

At the end of May pecuniary and domestic troubles which had long pressed heavily upon her reached a crisis, which exerted such a depressing influence upon her as not only to stop further progress, but actually to throw her backwards. During the long period of her illness her husband, who is a working man, had been obliged to pay some one to do the house-work and to attend on her, and having become involved in debt he now sent away the person who had done this work, and broke up his house. His wife was then taken to her relations, at her native place in the country, and I never saw her again.

It would be difficult, indeed almost impossible, for me to select a case more suitable than this to exemplify the efficacy of the therapeutical method, which consists in modifying the temperature of the spinal region; for this patient presented a combination of symptoms, each of which may be fairly regarded as a grave disease, and each of which was influenced beneficially and to a remarkable extent; there was paralysis with flaccidity and wasting of the affected muscles; there was also paralysis with rigidity of the affected muscles; there was headache, with giddiness, especially when the patient stood up; there was profuse dribbling of saliva; there was loss of appetite; habitual and extreme constipation; and the menses were very scanty. Now it appears to me that a skilful interpretation of these various symptoms would lead to the inference, that the spinal and collateral sympathetic centres were in that morbid condition, which indicated that the application of cold would exert a remedial influence. The history of the case seems to show that the disease originated by injury to the hand, that the morbid condition induced by the accident resulted in a transmission of a morbid influence to the spinal cord; and that this influence was diffused both along the cord, and over the sympathetic nervous centres. As might be expected, the disease was most intense in that region of the cord where it was first generated; so intense, indeed, that probably portions of the cellular constituents of that part ultimately became more or less atrophied or destroyed. Simultaneously the collateral sympathetic ganglia were kept in a state of morbid excitement by the influence of the disease in the cord, and thus exerted an excessively constringing force on the arteries under their control. In this manner, I account for the particular form of paralysis with which the muscles of the throat, neck, upper extremities, and dorsal region, were affected. While, on the one hand, that healthy influence dominating the nutritive processes which is diffused from the cerebro-spinal system throughout the organism was, in respect to the parts in question, greatly diminished and probably also perverted, and while the power of transmitting motor impulses to the muscles implicated was impaired, the supply of blood to these muscles was so greatly lessened, owing to the chronic spasm of the arteries distributed to them, that it was no longer possible for them to receive adequate nourishment. The conditions here described are precisely those which are calculated to insure muscular atrophy; and, I venture to suggest, that that peculiar feature which frequently distinguishes wasting palsy, viz.,—the wasting of special groups of muscles, and groups, moreover, which are often related to each other by co-ordinate action, originates in the fact that special series of nerve cells occupying limited areas within the spinal cord, and functionally related to the group or groups of muscles implicated, are diseased, the rest of the cord remaining healthy, or at least disordered only to the extent of hyperæmia. In this case the result of the diffusion of the morbid influence down the cord and to the collateral sympathetic centres was, as it seems to me, the production in those parts of hyperæmia, so considerable as, by greatly intensifying their functional energy, to induce a tetanoid condition of the lower extremities; the energy of the spinal cord transmitted to them was continuously excessive, thus inducing in them a state of continuous contraction. Simultaneously the stimulus diffused from the sympathetic ganglia over the arteries of the lower extremities, so far exceeded the normal amount, as to lessen in *some degree* the supply of blood to the muscles, and

thus insure the condition which, as is well known, is peculiarly favourable to the onset and continuance of spasm or muscular rigidity. On the other hand, while the nervous centres related to the arteries in question were so far unduly active as to produce the condition just described, they were not acting with that intense degree of morbid energy which would result in almost wholly cutting off the blood supply, and which, as stated, was the case with respect to the nervous centres related to the upper part of the body. Hence, while the muscles of that part were extremely wasted, some of them being almost wholly destroyed, the muscles of the lower extremities were not diminished to any considerable extent.

If it be admitted, as indeed it must be by all who have experience in this matter, that the cerebral blood-vessels are under the government of nerves emerging from the sympathetic within the region limited above by the fifth or sixth cervical vertebra, and below by the the third or fourth dorsal, and if it be further admitted that in the case in question the sympathetic centres within this region were morbidly affected, we should be led to predict that the cerebral circulation would become disordered, and that the disorder itself would present the form of cerebral anæmia. This, in fact, was the case; there was headache, associated with feeble mental action, and frequent giddiness, especially when the patient stood up.

Seeing that there were symptoms of grave disease of the spinal cord in that region where the nerves to the upper extremities are given off, it is easily intelligible how the uppermost part of the cord, together with the medulla oblongata, would, through the propagation of the morbid influence upwards, become hyperæmic. Now the nerves which cause the parotid and submaxillary glands to pour out their appropriate secretion, are motor-nerves from the cerebro-spinal system, viz., branches of the auriculo-temporal and the chorda-tympani, and the origin of these nerves is the medulla oblongata. These facts being recognised, it will be at once seen that hyperæmia of that nervous centre is likely to result in an excessive flow of saliva; and such in fact was the case with the patient in question.

Extensive experience has taught me that an abnormally irritable state of the spinal cord and collateral sympathetic ganglia along the lower half of the dorsal region, is peculiarly conducive to functional disorders of the stomach—loss of appetite, indigestion, nausea, vomiting. And I have long been of opinion, founded on many observations, that one at least of the immediate causes of nausea is a great superabundance of mucus in the stomach, owing to the excessive stimulus transmitted from the spinal cord to the mucous glands of that organ. Assuming my diagnosis of the case under consideration to be correct, it follows that there was hyperæmia of the whole of the spinal cord, below as well as above the part first diseased, and therefore an intelligible cause of the symptom—loss of appetite; and, as I shall now proceed to show, of the extreme and habitual constipation from which the patient suffered.

If my ideas concerning the pathology of constipation be correct, it may proceed from three causes—1st, from partial atrophy of the circular muscles of the intestines; 2d, from inadequate nervous stimulus of those muscles; 3d, from a deficiency of intestinal mucus. Now, as I contend (and I must content myself for the present with merely stating my conviction), both the muscles and the mucous glands derive their functional energy from the spinal cord; if so, the second and third causes just named of constipation, consist in enfeeblement of that part of the cord related to the intestines. This condition is far less frequent than that of excessive spinal irritability, the result of spinal hyperæmia. It follows, therefore, that constipation due to spinal anæmia, and therefore enfeeblement, is comparatively rare. On the other hand, cases very frequently come before me, in which obstinate and habitual constipation is associated with indubitable symptoms of hyperæmia of the nervous centres along the lower dorsal region. In these cases the production of constipation is, I apprehend, as follows:—The nervous energy diffused over the mesenteric arteries being excessive and continuous, forces *them into a state of chronic* contraction, many of the smallest ones being

often, I doubt not, closed entirely. The consequence is, that the intestinal muscles cease to be adequately nourished, and therefore, losing their wonted strength, become incapable of effecting those vigorous vermicular contractions on which the power of the alimentary tube to propel its contents with healthy force and regularity depends. It is conceivable, and indeed probable, that when, owing to chronic contractions of the mesenteric arteries, arterial blood is partially shut off from the intestinal walls, the mucous glands will receive less than their due supply, and that therefore, while those glands are being adequately stimulated to fulfil their functions, they are incapable, from want of blood, of elaborating a sufficiency of mucus; if so, two distinct causes may co-operate in producing the constipation in question. If these views be sound, we should be justified in predicting that, in the case just described, the character of the paralysis would be associated, as it was, with persistent constipation.

The last symptom to which I have to advert was scanty menstruation. Reasons have already been given for believing that the nervous centres in the lumbar region, as well as those above, were hyperæmic; and I have demonstrated by a large amount of evidence that, if the nervous centres along the lower half the spine are rendered hyperæmic by the application of heat, the menstrual function may be retarded, lessened, arrested, and, in some cases, at all events, completely suppressed. It will therefore be readily understood how the special menstrual disorder of this patient was one of the results of that general disease of the nervous system from which she suffered.

In seeking to understand the nature of things, it behoves us, as has been well said by Newton, to admit only such causes as are at once true and adequate to explain the phenomena in question. In what I have said concerning the foregoing case, I have alleged that one morbid state of varying degrees of intensity in various parts of the spinal cord and sympathetic was adequate to produce all the symptoms passed in review, and have attempted to explain, *seriatim*, the *modus operandi* of their production.

Of course, if my explanation of the proximate cause of the symptoms be correct, it follows that the most scientific method of removing that cause would consist in exercising a sedative influence on the nervous centres morbidly affected, and on no others. My treatment of the patient was the result of this reasoning, and considering how signally several other attempts to relieve her had failed, and the desperate state in which I found her, the benefit conferred is a striking testimony to the efficacy of the plan adopted. The medicines already mentioned, which were administered, played, I apprehend, a very subordinate part in the remedial process; the friction which I ordered to be applied to the wasted muscles with a view of facilitating an increase of their nutrition, as well as the galvanism which was applied chiefly to the lower extremities, probably co-operated more effectually; but my experience in many other cases enables me to state confidently, that the great improvement which was effected in this patient was mainly due to the application of cold along the spine.

[The next case was one of paralysis (hemiplegia and paraplegia) associated with epilepsy and inducing great impairment of speech, extreme incontinence of urine, excessive drowsiness during the day-time, and extraordinary coldness of the surface of the body, especially of the extremities, even in summer. The patient was a man 56 years of age. When he was 30 years old he had a severe epileptic fit, but there was no recurrence until he was 40 years old, when he complained of a feeling (pressure) in his head and immediately fell; he was much convulsed, and bit his tongue. Soon after this, without losing consciousness, he became nearly paralysed on the right side of his body, the speech being also affected. This was in great measure recovered from, but not completely, and again became worse when he had another attack in his 53rd year, at which period he came under Dr. Chapman's care.]

I ordered ice to be applied along the whole spine during two hours, five times during each twenty-four hours, and requested that the spinal ice-bag

should be enclosed in flannel; I also prescribed five grains of bromide of ammonium twice a-day.

June 18. Has had no fit, and does not feel likely to have one. His head feels better, feels lighter; "he don't feel," his wife says, "that heaviness that he did." Sleeps well at night and less in the day-time than before. Has not complained at all this week of the numbness in his hands; his wife says they are decidedly better. The weakness of the legs continues, and is, she thinks, greater than last week. The bowels have already become "very regular, once a day, and sometimes twice," since he began to use the ice. He retains his urine longer than formerly. Feels warm all over—the feet being especially warm.

25th. No fit; he says the "head feels very well; feels clear and cool I don't seem to have any fear of a fit;" the speech is clearer and more distinct; the sensibility on each side of the face is now equal; he feels two points at three lines apart round the angle of the mouth on each side; the right arm has decidedly gained in strength; and the sensibility of both hands has become normal; he now feels two points at four lines apart in each palm; the lower extremities are also stronger; he can now turn the left toes outwards; bowels regular; he notices a great diminution in the frequency of making water; continues constantly warm all over; to apply the ice as last ordered four times a day instead of five, and to continue the bromide of ammonium.

July 2nd. No fit; sleeps well at night, but *much less* in the day time than he used to do; bowels regular. He now makes water about four times during the night, and six during the day. To apply ice as before three times a day instead of four, and to enclose the spinal ice-bag in a thick worsted stocking instead of in flannel as before. ℞. Quinæ disulphatis, gr. i.; acidi sulphurici diluti, ℥vii. bis die.

9th. No fit; no headache; feels himself "as well as possible, but very hot—very hot;" his speech is greatly improved; his mind also; the mental improvement is clearly expressed in his face, which is lighter and more intelligent; sleeps "capital" at night, still less during the day; feels no numbness whatever of the hands and no sensation as of "pins and needles;" can still evert the left foot, and can now stand on each leg, but especially on the left one; he can also now walk alone; bowels regular; power of containing the urine continues to increase; now passes it about four times in the day and four times in the night. To apply ice twice daily, in three folds of worsted, and to continue the medicine as before.

16th. No fit. Reports himself still better. His wife says "he does not sleep in the day like he used to do, and sleeps very well of a night." He talks still more clearly. Retains his water still better, getting up only twice, or at most three times during the night. He can now sometimes walk very well, even without the aid of a stick. To continue as last ordered.

23rd. Has had no fit, but has just fallen, having being pulled down by his wife in her attempt to get him out of the way of a carriage, after getting out of an omnibus. He is shaken by the fall, and complained for a short time of headache afterwards. Otherwise is in all respects the same as last week. Treatment same as before.

30th. Still no fit, notwithstanding the fall and fright last week. Yesterday, for a short time, the right hand became "rather shaky" and numb; the numbness lasted about half an hour; the shakiness a good part of the afternoon. He was quite well again in the evening. His head remained quite unaffected. The improvement of the bowels and bladder continues. To apply ice during two hours three times a day; the bag being enclosed in one fold of flannel. ℞. Potassi iodidi gr. iij; ammonii bromidi gr. v; ex aquæ ter die.

August 6th. Continues without a fit or other bad symptom. To persist in the treatment last ordered.

September 9th. Had a fit on the 30th ult. at 5·30 a. m. His wife says she believes "it's worry that brought it on." She states that he had ice each *day until* the fit, but none afterwards. *Since he left off the ice he has become cold again.*

A careful consideration of the symptoms of this case, apart even from its history, can scarcely fail to lead to the conclusion that the paralysis of the extremities, the dull heavy headache, the defective speech, the disordered sensibility evinced by the cheeks and hands, the grave functional disease of the bowels and bladder, the excessive somnolency, and remarkable coldness of the surface of the body, are all referrible to excessive hyperæmia, merging into congestion of both the spinal cord and the sympathetic nervous system. According to my reading of the case, there was considerable congestion of the spinal cord, resulting in the paralysis of the lower extremities, and co-operating to produce the disordered sensibility especially evinced in the face and hands, as well as the extreme functional derangements of the bowels and bladder. On the other hand, the vascular condition of the sympathetic ganglia was rather one of extreme hyperæmia than of congestion, hence their preternaturally vigorous action which resulted in such vehement arterial contractions throughout the system as to deprive the brain, as well as the periphery of the body generally, of its due supply of blood. The consequence was, the mental heaviness and extraordinary somnolency and the excessive coldness which the patient experienced. Moreover, other of the symptoms were not less certainly, though less obviously, due to the same cause; the anæsthesia or numbness, and the sensations as of being pricked by "pins and needles," though partly referrible to congestion of the medulla oblongata and spinal cord, were also, I have reason to believe, in a great measure due to the excessive action of the sympathetic, which, lessening the supply of blood to the parts affected, prevented that complete nutrition and oxidation of the nervous constituents of the tissues of those parts which are essential conditions of normal feeling. The same cause resulted in the starvation of the muscles of the bowels, and, probably, those of the bladder. Hence, according to the explanation already given, the peristaltic energy of the bowels was impaired, and hence, probably, the muscular coat of the bladder, being also weakened, became abnormally irritable in accordance with the well-established fact, that badly-nourished, weakly muscles are most excitable.

Assuming this pathological view of the case, it is obvious that no treatment of it was calculated to prove so beneficial as that which was capable of abolishing at once the hyperæmia and congestion of the spinal cord and sympathetic; and as the case was one of many years' standing, and therefore presenting, probably, extreme dilatation of the blood-vessels of the nervous centres in question, I ordered the prolonged use of the spinal ice-bag each day in the first instance, in order the more effectually to remedy the morbid condition in question, and, at the same time, to prevent the possibility of reaction.

It is extremely to be regretted that this patient was not enabled to continue providing himself with ice for a much longer period than that during which my treatment of him lasted; for had the blood-vessels of the nervous centres acted upon been kept vigorously contracted to their normal diameters during an adequately long period—say probably six months, I have reason to believe the patient would have derived greater and more lasting benefit than was actually conferred upon him. It will be admitted, however, that the case as it stands, is an extremely interesting and instructive one, inasmuch as it affords the most decisive proofs of the truth of those pathological and therapeutical doctrines, which I have propounded. Within the space of five weeks, the right arm of the patient became much stronger; he was enabled to walk alone, even without the aid of a stick; to evert the left foot, and to speak very much more distinctly; his intelligence and mental expression were greatly improved; the numbness and other disorders of sensations, as well as his headache and drowsiness in the day time, vanished; he became not only warm all over, but "very hot;" his bowels were opened daily; his power of retaining his water so increased that he was obliged to get up only twice, or at most thrice, during the night; and, finally, whereas before treatment he had an epileptic fit about every month, one did not occur until nearly three months after the treatment began.

The fact that the patient became cold again after leaving off the spinal ice-bag is both interesting and instructive; the chronically contracted arteries were dilated to their normal volumes by the treatment adopted, but not being under its influence sufficiently long to enable the newly reacquired increase of their diameters to become established as a physiological habit, they contracted again when the spinal ice-bag was no longer applied, and hence the patient again became cold.—*Medical Press and Circular, May* 1, 1867, *p.* 409.

14.—ON RHEUMATIC PARALYSIS.

By Harry Lobb, Esq.

Perhaps of all the diseases that come under the treatment of the medical electrician, rheumatic paralysis is the most amenable to the judicious application of electricity. As a rule, ordinary routine medical treatment has no effect upon this malady, and the disease progresses from bad to worse, until the use of the limb or limbs is completely lost. The following case is a most instructive one, and is a fair type of this disease:—

Mr. G. called upon me on the 20th of last March, with a note from Sir Wm. Fergusson, stating that "the bearer will tell you his story; my idea is that electricity may be of service, and I recommend him to you with that view," and giving the following history, in writing, which I give in his own words: "On Saturday, the 2nd of February, 1867, on returning home in the evening, I felt what appeared to me to be a light blow on my right shoulder, which ached slightly from that moment until I got into bed, the pain then increased in the shoulder, neck, and back of the head, and kept me awake nearly all night. From that time, for three weeks, I did not sleep more than two or three hours in the twenty-four. I took medicine to operate on the liver, and also effervescing draughts to act on the skin. Vapour baths, hot-air baths at the hammam; shampooing; rubbing with stimulating oils, and douches were tried, from all of which I experienced temporary relief, but not of a lasting nature. At the end of six or seven weeks nothing but the rudimentary rheumatic pain was left.

"About the 5th of February, I began to feel great weakness in my right arm and shoulder, and difficulty in shaving and dressing. On the 10th I employed a barber, who shaved me every morning. After the 10th of March I virtually lost the use of my deltoid and biceps, but could always write when my arm was lifted and placed on the table. I saw ——, who gave me a liniment of chloroform and camphor, which proved useless. Afterwards he gave me iodide of potassium, with the like result. My arm not improving in the slightest degree, I consulted Sir W. Fergusson on the 20th of March, who sent me the same day to Mr. Lobb."

On examination, I found the deltoid completely paralyzed, the biceps almost so; both much wasted and very soft; the extensor very weak.

Electrical Examination.—The battery I employed is my own Portable Book Electro-Magnetic Battery, made by Ladd. First using the secondary wire, or induced current of electricity, attaching a moist conductor to the positive pole, which I placed upon the spine, I examined the skin covering the affected muscles with a wire-brush attached to the negative pole, using the whole power of the battery.

Sensation was very dull over the deltoid and biceps, and there was no reflex contraction of muscular fibre under the electrical stimulus.

The conductors were now shifted to the primary wire, and two moist conductors were employed. The one from the negative pole was placed beneath the spinous process of the scapula, and the conductor from the positive pole was placed upon the muscle to be stimulated,

On passing the current through the deltoid, faint contractions were discovered *in the anterior* and posterior fibres, but none whatever in the middle

fibres; the biceps contracted feebly; all the other muscles were in tolerable order.

Treatment.—A Pulvermacher band was applied, direct current positive pole on the spine, then wound round the deltoid, and the negative pole placed on the lower part of the biceps, and retained in position by an elastic bandage, to be worn night and day; excited once a-day with one part vinegar, and eight parts water. Daily, for half-an-hour, each muscular fibre of the paralysed muscles to be excited with the inverse current of the primary wire of the electro-magnetic battery.

This treatment, without change, was carried on daily for three weeks, with immediate benefit from the first application, and on the 18th of April I took Mr. G. to Sir William Fergusson completely cured, enabled to use the deltoid and biceps perfectly.

This is a model case. I have had numerous somewhat similar ones; but it must be borne in mind that the longer the paralysis has lasted, and the more, the muscle has wasted, the longer the treatment will be required before a cure can be effected.

Paralysis of the leg, following sciatica, is not uncommon; the treatment is precisely similar, and the result equally satisfactory.—*Medical Press and Circular, June* 12, 1867, *p.* 457.

15.—ON THE TREATMENT OF PARALYSIS FOLLOWING SCIATICA BY MEANS OF ELECTRICITY.

By Harry Lobb, Esq.

[The paralysis following sciatica is very similar to that resulting from rheumatism, and as regards the treatment is precisely the same. The following case is interesting from the rapidity of cure:]

Mr. M., the well-known veterinary surgeon, consulted me last year for a very severe sciatica of the left leg, from which he had been suffering for three months, and for which he could get no relief, the pain of a very acute kind, darting down the sciatic nerve to the foot; he complained that he had not had two hours' consecutive sleep for three months, and that he was nearly worn out with pain and fatigue. He certainly appeared much worn, and had evidently suffered much pain. He was very lame, and the leg was no use to him, as he could not bend it, or bear his weight upon it. The right leg was unaffected, and he managed to get on with this leg and a stick. He had been treated medicinally with alteratives, antacids, &c., and his diet had been properly attended to. He had also tried all the known nostrums, which friends so readily press upon the afflicted as infallible.

Considering the case to be one of pure neuralgia, requiring only the continuous voltaic current to be passed through the nerve in large quantities, to bring it back to its normal state, I at once applied a current from a Pulvermacher's 120 element battery, by applying the positive pole to the lower portion of the spine, by means of a wet conductor, and placing the foot in a basin of water brought it into the circuit with the negative pole. This current was allowed to pass for about twenty minutes, when I immediately put on two No. 0 bands, the positive pole on the lower portion of the spine, and taking a turn round the leg, the negative pole was placed beneath the sole of the foot—the bands being excited with one part vinegar to eight parts water, and retained in position by means of an elastic bandage.

Mr. M. called on me the next day, stating that he had had no pain since the application, and that he had slept the whole night. I, however, again applied the current from the battery and the bands, and as a precautionary measure, continued this for a fortnight, but he had no return of pain, and after a few applications of the interrupted electro-magnetic current, to stimulate to contraction the partially paralysed muscles, he rapidly regained the

use of the limb, and in about a fortnight was perfectly well, and had quite lost his lameness.

Occasionally one sees a case of paralysis of the leg following sciatica of long standing, where the pain had ceased, perhaps, for months, or even years, and where the patient had not recovered the use of the leg. There is generally much wasting of the unused muscles, and some time is required to effect a perfect cure.

The following case came under my notice in my hospital practice, and is worthy of record:—

William Pratt, 5, Buckingham-street, Pimlico—paralysis of the right leg of two years' standing, following sciatica—has still some pain in the leg; the coldness of the limb was remarkable; it had also wasted considerably, and it was stiff and useless.

Treatment.—A Pulvermacher voltaic band to be worn from the back to the foot, wound round the leg, and retained in position by a bandage, the band to be excited every morning with one part vinegar to eight of water, and to be worn day and night. A powerful current from a Pulvermacher battery to be applied, by means of a metallic surface, to the limb, direct current. This at once produced a return of circulation and warmth. By continuing the treatment for a fortnight he was quite cured, and could walk to and from the hospital with comfort and ease.

This is a remarkable case, from the length of time the patient had lost the use of the limb—namely, two years, and from the extent of the paralysis, from the wasting and peculiar coldness of the surface, as also from the rapidity of the cure. It appeared that on inducing circulation in the skin, a reflex effect had been produced upon the muscles through the cord, for a return of power was almost immediate upon a return of warmth to the surface.

I could relate fifty such cases as the above having a like result, but these two are sufficient to point out the nature of the treatment.—*Medical Press and Circular, July* 17, 1867, *p.* 54.

16.—CAPSICUM IN DELIRIUM TREMENS.

Since our last notice of the employment of this simple and efficacious plan of treatment, some well-marked cases have occurred in Dr. Lyons's practice. In one instance the patient, a tavern-waiter, of chronically intemperate habits, was admitted to the Whitworth Hospital in the first stage of this morbid condition. The patient exhibited tremor in almost all the muscles of the body, chilliness, debility, sleeplessness, foul tongue, severe and general uneasiness, but there was a total absence of illusions, horrors, or delirium to any degree. He got a single dose of capsicum, twenty grains in a bolus, after which he slept and fully convalesced, the disease having been thus peremptorily cut short. Dr. Lyons remarks on the great importance of this early phase of the disease being recognized and promptly treated. The patient is in that condition in which he may be by but slight further indiscretion plunged suddenly into all the horrors and moral degradation of the state of fully developed delirium tremens, with all its consequent loss of character with others, and loss to the patient himself of that last barrier against utter abandonment, the sense of shame and remorse. For not alone does the first occurrence of delirium tremens brand the sufferer with the character of an all but irretrievable dipsomaniac, but the fact that he has passed this moral rubicon, in the vast majority of cases deprives the patient of all stimulus to self-control, and under the demoralizing feeling that there is nothing further to be risked, his steps henceforward ever tend downwards and from bad lead on to worse.

As Dr. Lyons observes, a brief but variable period often precedes the fully *developed* attack of delirium tremens, especially in first cases, in which the

patient presents anomalous symptoms unintelligible to himself, and not always read aright by his attendant. This stage is in some patients marked by the occurrence of tremor, sleeplessness, and general distress and anxiety, without a trace of delirium. In other instances slight illusions prevail without tremor, from which the patient can by an effort arouse himself, and under strong self-directed exertion of the will even command his faculties for a time, and pursue avocations of business, to break down, it may be, hopelessly, a few hours subsequently, if his condition is neglected, misunderstood, or mistreated. Under these circumstances the treatment by capsicum comes in very opportunely, and by its employment we may, as in the case just cited, cut short the disease, and so save the patient from the consequences of his imprudence. and possibly restore him to a reformed life. Another case well illustrates the success of this drug when opium had completely failed to alleviate the symptoms, and seemed on the contrary in many respects to aggravate the patient's condition. The case was that of an individual who had taken six grains of opium within a period of two or three days without sleep being procured, or any relief to the illusions, tremor, and distress under which the patient laboured. After a twenty-grain dose of capsicum in bolus, profound and refreshing sleep for twelve hours was induced, and the patient awoke conscious and restored. In an almost precisely similar instance occurring about the same period, a thirty-grain dose of the drug had to be given a second time before full relief was procured. In one or two instances of individuals of confirmed and extremely intemperate habits, it was found necessary to repeat the dose some three or four times.

As to the physiological action of the remedy, Dr. Lyons's explanation is that already given in a former communication—namely, that it produces a powerful stimulant and sedative influence by its direct action on the gastric filaments of the vagi. Slight uneasiness in the stomach has been complained of in one instance only after its use, and in two instances somewhat smart purgation was noticed, but without any evidence of intestinal or other irritation.

As at present employed, the drug is administered in bolus made up with honey of roses; but Dr. Lyons suggests the feasibility of its being conveyed to the stomach in the more agreeable form of a capsule.

As capsicum belongs to the great order of the Solanaceæ, Dr. Lyons suggests the possibility of its containing a narcotic principle hitherto undiscovered. He has referred this question for further elucidation to his distinguished friend, Mons. Gages, curator of the Museum of Irish Industry, a chemist of great eminence.

Christison observes, "Capsicum and cayenne pepper belong to the class of irritant poisons; and the latter preparation has been known to cause death. It is entirely destitute of narcotic properties, so far as is known at present. In both respects it constitutes a singular anomaly in the natural order Solanaceæ, which are generally powerful narcotics, but feebly or not at all acrid."

Pepper (Piperaceæ), probably black pepper, was not unknown to the ancients medicinally. Celsus, it may be mentioned, has a chapter headed: "Curatio horroris in febribus si nec balneum quidem profecit, ante accessiornem allium edat aut bibat calidan aquan cum pipere, siquidem ea quoque assumpta calorem movent qui horrorem non admittunt." Dioscorides also alludes distinctly to the use of pepper in curing the shiver of fever, and in later times Van Swieten and Louis Frank have employed it for a like purpose. Under the form of piperin the active principle of black pepper has been by many practitioners in the present century prescribed in the treatment of fevers, some vaunting its efficacy as not second to that of quinine—*Medical Press and Circular, June* 20, 1866, *p.* 653.

17.—ON THE TREATMENT OF HYPOCHONDRIASIS.

By Dr. C. Handfield Jones, F.R.S., Physician to St. Mary's Hospital.

[We all know how difficult cases of hypochondriasis are to treat—how almost impossible it is to persuade the patient that the alarming apprehensions with which he is haunted have no basis in reality. It does not become a truth-loving and earnest physician to treat such a malady in a superficial manner.]

The following instances illustrate the importance of searching for physical causes which may give rise to the complaint. Romberg (vol. ii., p. 6) says, "The hypochondriasis of students frequently produces palpitation; and an instance of its occurrence from the same cause in advanced life is presented to us in P. Frank himself, who, while devoting special attention to the subject of heart disease in Pavia, when preparing his lectures, was attacked with such severe palpitations, accompanied by an intermittent pulse, that he felt assured he was affected with an aneurism. The symptoms only ceased after the completion of his labours, and after he had enjoyed the relaxation and diversion of a journey." I am well acquainted with the case of a gentleman who, while studying medicine, became haunted by the idea that his abdominal aorta was aneurismally dilated, and was pressing on the bodies of some of the lumbar vertebræ. He was quite aware of the delusive nature of this idea, and very sensibly endeavoured to shake it off by a trip into the country. This, however, entirely failed, and the morbid imagination prevailed to such an extent that his life for some time was rendered very miserable. A man of great mental capacity and attainment assured me that at one time, after much hard work, he became so unnerved that he did not like to drive to the city in his brougham unless his wife went with him. In all these instances the mind was clear and free in itself, and its actings were only impaired by reason of the cerebral machinery in one part being out of gear. To say that these persons were affected with insanity, as the term is currently employed, would no doubt be incorrect; but I cannot avoid believing that such aberrations from the normal acting of nervous centres are minor shades and degrees of the same morbid process which, in its higher developments, deranges the intellectual faculties more or less completely. One cause of exhaustion of nervous power, which often induces hypochondriasis to a most pitiful extent, is spermatorrhœa, or, as it might be better termed, "chronic prostatitis," the result of sexual excesses or abuse. The reality of this cause is admitted by Romberg, Copland, Albers, Trousseau, and Erichsen, although it has been denied by a late writer. I hardly think that anyone who reads the cases recorded by Lallemand, and compares them with those occurring in his own experience, can doubt the pernicious effect of such deep-seated urethral irritation on the superior nervous centres, or the advantage which may result from appropriate local treatment.

The subject of the following history, J. J., a carpenter, aged 37, married, applied to me as an out-patient at St. Mary's, Aug. 6, 1863. He continued under treatment till Nov. 17, 1864, when he was discharged fairly well, having improved almost continuously since the beginning of June. He was a rather pallid, pusillanimous-looking individual, tall and spare, who addressed you in a rather anxious fussy manner, with a low voice. His complaints were of pain in the lower lumbar and sacral regions, extending from the top of the sacrum forwards to a spot near the umbilicus, with weakness of the calves of the legs. At one time he had a sense of rising and suffocation in the throat, or a feeling of weakness in the part, attended with copious expectoration (?) of mucus. A nocturnal urticarious eruption was another of his troubles, but did not last long. His chief affliction, however, was a sense of scalding felt along the urethra after micturition, which lasted about half an hour. With this there was said to be a slight gleety discharge, but of which I could rarely discover any traces. It seemed, however, to dwell much on his mind, and he was constantly referring to it. He attributed it to a previous *gonorrhœa, and affirmed* that he had never been right since. I tested at one

time the effect of a belladonna injection into the urethra, in the hope that it might allay the scalding dysæsthesia, and arrest the discharge. This it might be expected to do, on the view maintained by several eminent authorities, that the drug acts as a tonic to the vaso-motor nerves. In this respect, however, it failed entirely—indeed, it acted in a contrary way, increasing the discharge considerably, and rendering it more yellow, though the scalding appeared to be relieved. The strength of the injection was fifteen to thirty grains of extract of belladonna, to four ounces of mucilage mixture. I may remark, by the way, that Mr. Jabez Hogg has seen a very weak solution of atropine produce in a healthy eye a very large amount of congestion (*vide* "Ophthalmic Surgery," p. 98). That belladonna acts as a sedative to the sensory nerves of an irritable bladder, or of irritable bronchi, and that in large doses it paralyses the vaso-motor nerves of the head and face, appear to me tolerably well-established facts, and by no means consonant with the views above referred to. I should, however, mention that I have found the drug to act very beneficially in cases of cynanche tonsillaris, and perhaps—as in this instance, the reduction of the inflammatory tumefaction depends probably on arterial constriction—the truth may be, that in large doses it acts paralysingly, in smaller toningly, just, indeed, as alcohol does. Whether a drug can act in one way on one region of the body, and in an opposite way on another, is at present extremely doubtful. But to return to our case. The remedy which appeared to effect a cure was strychnia, which he took in doses of a sixteenth to a twelfth of a grain, together with sulphate of iron and cod-liver oil, during most of the time, for ten months continuously. In another very similar case the same remedy appeared of decided efficacy, but the patient did not attend so perseveringly as the one whose history I have related. In him I think there can be no question that the urethral disorder was slight, and was not the real cause of the depression of nervous power. Had it been otherwise—had the cerebral paresis depended on genito-urinary irritation acting in an inhibitory manner upon the brain, local remedies would in all probability have been required. As it was—as the hypochondriasis was primary, and not of reflex origin, the steady tonic action of a nervine, aided, doubtless, importantly by the cod-liver oil, sufficed to restore the nervous centres to a fairly normal condition.

The following history may, I think, be compared instructively in some respects with the above. A gentleman, aged 28, who had been long exposed to the enfeebling influences of a tropical climate, and had suffered several severe attacks of tracheitis, consulted me at first for the latter, but subsequently complained of a good deal of what he called a gleety discharge. I found that this was not constant. There was no trace of discharge when I examined the penis. He stated that it came on after any exertion; often occurred quite irregularly, and without any erection taking place. It was evidently more of the nature of an emission than a running, and caused, when it happened, a great sense of debility. He felt it an inconvenience and a deterioration of his health, but did not complain about it more than a man reasonably might. He showed no sign of hypochondriasis. He acknowledged that his venereal appetite had always been strong, and that he had committed a good deal of excess in this way. I prescribed for him a pill composed of six grains of lupuline, a grain and a half of camphor, and a quarter of a grain of extract of belladonna, two to be taken twice a day; and in a short time the annoyance entirely ceased. Now I will ask the reader to observe—(1) That in this case there was quite as much, if not more, genito-urinary disorder than in the first, yet there was no hypochondriasis, which was so marked a symptom in J. J. (2) That the experience of the best authorities is quite decisive as to the production of even actual insanity by genito-urinary irritation. Brown-Séquard mentions having obtained the cure of a case of insanity with general paralysis, in a male who suffered from inflammation of the prepuce and glans with phimosis, by having circumcision performed (*vide* the Lancet, 1861, July 27th). (3) That therefore we must admit that the vital condition of the nervous centres determines very much the effect which shall be produced by a peripheral irritation. A slight one may cause *severe symptoms*; a *greater* none at all; according to the amount

of resisting capacity possessed by the patient. Lastly, and as the general lesson from the whole, I will remark that it is evidently of the first importance to distinguish primary hypochondriasis *attended* with genito-urinary dysæsthesia from secondary hypochondriasis *depending* on genito-urinary irritation; and that while it would be a great mistake to employ local measures, such as cauterisation, in cases of the first kind, they may be quite necessary in severe cases of the second.—*Lancet, May* 25, 1867, *p.* 627.

18.—CLINICAL REMARKS ON NEURALGIC PAIN OF THE SIDE.

By Dr. Peter Eade, Physician to the Norfolk and Norwich Hospital.

[There is a class of painful local affections in which the complaint is really the expression of an overworked and exhausted condition of the muscles, and of the nerves supplying them.• These pains are very common amongst labourers, and are generally supposed by them to be "rheumatism."]

It is seen in the muscles of the arm, chest, back, and especially the legs and the joints of the lower extremities: and whilst it is untouched by the ordinary anti-rheumatic remedies, it readily yields to rest, good diet, vegetable and mineral tonics, and perhaps mechanical support. Patients suffering from this class of ailment are not unfrequently sent into hospital with the odium attaching to them of being malingerers, rather, as it has sometimes seemed to me, because they have shown no marked symptoms of disordered health beyond the pain and weakness they state they suffer from, and because of the inefficacy of the medicines employed to produce a cure, than from any valid reason for such a suspicion. But these patients do obstinately persist in stating themselves unable to work, because really they feel themselves unequal to it, and because their muscles, exhausted by daily and too prolonged labour, become really weakened and incapable of further effort, and refuse to do the bidding of the will, because physically unequal to further active exertion.

No doubt real malingerers are met with, but I have seen several examples of men so classed who have gradually and steadily recovered in hospital, and in whom the incapacity to work was unquestionably real, and not the mere effect of a wanting will, and who readily enough resumed their employment when rest and treatment had restored their physical power.

The *actual condition* of the aching nerves in these cases of lateral pain can fortunately only be inferred; for, as the affection is one which is rarely fatal, and, indeed, never so unless some other more serious disease is engrafted upon the disorder which produces it, so opportunities rarely occur of ascertaining their pathological state by post-mortem examination. There is, however, no doubt that the painful peripheral twigs of nerve are, like the rest of the nervous system, in a state of atony; hence arise relaxation of the blood-vessels, congestive fulness, imperfect nutrition, a flabbiness or softness of the nerve-tissue, and then depravation of function and hyperæsthesia. But as this atonic condition of the nerves is doubtless present more or less in the whole body, as well as in the lower intercostal nerves, the question arises why these nerves suffer more than the others, and why the pain should always be felt in the side even when also present elsewhere. To these questions a very distinct answer cannot, perhaps, be given, but if we look at the class of affections in which this pain is mostly met with, we shall see that a large majority of them are more or less connected with imperfect uterine action, and although it is quite probable that the disordered uterine function itself may often, as well as the pain, be merely symptomatic of some more remote cause, yet the connexion between the two is so frequent as to justify their being looked at together.

Many explanations of this lateral pain have been attempted by writers on the subject, who have vaguely and variously referred it directly to sympathy with the uterus, to excitement of the sympathetic system, to the general anæmia, &c. Very recently Dr. Martyn (British Medical Journal, 1864), has endeavoured to show that it is a reflex neuralgia, induced by, and expressive of, some distress in the heart; and he gives details to show how such a causation might be anatomically probable. But my observations tend rather to show that a true neuralgia of the heart does exist which is quite a distinct affection from the one we are considering, and that with this cardiac neuralgia, whilst there is pain distinctly referred to the heart, inframammary pain is rather the exception than the rule; and that, on the other hand, with uterine or other constitutional irritation, it is almost universally present. Moreover, such an origin could hardly account for the widely diffused pain and tenderness so often felt not only along the side, but over a large portion of the dorsal spinal region. Looking, therefore, to its close connexion with, and almost constant presence in, uterine and ovarian disorders, and its relation to those faults of the blood which so continually eventuate in them, and bearing in mind also the light which the researches of Drs. Marshall Hall and Brown-Séquard have thrown upon reflected nervous affections, I am more inclined to the opinion expressed by the late Dr. Todd in his clinical lectures, and by others, that this pain is virtually merely the reflected expression of an uncomfortable and irritated uterus or ovary, the irritation of which, passing along the hypogastric plexus and sacral nerve, to the spinal cord, is then reflected along it to one or more of the intercostal nerves, in which, as is usual, the depraved sensation is felt at the peripheral extremity. This would thus form a generative sympathetic arc or circle, with one extremity formed by the peripheræ of the uterine nerves, the other by those of the intercostals; the fact that the pain of the back when present is rarely felt lower than the ninth or tenth dorsal vertebra being explained by our knowledge of the fact that the spinal cord usually ends at about the level of the tenth or eleventh vertebra, and divides here into the nervous cord forming the *cauda equina*—in other words, it ceases here to be a sensitive or sensory nervous centre, and becomes only a conducting body. Why the pain should generally be felt in one side only, and that the left, is a problem perhaps yet unsolved, although the nervous relations in the chest, described by Dr. Martyn, may perhaps be the real anatomical cause of it; and I can see no reason why his explanation of the localisation of the pain through the peculiar relations and distribution of the sympathetic and intercostal nerves should not be equally true, whether the exciting eccentric cause be seated in the thoracic or the pelvic cavity. I have tried, but failed to discover any greater tenderness (when any exists) in one ovary or side of the uterus than the other—a tenderness which, on Brown-Séquard's theory of the decussation of the sensitive fibres in the spinal cord, should be looked for usually in the right rather than in the left flank.

It is remarkable that whilst leucorrhœa (the connexion with which was especially pointed out by Dr. Todd) and other chronic uterine and ovarian affections induce this pain, gonorrhœa, also a disorder accompanied with constant discharge, but which is a vaginal and not a uterine affection, does not give rise to it; and the same is the case with chronic catarrhal and other disorders of the bladder. When any secondary nervous symptoms do result from these causes, they are observed not in the upper part of the body, but in the lower extremities, the lesion being paraplegic, and the reflex action being then induced in another arc, with the nervous circle composing it on a lower level of the body, and the direction of the nervous current reversed.

It is also to be noticed that men who spend their days in the most sedentary occupations, and often in a most confined atmosphere, do not suffer from this pain of the side as women so confined and so occupied do. Shoemakers, for example, suffer much from epigastric sinking and uneasiness, irregular action of the heart, and hypochondriasis; but, unless other causes are super-

added, they do not suffer from disorder of their generative organs; nor do they complain of intercostal pain or tenderness, nor yet spinal symptoms. Tailors, again, who often work under conditions not very dissimilar from those of milliners and dressmakers, and often get almost similarly ætiolated, do not suffer from this form of pain as these latter do. It is necessary, therefore, to look for some other cause as active in producing it; and there seems little reason to doubt that the uterine nervous system in a state of continued irritation is this cause.

But, although this pain is so unfrequently met with in the male sex, yet instances are occasionally seen of its occurrence in them, and I have met with several examples of male patients who have complained of abiding pain in one or other side, not to be distinguished from that seen in women. Such patients are generally of a nervous, excitable, or, indeed, of truly hysterical temperament, whose emotional system has been excited unduly and beyond its power of resistance. A somewhat similar pain is also occasionally met with in weakly men who have exhausted the nervous energy of their intercostal muscles by too great or too prolonged muscular exertion of the chest, and is notably seen in clergymen, public speakers, and others, who make great efforts in the public exercise of their vocation.

Neuralgic pain of the side is but a symptom of disease, and not only so, but it is a symptom which may arise in the course of many different, though perhaps allied, diseases. To speak exhaustingly, therefore, of the remedies proper for this pain would be to compose a treatise upon those means, mental, moral, hygienic, and medicinal, which are appropriate to the numerous conditions of disordered health in which it occurs. This is not the object of these remarks, and I will only observe that a glance at the list of causes, and of the morbid affections with which it is associated, will suggest a large proportion of the means essential for their and its removal. Thus, according to the special circumstance of the case, a good or improved diet, change of scene, sunlight, cold sponging or bathing, shower-baths, frictions to the skin, daily and appropriate walking exercise in the open air or on horseback, cheerful society and occupation, the avoidance of close and heated rooms and of late hours, or of too much emotional excitement, with means directed to the quieting and soothing of an over-wrought body or mind, may all in their proper place conduce to recovery.

So, again, some one or more of the various nervine tonics or alteratives may contribute to its removal by restoration of the general or uterine health. Such are ammonia, valerian, assafœtida, camphor, steel, quinine, strychnine, acids and alkalies, iodine, bromine, cod-liver oil, &c., &c. Vaginal or uterine injectines, or mechanical appliances, will also in appropriate cases be useful. Locally, more or less relief will be obtainable from the judicious use of warm fomentations, either simple or medicated; of belladonna or opium plasters; of liniments of opium, belladonna, aconite, or chloroform; of epithems or blisters of chloroform; and occasionally of blisters, sinapisms, leeches, or ice-bags. Belts or bandages to the sides and mechanical supports to the spine will also be occasionally of service.

My own special experience of the treatment of the disorders in which this pain occurs, and therefore of the pain itself, may be summed up as follows:—That steel is usually essential (except there be pure active hysteria with plethora, or acute suppression of the menses in otherwise healthy subjects), and that its efficacy is much increased by the addition of a little aromatic spirit of ammonia, or of what is often better, the ammoniated tincture of valerian.

That where there is obstinate amenorrhœa, the addition of a grain or two of iodide of potassium to each dose of the steel mixture is often extremely beneficial.

That where ovarian excitement predominates, camphor and henbane in pills, bromide of potassium with steel and valerian, and chloroform blisters, or perhaps leeches to the flanks, will be found most useful, and that where, with general debility, leucorrhœa is the existing form of uterine disorder, a

combination which I have used largely in private as well as hospital and dispensary practice will often be found singularly efficacious. The following is my formula for this chalybeate mixture:—℞. Tinct. ferri sesquichlor. ℥xv.; acid. nitric. dil. ℥xv.; magnes. sulph. ℥ss.; tinct. aurantii ℥xv.; aquæ ad ℥j. M. ft. haustus bis vel ter in die sumendus. When much weakness is present, a little quinine or tincture of nux vomica is a useful addition to the draught. The *rationale* of its beneficial action would appear to be that in many of these cases there is a sluggish state of liver, which increases the passive retardation of the abdominal circulation that already exists from the very nature of the affection; that the combined effect of the sulphate of magnesia and nitric acid is to disgorge the liver of its bilious matters, and so to relieve its general and portal congestion, and by its secondary action on the bowels to stimulate alvine secretion as well as evacuation, at the same time that, by thus relieving the portal as well as general abdominal circulation, it enables the pelvic veins more readily to empty themselves, and thus to diminish the passive uterine and ovarian congestion which doubtless is often also present in these cases, the steel and other tonics further improving the state of the blood, and so secondarily giving tone to the nervous centres and to the uterine nerves and fibres. But whether or no this be the proper explanation of its action, certain it is that it is a most useful medicinal combination, and often does great good where steel alone or in other forms has been given in vain. Its mode of action must be somewhat that of the morning dose of sulphate of iron and sulphate of magnesia recommended by the late Dr. Rigby, but it has the advantage over it of being less unpleasant, less voluminous, and, being given in smaller doses, of acting more gradually. It is also an equivalent, but in my opinion far preferable, to the combination of sulphate of iron and sulphate of magnesia in common use in the London Hospital.

Locally, leeches (when applied either to the side or to the spine) often relieve at first, but the relief given is seldom permanent, and they frequently leave a disagreeable sensation of weakness, but they are sometimes distinctly of service when applied over the ovaries or in the groins. Sinapisms and blisters are often useful; but, in my experience, are more efficacious at a later period, when steel and other remedies have been for some time persevered with, and then a blister will sometimes at once and completely remove the pain.

Belladonna plasters, worn continually on the painful side or in strips down the back are often of much use; opium or belladonna liniments are less to be relied on. Chloroform, applied as a rubefacient or as a blister, sometimes relieves markedly, and hot poppy fomentations will often prove of great advantage when applied for some length of time in cases where the pain is severe or shooting in character. When the spine is a principal seat of pain, and the patient suffers from undoubted spinal weakness, a mechanical support, after the fashion of the instrument made by Pratt or of that recommended by Dr. Dick, may sometimes be of great service. It only remains to allude to the employment of subcutaneous injections of morphia or belladonna, or of the local etherisation of the aching part by Dr. Richardson's apparatus in those rare cases in which the pain is sufficiently severe to necessitate the having recourse to more potent sedatives or anæsthetics.—*Medical Times and Gazette. July* 20, 1867, *p.* 64.

19.—ON NEURALGIA.

By Prof. Trousseau.

In his lecture on neuralgia, Professor Trousseau brings forward his important observation that in all cases of this disease there is more or less acute

pain on pressure over the spinous processes of those vertebræ which correspond to the origin or point of exit of the affected nerves; and to this *spinous* point he justly attaches very considerable diagnostic value. He draws attention to another peculiarity, concerning which, he thinks, writers have not been sufficiently explicit—the existence, namely, of cutaneous hyperæsthesia at the points of exit of the nerve-trunks. This he terms the *spot of peripheral expansion*. He denies the statement of Valleix regarding the *superficial tender spots* to be found in intercostal neuralgia, but admits the existence (amongst others) of those which he indicated in cases where the cranial nerves are affected. Where the neuralgia is superficial, Trousseau finds that the local application of atropine or belladonna is sufficient to relieve pain in the majority of cases. He generally employs a compress steeped in a solution of sulphate of atropine (five grains of sulphate of atropine in three ounces of distilled water), and covered with a piece of oiled silk to prevent evaporation. This application is continued for an hour at a time, and is frequently renewed, provided no disagreeable constitutional effects are produced. When the neuralgia is more deep-seated or severe, he has recourse to the endermic use of morphia, or the subcutaneous injection of morphia or atropine; and in obstinate cases he makes an incision through the skin, and places in the wound one or two medicated boluses. When he adopts the endermic method, he removes the cuticle by means of ammonia, as it can be done in this way much more neatly and expeditiously than by means of cantharides. He also advocates the inhalation of chloroform during the attacks.—*Lancet, July* 6, 1867, *p.* 12.

DISEASES OF THE ORGANS OF CIRCULATION.

20.—ON CARDIAC DIAGNOSIS APART FROM MURMURS, AND SPECIAL STATES OF VALVULAR DISEASE.

By Dr. W. T. Gairdner, Professor of Practice of Physic in the University of Glasgow.

[In all cases where the state of the heart is in question much may be learnt by attention to little things. The frequency and character of the pulse, the state of the radial artery, and of other arteries, as of the temples and of the superficial part of the neck; the situation and character of the impulse against the chest is also of importance. The frequency and character of the respiration must also be noticed and compared with the frequency and character of the pulse. The nominal relation is about 1 to 4, and if we find this much altered we should ascertain the reason.]

These are a few of the preliminary points, which a physician of observant habits and good education will accustom himself to note in almost every case of suspected cardiac disease. I now pass to some observations touching more directly the physical state of the heart itself.

The heart lies beneath and to the left of the sternum, inclining, as you are well aware, from right to left, and from above downwards; so that while the base of the ventricles may be said to be about the left border of the sternum near its junction with the third left costal cartilage, the apex extends to a point at a variable distance below the nipple, and usually corresponding with the interval between the fifth and sixth ribs. Between these limits lies the extreme length of the ventricles, and the breadth of the ventricles may be roughly estimated in a normal subject as extending from the middle line of the sternum to a point indicated either by the apex-beat, or by the extreme limit of the percussion dulness leftwards. But in estimating the position of the heart you have always to keep in mind that a considerable portion of the organ is not in contact with, nor even very near the wall of the chest. Both the auricles entirely, and the left ventricles in great part, are concealed as it

were from direct observation by the lungs, which overlap the base of the organ, and by the right ventricle, which lies in front, and thus interposed between the left ventricle and the ribs. The small portion of the left ventricle, which constitutes the apex of the heart in normal subjects, and the impulse of which at the fifth intercostal space constitutes the apex-beat, is literally the only part of the whole left heart that is directly within the scope of observation from without. The right ventricle, on the other hand, with the pericardium alone intervening, may be said to be almost in contact with the thoracic wall over the greater part of its surface. Now, upon this arrangement of the parts depends a very important point in diagnosis. Normally, you have the left ventricle, through its extreme apex only, impinging with an extremely well-marked abrupt impulse on a particular point in the wall of the chest, and thus giving rise to what we call the apex-beat. But the greater part of the left ventricle retires altogether from observation, while the right ventricle, with its much larger surface of contact, and its much more diffused and comparatively feeble impulse, occupies the greater part of the precordial space. This is the normal condition, a direct, well-defined, punctuate impulse of the left ventricle; and an equally direct, but much more diffused impulse of the right ventricle. Suppose, however, that you have one of these ventricles dilated or hypertrophied as compared with the other; a certain amount of hypertrophy of the left ventricle will simply lead to increase impulse, and at the same time to displacement downwards, of the apex-beat. A much greater degree of hypertrophy, and more especially of dilatation of the left ventricle will, it is true, lead to modifications of a quite different kind, hereafter to be described; but the effect of hypertrophy of the left ventricle upon the apex-beat is usually to intensify it, and perhaps also to increase its diffusive force in accordance with the increased power of the ventricle. In the case of the right ventricle, on the other hand, a very moderate amount of hypertrophy or dilatation has usually a most remarkable effect upon the apex-beat. Dilatation of the right ventricle, in fact, to the extent to which it renders that ventricle more prominent, also causes the left ventricle to recede from the surface; interposes, in fact, the right ventricle as a buffer (so to speak) between the impulse of the left ventricle and the thoracic wall, and thus to a certain extent tends to suppress the proper apex-beat. In other words, while up to a certain point hypertrophy of the left ventricle usually tends to increase the distinctness and definition of the apex-beat, hypertrophy of the right ventricle to a corresponding extent almost always tends to the diminished distinctness of the apex-beat, and ultimately to its extinction. Now partly by discovering and carefully defining the apex-beat; partly by listening to the sounds and closely observing the impulse all over the heart's surface; partly also by the use of percussion, we are able to discover with tolerable certainty what is the state of any particular case as regards the *presentation* (to use a term familiar in midwifery), of the right or left ventricle, that is, we are able to determine, up to a certain point, whether the right or the left ventricle is the more prominent, and whether it owes this prominence to hypertrophy, or dilatation, or both. I have here stated in general terms a very important diagnostic problem, on the details of which it will be necessary to make some further remarks.

The first step in the solution of this problem is to determine, if you can, the exact position of the apex-beat. What we call the apex-beat proper, is an impulse of a quite peculiar and definite character, tangible, or visible, or both, limited to a single spot you can cover with the point of one finger, or at most with the point of two fingers placed in a line in the intercostal space. If the impulse is over a much greater space than this, or if it is not perfectly well defined within these limits, you have reason to suspect that it may not be the apex-beat proper, or that the anatomical conditions ruling the apex-beat are considerably modified. In every instance, therefore, of suspected cardiac disorder, we feel for the apex-beat; we determine as far as possible its character and position, and reason from these to other data connected with the impulse and sounds of the heart. We expect to find the

apex-beat, as a rule, in the fifth intercostal space, about half-an-inch (perhaps more, perhaps less), within a vertical line falling from the nipple. And if the apex-beat is not found in this, its normal position, we expect before we have done to know the reason why.

It is necessary here to observe, however, that in certain cases, not very easily defined or classified, you may have great difficulty in finding the apex-beat at all. The action of the heart may be too feeble, or the patient may be too fat and flabby, or some other morbid conditions may have intervened to alter the relations of the normal apex-beat to the chest-wall. The position of the apex-beat, moreover, is normally, to some extent, variable, even in the same subject, according to position; and it varies very considerably in its relation to the chest-wall in different subjects, within the limits of health, the chief force of the impulse being sometimes received on a rib, but more commonly in the fifth intercostal space, as above stated. When you are in any difficulty on this point, lay the patient over on the left side, well towards the face, and feel with great care for the apex-beat in the fifth and the adjoining intercostal spaces. If you cannot detect the defined and strictly localised impulse already described in this position of the body, you will probably not be able to find it at all; and the inference then will be, that there is something in the condition of the heart and great vessels which prevents the apex of the left ventricle from coming to the surface.

Of course you will take care not to be misled by displacement of the heart; from pleuritic effusion, which may carry the apex-beat over to the other side of the chest altogether; or, from atrophy of the upper lobe of the left lung, which may (as I have elsewhere shown) carry the apparent apex-beat up to the second and third intercostal space. It will be well, therefore, in all cases in which the apex-beat cannot be distinctly verified at first, in the position above described, to try with the stethoscope if you can find a spot, either at the usual seat of the impulse, or, if not there, at any other point, however remote, at which the first sound of the heart is decidedly and definitely more clear and superficial than elsewhere; then resume the examination by touch, and try if you cannot discover there a tangible, and perhaps a visible, apex-beat—not the heaving of a comparatively large space, but a defined and punctuate impulse, as aforesaid.

Next, if you find ever so little trace of an apex-beat, examine the radial pulse, or the pulses in the other arteries, and compare, mentally, the strength of impulse denoted by the arterial pulse with the force and definition of the apex-beat; for I need hardly say that there is an obvious reason why there should be a certain correspondence between the one and the other. If you should find a very strong and well-defined apex-beat, for example, with a very feeble arterial pulse; or if, on the other hand, there should be found a very strong, or even a normal, radial pulse, associated with a very feeble apex-beat; in either case there would be reason to inquire into the cause of the discrepancy, and much more so if, with a strong or normal radial pulse, you should fail in discovering the site of the impulse of the left ventricle at all.

Now, suppose you have arrived at this point—the impulse of the left ventricle at the apex is either undiscoverable or abnormally indistinct, as compared with the radial pulse—you desire to know what is the cause of the discrepancy, the cause of the relatively feeble or perhaps inappreciable character of the apex-beat. You have as yet simply noted a fact which may have many cases, and you are advancing step by step towards the true cause. It may be merely that the apex beats upon a rib, instead of into an intercostal space; in such a case you will have, of course, a very ill-defined apex-beat, but then the stethoscope will inform you of a clearly defined and circumscribed space corresponding to the apex-beat in which the first sound has its usual well defined superficial character, and this will save you from error. Again, an emphysematous lung may, by projecting over the apex, *i.e* over the small portion of the left ventricle which is normally uncovered by lung, suppress the apex-beat; but here the signs and the history of emphy-

sema of the lung, and especially the state of the percussion near the edge of the heart, will usually reveal the cause of the difficulty. Again, the dilatation of the left ventricle itself may render the apex-beat indistinct; not (I desire you to remark) hypertrophy merely, but either dilatation alone, or dilatation with hypertrophy of the left ventricle. Hypertrophy alone, or hypertrophy with moderate dilatation, does not suppress the apex-beat, but rather increases its force, and within certain limits its definition, as I have already said: displacing it at the same time towards the left and downward. But great dilatation, apart from hypertrophy, of the left ventricle, will, by rounding the apex, by weakening the fibre of the heart, and by increasing the extent of surface impinging upon the chest, have the effect of relatively weakening, and possibly of apparently suppressing, the apex-beat. Effusion into the pericardium will also have the effect of suppressing the apex-beat; and lastly, as I formerly remarked, the mere distension of the right ventricle with blood, and, still more, the permanent hypertrophy and dilatation of the right ventricle carried to a certain point, will cause the apex-beat of the left ventricle to disappear altogether.

Before proceeding further, let me state to you a remarkable fact, or rather an experiment which any of you may perform upon himself, and which completely illustrates, in a manner that you will not easily forget, this influence of the state of distention of the right ventricle on the apex-beat. Put your hand upon the fifth intercostal space, lean over well to the left, and make sure that you have clearly defined the apex-beat in your own person. You will find that you can suppress it, or nearly suppress it, at will, simply by holding the breath. Do not take a very deep breath, for that is to cause the lung to expand, and so to project more or less over the apex, but simply *stop* breathing, when you have your hand upon the apex-beat, and when you have made sure that it is quite well defined. Keep the same position without breathing as long as you can, until, in fact, the sensation becomes intolerable, and you will find that after a short interval the impulse of the left ventricle becomes less and less distinct, and finally, but quite gradually, it vanishes altogether. What is the cause of this? Not, of course, that the circulation has ceased, for you will find the pulse quite distinct at the wrist, but simply because all the time that you have been holding your breath the right ventricle has been filling and over-filling, so that long before you are even half asphyxiated the right ventricle and auricle have been becoming more and more distended—more and more prominent; and thus the left ventricle, or rather the apex of the left ventricle, has been to a corresponding degree thrust into the back ground. This is a most instructive experiment, which I was led to make very frequently while examining into the case of M. Groux, the singular example of congenital cleavage of the sternum—a case which illustrated so many points in the physiology of the heart. You will learn from this experiment, which you may easily repeat as often as you please without danger, almost without inconvenience, precisely what happens in nearly the whole of the cyanotic class of diseases, and in all diseases whatever which are attended by well-marked hypertrophy and dilatation of the right side of the heart.

The actual tests, then, of hypertrophy, or dilatation of the right ventricle, are these:—With a suppressed or a relatively diminished left apex-beat, you have a diffused impulse to the right of the usual position of the apex-beat, and commonly easily distinguished from the latter (even when both are present) by its diffused and heaving character, readily appreciated by the palm of the hand strongly pressed over the costal cartilages. Sometimes you will have no difficulty in detecting a proper apex-beat in its usual situation, or perhaps in such a position as to indicate hypertrophy of the left ventricle, and, in addition, you will discover the diffused flat impulse, heaving up the whole palm of the hand, quite distinguishable from the other, and extending from the third or fourth left cartilage almost to the xiphoid cartilage, or into the epigastrium. In such a case it is to be presumed that both ventricles are very considerably enlarged; but the relative degree of enlarge-

ment of the right or left ventricle will be determined mainly by the degree to which the apex-beat retains or loses its definite character.

[To aid in the diagnosis the limits and position of the heart must be approximately determined by percussion. There is no difficulty in defining the position of the left ventricle by the apex-beat, and the right border of the organ can be determined by percussion. The right border, except in cases of dilatation and hypertrophy of the right ventricle, usually corresponds with the mesial line.]

If the apex-beat is not distinct and punctuate, you are left in uncertainty as to the amount and the limits of the precordial dulness which corresponds to the left ventricle, and there is no way, so far as my experience goes, in which in such cases the precise limit of the two ventricles can be determined. But, speaking generally, if you find even the trace of a punctuate apex-beat, you may assume that by far the greater part of the dull space which is to the right of the apex-beat and above it corresponds to the right ventricle, and when the apex-beat cannot be defined, you may generally assume that nearly the whole of the precordial dulness corresponds to the right ventricle, or perhaps also the right auricle; at all events to the right side of the heart. Now, if this space gives to the hand a dull, flat, heaving impulse, extending towards the xiphoid cartilage or into the epigastrium, and if the sounds of the heart are propagated with undue distinctness in the same direction, while they are extremely indistinct, muffled, or distant over the usual site of the apex-beat, you may feel perfectly satisfied that the right ventricle and auricle are in a state of comparative dilatation and hypertrophy, from some cause or other, most probably from emphysema of the lungs.

But there is yet another test of the comparative condition of the two ventricles, viz., the state of the second sound of the heart. You know that the second sound is caused by the blood falling back upon and closing the sigmoid valves. Each ventricle, in contracting and expelling its blood, gives rise to some part of what is known as the *first* sound of the heart, the precise origin of which has been the subject of much discussion; but there is no such discussion as to the origin of the *second* sound, which is all but universally admitted to arise from the recoil of the blood in the arteries upon the sigmoid valves, at the moment after the systole of the ventricles is completed, and when their diastolic movement is about to begin. Now it is important to remark, that although they are often blended to the ear, each ventricle has a quite separate second sound of its own, corresponding with the recoil of the blood in the one case of the pulmonary artery, in the other of the aorta, on their respective sigmoid valves. And although the observation is at times a difficult one, and cannot be followed out in any case without great caution and special experience, it is certain that the sound of the pulmonic recoil can at times be accurately distinguished from that of the aortic recoil, and *vice versa*. To acquire this special skill it is necessary that your ear should be familiar with the precise character of the sound peculiar to each ventricle, normally. To obtain the aortic second sound in its purest form, listen in a normal chest exactly over the second right costal cartilage, and very close to the sternum—indeed with the stethoscope overlapping the edge of the sternum as well as that of the cartilage. To obtain the pulmonic second sound as much as possible isolated from the other, listen over the third left costal cartilage near the sternum, but not quite so near as in the other case. There are various circumstances, which can only be learned by experience, which tend in certain cases to modify the results of this examination, and the position of these sounds; but, speaking generally, you may say that the aortic second sound corresponds with the point at which the aorta emerges from below the pulmonary artery, and approaches the surface; while the pulmonic second sound corresponds in position with the extreme base of the heart, where it is uncovered by lung, or in certain cases with the *conus arteriosus*, or the most prominent part of the right ventricle. In cases where, from particular circumstances, the difficulty of distinction is great, the aortic second sound may be distinguished absolutely by placing the stethoscope

over the innominate artery, or the arteries of the neck; while the pulmonic second sound may be heard at the position before indicated, so that, in particular instances, a thoroughly skilled observer will find himself able to declare with positive certainty that one or other of these sounds is relatively exaggerated, or deepened in tone, or otherwise altered in character.

Now, in these alterations of the second sound we have a very important aid to our estimate of the state of the two ventricles—an auscultatory test, as it were, not perhaps in all instances to be trusted alone, but acting precisely like many of our most useful diagnostic tests, in confirmation or correction of others. The quantity of blood expelled by each ventricle at each systole —the force and rapidity of the systolic movement—the elasticity of the arterial walls—the amount and rapidity of their distention—the suddenness and force of the recoil, are all factors which enter into the determination of the precise character of the second sound in each case. And amid these complicated conditions the ear of the skilful observer has to decide and define as it may. But if either the left or the right ventricle is, to a very marked extent, predominating in power, it will probably expel a larger quantity of blood at a higher rate of pressure, and the recoil will be correspondingly sudden and strong. Accordingly, there will be an increased, and to some extent an altered, character of the second sound which is peculiar to that ventricle and its corresponding artery. If, on the other hand, there is a marked deficiency in the power or the expulsive activity of either ventricle, or if the mechanical conditions under which it expels its blood are interfered with, there will be a corresponding diminution or *tonelessness* of the second sound corresponding to that ventricle. In the case of the pulmonic second sound, the increase of the recoil which attends hypertrophy of the ventricle is very frequently observed, and not unfrequently in connection with an equally marked diminution in the tone of the aortic second sound. When this is the case, the conclusion as to the state of the two ventricles is irresistible. On the other hand, the aortic second sound is often so peculiarly altered as to lead to results of great importance in the diagnosis of the condition of the left ventricle and of the arterial system, but these results will come under consideration on a future occasion.—*Glasgow Medical Journal, July*, 1867, p. 81.

21.—THE SOUNDS OF THE HEART IN THEIR RELATION TO PATHOLOGY.

By Dr. Arthur Leared.

(Read before the Medical Society of London.)

Dr. Leared commenced by stating that the normal and abnormal sounds formed by the circulation of the blood are more closely allied in their causes than is generally supposed. He added that it would be granted that whatever explanation of the normal sounds fits into and accords most clearly and in the larger number of instances with the explanation of their abnormal changes, is most worthy of acceptance; and, conversely, that any theory which is incapable of adaptation in a salient respect should be rejected. Most theorists have been compelled to ignore any connexion between the two classes of sounds, on account of incompatibility in their explanation of the respective causes. There are certain broad facts in which, if physiology and pathology do not agree, there must be an absence of truth on one side or on the other. *De minimis lex non curat;* and he proposed, as sufficient for his purpose, to deal with the more important points of cardiac diagnosis, and to test the truth of prevailing theories of the sounds of the heart by investigating the connexion which exists between the sounds issuing from the organ in health and in disease. He affirmed that the sounds of the heart, in common with all the sounds formed by the circulation, mainly emanate from the blood itself. They are not essentially due to the vibration of valves, or to

the tension of muscles; and even if these causes contribute in a minor degree to form the sounds, it does not affect the argument. If the blood circulated without any rythmic motion, no sound would be caused; but it is thrown into broken and impetuous currents by the action of the heart, and sounds are thereby necessarily produced. Besides rhythmic motions, certain conditions are requisite to produce the normal heart sounds. If all these conditions be not present, the sounds will be abnormal. Both sounds of the heart are formed by the blood itself in the commencement of the aorta and pulmonary artery, and are closely allied in their mechanism. Dr. Leared in part quoted from and in part referred to his published writings on this subject. The first sound he maintained to be caused by the blood in the ventricles coming into forcible contact with that in the great vessels, which had just attained a state of momentary repose. The second sound he alleged to be due to falling back of the blood in the outlet vessels, and its arrest by the valves—not, however, by the vibration of the valves, but on the same principle as the sound is formed in a tube through which water is flowing from a cistern, when its flow is suddenly arrested by turning a cock. Four conditions are necessary for the production of the perfect first sound. 1. Sufficient viscidity of the blood. 2. Sufficient pressure upon the blood. 3. Sufficient force in the circulation. 4 The absence of obstruction at the outlets of the heart. If the first condition—viscidity—be wanting, the blowing sound so often heard in anæmia is substituted. If the second be deficient, the first sound also changes to a murmur, as happens in cases of great hemorrhage, by which the pressure on the remaining blood is much diminished. If the third be deficient, the first sound simply becomes proportionately faint, as proved by the want of sound in fevers when the power of the heart is much impaired. The fourth condition is resolvable into the second, because any obstruction at the mouth of the aorta or pulmonary artery must cause diminished pressure beyond the obstruction. For the production of the second sound less force is requisite, and for this reason, when the heart becomes feeble, the first sound becomes impaired much sooner than the second. Viscidity is not a necessary condition; hence in anæmia the second sound never degenerates into murmur like the first. Murmurs in the heart are nothing more than modified shock or normal sounds, and in their causes all the sounds formed by the circulation of the blood are closely correlated. And since certain conditions are requisite for the formation of a normal or perfect sound, Dr. Leared proposes the term "degenerate" to express any imperfection which would constitute it an abnormal sound. Dr. Leared next dwelt upon an inaccuracy of expression used by medical authors. Speaking of murmurs in the heart, they commonly say, "there was a *bruit de soufflet* with the first or the second sound," as the case may be. This arises out of the false conclusion that normal sounds have their origin in something different from that which produces murmurs. But in that case a sound emanating from anything but the blood itself, whether muscle or valve, would invariably be heard coincidently with a murmur—in other words, with an admitted blood sound—except when the murmur is loud enough to drown the other sound—a rare occurrence. When the subject is thus approached, an incongruity is perceived, and, to elude this, the word "with" when it should be "instead of," is employed. For the normal sounds are not generally accompanied by, but substituted by murmurs. In most cases in which simple murmurs are heard immediately over the left side of the heart, corresponding faint normal sounds may be heard by shifting the position of the stethoscope slightly, so as to place it over the right side of the heart. In considering the question whether a normal sound is supplanted or merely overpowered by the loudness of the murmur, the duplicate origin of the sounds must be kept in view. In making an examination the stethoscope must be fairly placed over the cavity from which the murmur proceeds, and not at the same time partly over the adjoining cavity. Dr. Leared maintained that other changes—besides that of the first sound into murmur—as of its intensity, duration, and pitch, are better accounted for by the *present explanation* than by any other. They are due to variations in the

force with which the blood is impelled, or else in its pressure or viscidity; while by no other means can the complete degeneration of the sound into a murmur of moderate loudness be satisfactorily explained. Viscidity of the blood has been stated to be one of the essentials for the formation of the normal first sound, and, what is very significant, not of the second sound. If water instead of blood circulated in the body, no such sound would ever be heard. The first sound would always be a murmur, while the second would be the same as if caused by blood. It is on this principle that the first sound so commonly degenerates into a murmur in anæmia. The second sound, however, never in this disease undergoes the same change, because at the time of its formation the blood in the great outlet vessels is separated from that in the ventricles by closed valves. The formation of sonorous currents in the intermingling blood owing to its increased mobility is thus prevented. Dr. Leared added, when the advocates of other theories of the normal sounds of the heart afford by them an intelligible explanation of these everyday facts—assuming the heart and its great vessels to be healthy, the blood being less viscid than natural, the first sound is prone to change into a murmur, while the second sound never so changes he will be prepared to question the truth of the present theory. Increase of viscidity of the blood is also probably attended by modification of the first sound, and the result of experiments out of the body shows that this would tend to make the sound more prolonged and less loud than natural. In typhus fever fibrin exists in excess in the blood, and the impairment of the first sound of the heart so often observed in this disease may be in part due to this cause. Diminished pressure, by which is implied that the total quantity of blood in the heart and arteries is less than natural, also causes the first sound to degenerate. Sufficient friction to induce murmur is then also formed between the blood issuing in a less body than natural from the ventricles and the blood in the great vessels. Nothing is more conclusive of this position than the fact that one effect of excessive hemorrhage is to transform the first sound into a murmur. Dr. Marshall Hall's experiments proved that this transformation occurs in dogs, and it may be sometimes observed in the human subject, as in great loss of blood after parturition. Impairment in the quantity of blood, then, produces the same result as impairment of its quality; and Dr. Leared again challenges the advocates of the origin of the first sound from the structures of the heart itself to explain by their theories the change, in this instance, of the normal sound into a low-pitched murmur. In certain cases murmurs are heard equally loud at the base and at the apex of the heart in which, after death, the mitral valve is alone found diseased; whereas, according to theory, the aortic orifice ought to be also diseased. Dr. Leared thinks this may be explained by the diminished volume of blood sent by the ventricle into the aorta in consequence of mitral regurgitation. By this means the aorta would be, in relation to the blood sent into it, in the same position as if a great loss of blood by hemorrhage had occurred. Thus the sound would degenerate into a murmur on the principles explained, and a double murmur, one being regurgitant and the other direct, would ensue. Sufficient force in the circulation is so necessary a condition that whatever theory of the sound is adopted, it must always be a constant factor. Loss of force implies loss of velocity in the blood, but this does not cause degeneration of the first sound: it merely becomes less loud, as in the case of typhus fever. Absence of obstruction at the outlets of the great vessels is the last condition to be considered. This condition is closely allied to one previously considered—namely, that there shall be sufficient pressure on the blood, for an obstruction from atheroma or contraction of the vessel produces diminished pressure beyond it, and then currents are permitted by which the integrity of the shock or natural sound is destroyed. Conversely, any considerable dilatation of the aorta is a cause of lessened pressure, because the proper relation between the blood which it contains and that which it receives from the ventricle is altered. Hence dilatation is a not unfrequent cause of murmur. The production of murmur from diminished pressure is easily illustrated by experiment. Let one end of an india-rubber tube of an

inch diameter, and of moderate length, be connected in a perpendicular manner with a cistern containing water; and let the other end be also inserted into a vessel of water. As soon as any air that may have been contained in the tube has been expelled, the water will flow through it quite noiselessly. If the tube be now somewhat compressed in the middle, and the ear be applied below the point of compression, a murmur will be heard. It will be found also that the murmur is louder here than at any other part of the tube, and therefore formed in this position. But if the murmur be not due to vibration of the com pressed part of the tube, why should it be thus localised? Because the velocity of the water as it passes through the obstruction is proportionate to the pressure upon it—in other words, to the length of the column above it. Here it must be explained that although, if the water be at rest, the pressure upon it would go on increasing in the ratio of the length of the tube, notwithstanding the obstruction, the case is different when the water is in motion. Now, if a tube be obstructed to the extent of half its area, it is manifest that, since it is also full below the obstruction, the more rapidly moving water from above will be driven forcibly through the more sluggish stream below. And at the spot where this first occurs—namely, immediately below the obstruction—the murmur will be most intense; for as this point is receded from, the velocity of the two streams becomes equalised, and the friction between them, which is the cause of sound, vanishes. Doubling or reduplication of the heart's sounds is an obscure phenomenon, of which no satisfactory explanation has been as yet offered. Dr. Leared detailed the various explanations that have been given, together with the objections to them. He has often noticed that when a tap from which water is flowing is suddenly turned, the jarring noise which results is not merely single, as previously described, but sometimes double, from a rebound of the descending fluid and a second concussion. He thinks it possible, therefore, that reduplication of the second sound of the heart may be caused in this way.—*Medical Times and Gazette, Aug.* 31, 1867, *p.* 241.

22.—ON THE USE OF NITRITE OF AMYL IN ANGINA PECTORIS.

By Dr. T. Lauder Brunton, Senior President of the Royal Medical Society, and Resident Physician to the Clinical Wards of the Royal Infirmary, Edinburgh.

Few things are more distressing to a physician than to stand beside a suffering patient who is anxiously looking to him for that relief from pain which he feels himself utterly unable to afford. His sympathy for the sufferer, and the regret he feels for the impotence of his art, engrave the picture indelibly on his mind, and serve as a constant and urgent stimulus in his search after the causes of the pain, and the means by which it may be alleviated.

Perhaps there is no class of cases in which such occurrences as this takes place so frequently as in some kinds of cardiac disease, in which angina pectoris forms at once the most prominent and the most painful and distressing symptom. This painful affection is defined by Dr. Walshe as a paroxysmal neurosis, in which the heart is essentially concerned, and the cases included in this definition may be divided into two classes.

In the first and most typical there is severe pain in the precordial region, often shooting up the neck and down the arms, accompanied by dyspnœa and a most distressing sense of impending dissolution. The occurrence and departure of the attack are both equally sudden, and its duration is only a few minutes.

In the second class, which from its greater frequency is probably the more important, though the pain and dyspnœa may both be very great, the occurrence of the attack is sometimes gradual, and its departure generally *so; its* duration is from a few minutes to an hour and a half or more, and the *sense of impending dissolution* is less marked or altogether absent.

Brandy, ether, chloroform, ammonia, and other stimulants have hitherto been chiefly relied upon for the relief of angina pectoris; but the alleviation which they produce is but slight, and the duration of the attack is but little affected by them.

In now publishing a statement of the results which I have obtained in the treatment of angina pectoris by nitrite of amyl, I have to observe that the cases in which I employed this remarkable substance belonged rather more to the second than to the first of the classes above described.

Nitrate of amyl was discovered by Balard; and further investigated by Guthrie, who noticed its property of causing flushing of the face, throbbing of the carotids, and acceleration of the heart's action, and proposed it as a resuscitative in drowning, suffocation, and protracted fainting.

Little attention, however, was paid to it for some years, till it was again taken up by Dr. B. W. Richardson, who found that it caused paralysis of the nerves from the periphery inwards, diminished the contractility of muscles, and caused dilatation of the capillaries, as seen in the web of the frog's foot.

Dr. Arthur Gamgee, in an unpublished series of experiments both with the sphygmograph and hæmadynamometer, has found that it greatly lessens the arterial tension both in animals and man; and it was these experiments—some of which I was fortunate enough to witness—which led me to try it in angina pectoris.

During the past winter there has been in the clinical wards one case in which the anginal pain was very severe, lasted from an hour to an hour and a half, and recurred every night, generally between two and four a. m.; besides several others in whom the affection, though present, was less frequent and less severe. Digitalis, aconite, and lobelia inflata were given in the intervals, without producing any benefit; and brandy and other diffusible stimulants during the fit produced little or no relief. When chloroform was given so as to produce partial stupefaction, it relieved the pain for the time; but whenever the senses again became clear, the pain was as bad as before. Small bleedings of three or four ounces, whether by cupping or venesection, were, however, always beneficial; the pain being completely absent for one night after the operation, but generally returning on the second. As I believed the relief produced by the bleeding to be due to the diminution it occasioned in the arterial tension, it occurred to me that a substance which possesses the power of lessening it in such an eminent degree as nitrite of amyl would probably produce the same effect, and might be repeated as often as necessary without detriment to the patient's health. On application to my friend Dr. Gamgee, he kindly furnished me with a supply of pure nitrite which he himself had made; and on proceeding to try it in the wards, with the sanction of the visiting physician, Dr. J. Hughes Bennett, my hopes were completely fulfilled. On pouring from five to ten drops of the nitrite on a cloth and giving it to the patient to inhale, the physiological action took place in from thirty to sixty seconds; and simultaneously with the flushing of the face the pain completely disappeared, and generally did not return till its wonted time next night. Occasionally it began to return about five minutes after its first disappearance; but on giving a few drops more it again disappeared, and did not return. On a few occasions I have found that while the pain disappeared from every other part of the chest, it remained persistent at a spot about two inches to the inside of the right nipple, and the action of the remedy had to be kept up for several minutes before this completely subsided. In almost all the other cases in which I have given it, as well as in those in which it has been tried by my friends, the pain has at once completely disappeared. In cases of aneurism, where the pain was constant, inhalation of the nitrite gave no relief, but where it was spasmodic or subject to occasional exacerbations it either completely removed or greatly relieved it. It may be as well to note that in those cases in which it failed, small bleedings were likewise useless.

From observations during the attack, and from an examination of numerous sphy*gmographic tracings taken* while the patients were free from pain,

differing only in degree. Another means of diagnosis has recently been added to our list, viz.: the rise in the temperature of the body in phthisical disease as ascertained by the delicate clinical thermometers now in use. In fever, thermometric observations on the skin have been already found to be of great value, especially as regards the prognosis; but will the thermometer enable us to distinguish between true and spurious phthisis? We think not; for in both there is inflammatory action, which may and will elevate the temperature of the skin. After all, our predecessors, who knew nothing of auscultation or percussion, who were obliged to form their diagnosis upon the general history of the case, and the outward signs they could observe, were not wasting their time, and at the present day we reap the benefit of their observations; we readily acknowledge that without an accurate knowledge of the early details, without a carefully considered history of the symptoms, we may, in spite of our boasted skill with the stethoscope, fall into grievous mistakes.

While, then, we concede that spurious phthisis may be, and often is, perfectly cured, can the same be said of true tubercular disease? If Virchow's doctrine be correct, that the true grey semitransparent tubercle may do and occasionally does, at an early period of its existence, dry up, obsolesce, and cease to be dangerous, can we cure the diathesis, the faulty assimilation, the defective nervous influence, the failure or alteration of the pancreatic juice, which may be the cause of the production of tubercle? Can we decide on the existence of the tubercular diathesis before the perilous matter is formed in the lungs? With certainty we cannot, but we do, with most of the best pathologists of the present day, acknowledge the identity of the scrofulous and tubercular diathesis, and the former exhibits its presence by outward signs often appreciable at a very early period of life. The old physicians were right; the existence of scrofula in a family or in an individual should make us prepare against the invasion of tubercle, and adopt, as far as lies in our power, those hygienic measures which may ward off the disease. Have we, then, with our increased acquaintance with the nature of tubercle, advanced *pari passu* in the treatment of this dreaded disease? Is the mortality from phthisis lessened, or does it not still form one of the most important items in the bills of mortality? Do not the hurry and haste in which we now exist, the wild passion for money-making, the wilder and still more frivolous pursuits of fashion or pleasure, compensate for our better knowledge of tubercular disease? To a certain extent this may be the case; but from the time when Sir James Clark, thirty years ago, published his admirable essay on climate in the treatment of phthisis, a better prospect has been opened to us, and some here may live to see the day when the hygienic and the prophylactic treatment of phthisis will not be left merely to the isolated opinion of the medical attendant, but, like the great popular questions of vaccination, of cholera, and other epidemic diseases, will become a subject of careful legislative enactment. Still it cannot be said that the medical profession has been backward in the attempt to stay by treatment the ravages of phthisis. Every new remedy (and their name is legion) has been carefully tested; and meanwhile great and marvellous has been the change that within the last thirty years has occurred under our own eyes in the therapeutics of this malady. The old shutting up in a room and lowering treatment are exploded; tonics and open air exercise, whenever the latter can be borne, have taken the place of these. Iron and quinine have replaced the lancet and sedatives; it has been found that the cough is moderated while the appetite is improved by these tonics; and assimilation is far more vigorously carried on, than when the digestive powers were weakened by the constant repetition of opiates. Great attention is now paid to the functions of the skin; we have become altogether a bathing and washing people to an extent that our forefathers never dreamed of. That cod-liver oil has had its day and will soon be numbered among the things of the past, we are by no means willing to allow. We regard this remedy as really valuable, as an easily assimilated fat, helping to support the system, *while we strive by other* tonics to alter the tubercular diathesis. It may be

that the appearances of returning health that it produces are often fallacious, but we have ourselves, we are sure, seen many cases where its use has long protracted the disease. Of late years the statistics of phthisis have shown us that the malady is of much longer average duration than was laid down by Louis thirty years ago. We have indeed improved since then in the power of detecting the early stage of the malady; but, even allowing for this, we are disposed to believe that the greater average duration of phthisis at the present day is due partly to the tonic and more rational invigorating treatment employed, and partly to the free use of cod-liver oil. But there is another circumstance which above all has contributed powerfully to prolong the phthisical patient's existence, and in some few cases perhaps to effect a cure. The facilities afforded by railway travelling are now so great that in a few hours the ailing invalid can leave our Arctic winter, and our still more bitter spring, and be enjoying the soft southern breezes and open air exercises on the northern or southern shores of the Mediterranean. The exodus of the healthy and their families from England during the winter and the spring months is as yet only beginning. In a few years, if peace continue to reign on the continent of Europe, it will be infinitely more extensive; indeed we anticipate that no one who can afford a winter's residence abroad will care to stay at home. While the phthisical patient of limited means must, during our winter and spring, remain the greater part of the time within doors, the sojourner at Mentone, or at Hyères, may be out for hours each day in the open air. During the last winter, a season of such severity as will be remembered by us all, the themometer at Hyères only sank one single night to the freezing point. To how many scrofulous and hence possibly phthisical children may not this southern annual journey have been the means of stopping the first advances of tubercular disease, and in how many cases of the already established malady may not valuable lives have been prolonged for months, and perhaps for years.—*British Medical Journal*, *Aug.* 24, 1867, *p.* 146.

24.—ON THE NATURE AND THE MODE OF PROPAGATION OF PHTHISIS.

By Dr. William Budd, Consulting Physician to the British Royal Infirmary.

The following are the principal conclusions to which I have been led regarding Phthisis or Tubercle:—

1st. That tubercle is a true zymotic disease, of specific nature, in the same sense as typhoid fever, scarlet fever, typhus, syphilis, &c., &c., &c., are.

2nd. That, like these diseases, tubercle never originates spontaneously, but is perpetuated solely by the law of continuous succession.

3rd. That the tuberculous matter itself is (or includes) the specific morbific matter of the disease, and constitutes the material by which phthisis is propagated from one person to another, and disseminated through society.

4th. That the deposits of this matter are, therefore, of the nature of an eruption, and bear the same relation to the disease, phthisis, as the "yellow matter" of typhoid fever, for instance, bears to typhoid fever.

5th. That by the destruction of this matter on its issue from the body, by means of proper chemicals or otherwise—seconded by good sanitary conditions—there is reason to hope that we may, eventually, and possibly at no very distant time, rid ourselves entirely of this fatal scourge.

The evidence on which these conclusions are founded is drawn from the following principal sources:—

(*a*) Considerations based on the pathology of phthisis, as showing it to consist in the evolution and multiplication within the organism of a specific morbid matter, with a universal tendency to elimination and casting forth of the same, after the type of zymotic diseases generally.

(*b*) Actual instances in which there was evidence to show that phthisis was communicated from *one person to* another

(*c*) The geographical distribution of phthisis in past and present times, and, especially, its great fatality now in countries which when first discovered by Europeans were known to be entirely free from it.

(*d*) Its much greater prevalence in low levels and among crowded communities, and its entire absence, unless by casual importation, at very high levels,—conditions which are well known to rule, in the same directions, the spread of zymotic diseases generally, and especially of that group in which, as in phthisis, the morbific matter is cast off in a liquid form.

(*e*) Its very high rate of prevalence in convents, harems, barracks, penitentiaries, &c.—that is to say, under the very social conditions which are known most to favour the propagation of diseases of the zymotic group.

Among the data relating to geographical distribution, the following striking facts may be here mentioned:—

1st. When the South Sea Islands were first discovered phthisis did not exist there. Since the aborigines have come into intimate contact with Europeans the disease has not only made its appearance among them, but has become so wide-spread as to threaten their extermination.

The contrast between original entire immunity and present extreme fatality is very striking, and can only be rationally explained by the importation of a new and specific morbific germ.

Try every other supposition, and the facts are inexplicable; make this one supposition, and they are at once explained.

2d. The late Dr. Rush, of Philadelphia, who made very accurate inquiries to determine the point, satisfied himself that when America was first discovered, phthisis was unknown among the native American Indians. Now it is very fatal to them.

The very significant contrast here exhibited between the past and present history of these two races, in respect of phthisis, is exhibited at once, and at the present time, among the negro race in Africa, in different parts of the area of that great continent.

It is well known that negroes are peculiarly liable to phthisis.

Now, everywhere along the African seaboard, where the blacks have come into constant and intimate relation with the whites, phthisis causes a large mortality among them. In the interior, where intercourse with the whites had been limited to casual contact with a few great travellers or other adventurous visitors, there is reason to believe that phthisis does not exist. Dr. Livingstone and other African travellers have given me the most positive assurances on this point.

The idea that phthisis is a self-propagated zymotic disease, and that all the leading phenomena of its distribution may be explained by supposing that it is disseminated through society by specific germs contained in the tuberculous matter cast off by persons already suffering from the disease, first came into my mind, unbidden, so to speak, while I was walking on the Observatory hill at Clifton, in the second week of August, 1856. The close analogy in many quite fundamental points between this disease and typhoid fever had often impressed itself on me with very great force while I was engaged in the study of the latter, and in the preparation of the papers which I have published on it. I now saw with a clearness which had never occurred to me before, that, with the exception of the qualifications necessary for their application to a chronic disease,—for the most part of slow evolution and indefinite duration,—the leading conclusions to which I had been led respecting the propagation of the fever, might be applied with the same strictness to phthisis also.—*Lancet, Oct.* 12, 1867, *p.* 452.

25.—CASE OF ACUTE TUBERCULOSIS.

Under the care of Dr. JENNER, at University College Hospital.

[The following short and interesting case is taken from the reports of practice at the London Hospitals in the Medical Times and Gazette. One often hears of cases of so-called acute tuberculosis, but the term is most frequently

applied to instances of "galloping consumption," when patients die with all the appearances of advanced phthisis after six or eight weeks' illness; but the case related here is of a much more uncommon sort.]

A servant girl, aged 25, was on June 6th rather out of sorts, but nothing particularly wrong was noticed; even on the 9th she only complained of headache, but on the 10th she felt so ill that she was obliged to keep her bed. On the 14th she was brought to the hospital. When admitted, she complained of little beyond a slight frontal headache: yet it was evident that something unusual ailed her, from her pale face, dull eyes, dry, brown, and tremulous tongue, and the sordes on the lips and teeth. But what was it? The condition of the chest was normal, but the abdomen seemed rather full and tender on deep pressure. There were no spots on the surface, whilst the pulse was full, regular, and only beating 72 a minute. The diagnosis lay between tuberculosis and typhoid fever. By evening all aid to be derived from the patient's sensations was removed, for she became totally insensible. The thermometer, also, which might have given some indication, was at fault, the temperature being 103·2°—the temperature of fever rather than of tuberculosis. On the evening of the 15th her pulse was quicker—120—regular and of fair strength, but otherwise she was extremely prostrate, and died on the morning of the 16th, exactly ten days from the time she first felt out of health, and six from the time she took to bed. The mystery which surrounded the case in life was removed by a post-mortem examination, which showed the whole peritoneum thickly studded with tubercles, and the omentum thickened by a deposit of the same kind. The mesenteric glands were enlarged, and the peritoneum round them somewhat vascular; but there was only a small quantity of fluid in the cavity of the abdomen. There were tubercles in the stomach, tubercles in the kidneys, tubercles in the pleura, and many tubercles in the lungs. Tubercles there were also in the pia mater of the brain, about the fissure of Sylvius, and the cerebral ventricles contained an unusual quantity of fluid, whilst the brain matter around was somewhat softened. In fact, it was apparent that the whole body was tubercular, and that the patient had died of acute tuberculosis.—*Medical Times and Gazette, June* 27, 1867, *p.* 90.

26.—ON THE INOCULATION OF ANIMALS AS A MEANS OF DIAGNOSIS IN TUBERCULAR PHTHISIS.

By Dr. William Marcet.

We are indebted to Dr. Villemin for the very remarkable discovery that tubercular phthisis can be inoculated from man to animals; rabbits and guinea-pigs, which are naturally liable to the disease, being fit subjects for inoculation. When I became acquainted with Villemin's papers, I happened to be engaged with an investigation of the expectorations in phthisis, and it struck me that, if his results were correct, by inoculating the expectorations of phthisical patients, containing tubercular matter, to healthy rabbits or guinea-pigs, these animals would become tubercular; thus I thought that a physiological method of diagnosis of great importance might be arrived at, for should the inoculated animals, after a certain time had elapsed, either die tubercular, or, on being killed, exhibit tubercles in any part of their body, there could be no more direct evidence as to the nature of the diseased condition of the patient whose expectorations had been used for inoculation. If the result of the inquiry was found to be in accordance with this view, I had yet to show that the non-appearance of tubercles in guinea-pigs inoculated with expectorations from a supposed incipient case of phthisis might be considered as evidence of the patient's lungs not being tubercular. This second part of the question is more difficult to solve than the first; I am engaged with it at present. Twenty-two guinea-pigs were submitted to experiment. The expectorations of nine different patients suffering from phthisis were inoculated to as many guinea-pigs; in two cases two animals were inoculated respectively *with the sputa* from the same patient. Of these 9

patients, 8 could be safely considered in the second stage of phthisis, the tubercles undergoing softening and being expectorated. One patient appeared to be in the chronic third stage, with cavities contracting. The sputa of one well-diagnosed case of bronchitis was inoculated to a guinea-pig, for the sake of comparison, and one young animal of the same litter as two which were inoculated was kept without being inoculated for a similar purpose. This will account for thirteen of the animals. The remaining nine were used in these experiments as follows:—Two guinea-pigs were inoculated with the expectorations of two cases of doubtful phthisis. One animal was inoculated with blood taken from a body during a post-mortem examination at the Brompton Hospital; another with blood procured from the last animal eleven days after the former operation; two were inoculated with pus taken from the chest in a case of empyema, where paracentesis thoracis had been performed; lastly, three sound guinea-pigs, kept with those that had been operated on, were killed in order to ascertain whether their internal organs were free from tubercles. The following results were obtained from the inoculation of the eleven guinea-pigs with the expectorations from phthisical patients;—One of the animals died three days after the inoculation, obviously from some cause independent of the influence which the inoculated matter might have exerted towards the development of tubercles, and in this case no tubercle could be found in the body of the animal. Six guinea-pigs died, and every one of them exhibited most positively tubercles to a greater or less extent. Of these five died between forty-three and fifty-six days after being inoculated, and one case proved fatal in twenty-one days, on February 7, the cold weather having obviously weakened the animal, and contributed to the rapid termination of the disease; in the present case the guinea-pig's spleen alone was found tubercular. Four other guinea-pigs were killed at periods varying from forty-seven to fifty-four days after inoculation, and tubercles were found in every one of these animals. There are seven more cases of inoculation to account for—1st. Inoculation with expectorations from a patient exhibiting doubtful signs of tubercles. Results: Death two days after inoculation from the effects of the operation; no tubercles found. 2nd. Inoculation from another doubtful case of phthisis; animal killed fifty days after the operation; no tubercles found. 3d. Inoculation with blood from tubercular human dead body; animal killed eighty-three days after inoculation; tubercles found in lungs, liver, spleen, lymphatic and mesenteric glands. 4th. Inoculation with blood from last animal eleven days after the operation; guinea-pig killed seventy-two days later; no tubercles. The stage of the disease at which the blood was taken may have been too early to be productive of tubercles; hence the negative result. 5th and 6th. Inoculation of the two guinea-pigs from the case of empyema; result, one animal died eight days, and the other was killed fifty days, after inoculation. The first guinea-pig dying so soon appears to show that it was tubercular when operated, which does away with the value of the experiment. The second exhibited tubercles in the lungs, liver, spleen, lymphatic and mesenteric glands. 7th. Inoculation with the expectorations from the case of bronchitis. The animal was found, on being killed eighty-two days after inoculation, in no way tubercular. Finally, not one of the three animals left sound, and which had lived with those that had been inoculated, presented, on being killed and examined, the slightest trace of tubercles, although they had been for about two months under precisely the same conditions as the others; and the young guinea-pig of the same litter as two of the inoculated ones, and which was preserved healthy, is now growing rapidly, and appears in perfect health. I may add that Mr. M. J. Salter, who assisted me in these experiments, has witnessed every one of the results I have recorded in the present communication. In conclusion, I believe the results of the experiments and observations related in the present paper to be as follows:—1st. The inoculation of guinea-pigs with the expectorations of patients suffering from phthisis will, at all events in a certain stage of the *disease*, and possibly throughout, give rise to the formation of tubercles in *the operated animals*. 2nd. If two or more guinea-pigs inoculated with human

expectorations, brought up by coughing, should die from tubercular disease, or should, on being killed at least thirty days after inoculation, exhibit tubercles, this may be considered as a direct and positive evidence that the person whose expectorations were inoculated was suffering at the time from tubercular phthisis. 3d. If two or more guinea-pigs be inoculated with the expectorations coughed up by a person considered to be in the third stage of phthisis, and if these animals do not die of tubercular disease, or exhibit any tubercles when killed at least fifty days after inoculation, it may be considered that in the present case the softening of tubercles and the secretion from the pulmonary cavities are arrested, the patient being in a fair way of recovery. 4th. Other materials besides the pulmonary expectorations taken from the human body in certain, if not in all, stages of phthisis, as blood and pus, appear to be also possessed of the power of causing the formation of tubercles in guinea-pigs when inoculated to these animals. 5th. The spleen appears to be the first, and the lungs one of the last, organs in guinea-pigs to be attacked with tubercular disease.—*Medical Times and Gazette, Sep.* 28, 1867, *p.* 361.

27.—ON THE INFLUENCE OF ALPINE CLIMATES ON PULMONARY CONSUMPTION.

By Dr. Hermann Weber, Physician to the German Hospital.

Case 1.—H. F., aged 31, a German, came to London in 1852, had a chronic cough in 1853, and hemoptysis in the summers of 1854 and 1856. He spent then seven months in Cairo, where he became better, without, however, entirely losing his cough. He had again hemoptysis in March 1857 in Cairo, and much cough during the summer of 1857 in London. There was improvement during the following winter at Cannes and Nice; but he was much worse after his return to London. He was first seen by myself in July 1858. He had lost 29 lb. in three years. There was dulness on the left side from the apex to the fifth rib, with bronchophony and crackling rhonchus in the supra- and infra-clavicular spaces; and an analogous condition on the back of the same side. Nothing abnormal was discovered on the right side. He had shortness of breath. Pulse 85; thoracic expansion, 35 to 36½ inches. He went to Valparaiso, and had there again hemoptysis. He spent then eight months on the Peruvian Andes (at an elevation of between 9,000 and 10,000 feet), and gained flesh and lost cough. He went afterwards to New Orleans, where he again began to cough and lose flesh. He was sent thence to the table-land of Mexico, where, to his own feeling, he entirely recovered his health. When seen by me in the autumn of 1860, the dulness on the left side reached only from the apex to the third rib. This portion was much flattened; and there was absence of vesicular breathing, but no bronchophony, and no rhonchus. He had gained 24 lb. in twenty-seven months. Pulse 66; expansion of chest, 36 to 38 inches. He remained well until the spring of 1866, when he again began to cough, was often feverish, lost much flesh, and had some hemoptysis. When seen in the beginning of October 1866, the old affection of the left side had remained almost unchanged; but on the right there was dulness from the apex to the fourth rib, with bronchophony and occasional crackling rhonchus in the subclavicular space. He had lost 20 lb. in twelve months. Pulse 95; chest-expansion, 35½ to 37 inches. He went to the Valley of Jaujo, in the Peruvian Andes, and lost his cough almost entirely; and when seen in June 1867, he had gained 15 lb. The dulness descended on the right side only to the second rib. There was no rhonchus, but scarcely any respiratory murmur over the dull space. Pulse 74; expansion of chest, 36 to 38 inches.

The rapid improvement repeatedly obtained in this case by the removal to elevated regions is very striking; and it is also important to remark, that the improvement effected by the stay at Cairo and in the South of France, was neither so great nor so lasting as that from the mountain sojourn.

Case 2.—B. D., from the lower parts of Switzerland, came to London in 1864, aged 22. He was much depressed by the fogs and dulness of the

atmosphere. He had repeated attacks of bronchitis during the winter; and in the spring of 1865 he coughed and became emaciated during the summer. There was slight dulness in the left clavicular and infraclavicular region with diminished respiratory sounds. He spent the winter at Bordeaux and at Cannes; he never lost his cough entirely, and continued to lose weight. He returned to London in May 1866. Dulness extended on the left side from the apex to the third rib. He had, early in June, an attack of sore-throat, which was with him the usual forerunner of bronchitis; he became feverish and at the end of the month a pleuropneumonic affection was ascertained in the lower part of the right side, which gradually crept upwards to the upper third of the scapula. By the end of July, the pyrexia had almost ceased, under a treatment of milk, quinine, and cod-liver oil; but the dulness over the back of the right side had not yielded. There was crepitant rhonchus over the upper part of the dull space, and absence of respiratory murmur over the lower; pulse 92 to 100; expansion of chest, 31½ to 33 inches. The dyspnœa was great at every exertion; frequent cough, with about two and a half ounces of mucopurulent expectoration. He had lost 27 lb. in as many months' stay in London. In this state he went, towards the end of July, to the Righi (Kaltbad and Scheideck), lived there almost entirely in the open air, and drank about two quarts of milk every day. In less than six weeks he was able to take considerable walks; and in October he had lost his cough almost entirely. He spent the winter in different parts of Switzerland, usually above 2,500 feet above the sea-level. He had not a single attack of sore-throat and bronchitis; and when he returned to London, towards the end of May, the right side was quite normal; the left side was in front, the supra and infra-clavicular spaces, slightly sunk in; there was moderate dulness and scarcely perceptible breathing from the apex to the second rib. He gained 19 lb. in weight during ten months, and felt in every respect very well.

Case 3.—B. K., aged 21, had pneumonia of the left side about ten years ago. He came to London in spring 1865, and began to cough and lose flesh in the winter 1865–66. Hoarseness supervened in spring 1866, and the cough gradually increased. When seen in July, he was thin and pale, with circumscribed red cheeks; he had chronic swelling of the tarsal portion of the eyelids; pulse 105 to 110; respirations 26 to 30; temperature every evening slightly increased. There was fresh pneumonic affection of the lower part of the left side; dulness and absence of respiratory murmur on the right from the apex to the third rib. The evening pyrexia gradually subsided under perfect rest and milk diet; but the pneumonic affection slowly crept upwards, the dulness extending on the posterior part of the left side from the bottom to the upper third. When he left England towards the end of August, there was crepitation with slight bronchophony over the greater part of the left scapula; and the older affection of the right side had remained unchanged. He was advised to go to the Davos, but went first to Germany; and only after having been attacked by fresh colds he repaired in November to Davos am Platz, where he stayed until the middle of April, and, in spite of a rather unfavorable winter, entirely lost his cough, and gained 7 lbs. in weight; and the spirometer showed an increase of pulmonary capacity amounting to more than 900 cubic *centimètres*. According to Dr. Unger's report at the time of B. K's departure from the Davos, nothing abnormal could be discovered on the left side, except, perhaps, slight dulness in the lowest part; and the dull space on the upper portion of the right side was likewise diminished. In addition, the affection of the eyelids, which had existed for several years, had entirely disappeared without remedial interference.

With regard to the experience collected at Davos am Platz, accurate records date only from February 1865, when Dr. Unger, himself formerly consumptive, and then not yet quite recovered, came to the village, accompanied by a young gentleman with extensive lung-disease. Between February 1865 and March 1867, 35 patients with consumptive affections had been *under the observation* and treatment of Drs. Spengler and Unger, who have

kindly furnished me with a report; of which, however, I can only give a very short summary. Of these 35 cases, only 1, belonging to a consumptive family, offered no distinct signs to auscultation and percussion, although the spirometer showed a considerable impairment of the respiratory capacity. Of the other 34 cases, 19 were in the first stage, with distinct deposits; 15 in the second, with cavities. Of the 19 in the first stage, 2 had deposits in both sides and some had other complications, as pleuritic effusion; 7 of the 19 left cured, 6 improved, 6 remained under treatment; 18 of the 19 had gained in weight; 8 had previously had attacks of hemoptysis, which only in one of them returned in a very slight degree during the stay. Attacks of fresh colds were remarkably rare in all of them, Of the 15 in the second stage, only 2 were free from fever; 3 of them had pleuritic exudations; 5 of the 15 died (3 from gradual consumption, 2 from hemoptysis); 1 left worse than he arrived; 1 improved; 2, who had arrived with considerable pulmonary distension, are steadily improving; and 1 is regarded as perfectly convalescent. It is worthy of remark, that diarrhœa did not occur in any of them, and that the night perspiration in almost all cases disappeared after a short stay. In one case only, careless exposure led to a croupous pneumonia of the left lower lobe, with a favourable termination on the fifth day.

The treatment adopted at the Davos consists in the use of much milk, and light nourishing food; a moderate amount of wine, principally the red wine of the Valteline; and graduated exercise, first on level, later up hill. The cold douche is likewise in many cases used with advantage.

Those who consider the facts related will probably not deny that they offer testimony in favour of mountainous climates. The two cases which at a later period ended fatally are particularly instructive, by showing that a curative process had taken place during the stay in elevated regions. Both cases, and probably also the three others related, belonged to the class described by Addison under the head of pneumonic phthisis. It is probably on these low forms of catarrhal pneumonia, with a tendency to chronic infiltration, cheesy transformation, and formation of cavities, that the Alpine climates have a truly curative and prophylactic influence.

The question, how the mountain climates exercise so favourable an influence, can probably not be answered without entering a field of theories, which I wish to avoid as much as possible. There are, however, some undeniable facts. Thus the air of elevated regions is lighter, more rarefied, and cooler; and it is usually free from the foreign admixtures found in towns, and also free from the various kinds of malaria. The food, the occupation, and the whole manner of living, are generally different from those prevailing in large towns and much inhabited districts. The influence observed by most people on their removal to mountainous climates is an improvement of appetite, digestion, formation of blood, and nutrition in general. Exhilaration and increased inclination to exercise are likewise frequent attendants of the change. All the influences enumerated to which others might be added, are of importance in the consideration of the question before us. I will only allude to one; viz., the rarefied air. A given volume of air contains absolutely less oxygen and other constituents at an elevation of 5,000 feet, than at the level of the sea. The movements of the thorax must, therefore, be increased, if the same amount of oxygen is to be introduced; or a smaller amount of oxygen will be introduced if the movements of the thorax remain the same. It has been asserted, but, as far as I know, not yet been proved experimentally, that the inspirations on mountains are deeper. Healthy persons, at all events, usually have an inclination to take deeper inspirations; and the broad chest of mountaineers may be regarded as an effect of increased respiratory movements. If this view were correct, it might be asked, how far this augmented action of the organs of respiration, and, as a necessary consequence, those of circulation, influences the sanguification and all the processes of nutrition—how far, I say, this mere increase of chest-expansion would go to explain the facts before us. All influences, climatic, physical, or mental, by which the breathing is habitually increased, appear to act beneficially in the tendency to consumption; while all those

causing diminished breathing seem to favour, in man and animals, the development of scrofulous and catarrhal inflammations, and of consumptive diseases of the lungs in general.

Although I have already occupied too much space, I must still add a word of caution. Not all consumptive constitutions probably are fit for the Alpine climate, and not all the stages of consumptive disease may be benefited. The invalid ought not to be allowed to go to any Alpine climate he likes, and do there as he pleases; but he ought to be directed to a place where he can have the advice of a medical man who will carefully superintend his diet, his exercise, and his whole manner of living. Many invalids lose the benefit obtained in the commencement by over-exercise and careless exposure; and the very help which these climates have given them may be turned by them to their ruin. It is further necessary that the invalid be sent in an early stage of the disease, and that he remain long enough, if possible, to regain the full capacity of healthy lungs; and that he be not allowed to return too early to his former unhealthy residence and occupation. On all these points, however, I cannot suggest better advice than that embodied in the excellent works of Sir James Clark on the *Sanative Influence of Climate* and on *Pulmonary Consumption.—British Medical Journal, Aug.* 24, 1867, *p.* 148.

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28. ON THE ACTION AND USES OF PHOSPHATE OF SODA IN SMALL DOSES.

By Dr. Wm. Stephenson, Extra-Physician to the Royal Hospital for Sick Children.

[Phosphate of soda is usually described as a mild saline aperient. Its action is such, however, only when given in large doses, and in the present day it is but rarely prescribed.]

It is an important salt in the animal economy, and one often sadly deficient in the food of children. A further reason for its use he bases on the experiments of Dr. Marcet, who first pointed out that the fatty acids are converted into an emulsion by its agency, and that they thus may become more easily assimilated. This is certainly a point deserving of more attention at the hands of medical men than it has received. Further than this, I have not found any special therapeutic action ascribed to it when administered in small doses. That it has such, however, it is the object of this paper to show.

My attention was first drawn to it by seeing it frequently prescribed by my colleague, Dr. Ritchie, who uses it according to the recommendations of Dr. Routh. The first case I gave it in was for a child, four months old, who was being artificially reared on the bottle, and although healthy in every other respect, was somewhat confined in the bowels, having but one motion each day, copious, but dry and white, exactly like coarse white chalk and curdled milk. A pinch of the phosphate of soda was ordered to be added to each bottle of milk. The result was striking, for the very next day the motions were in every way natural and healthy. I had afterwards several occasions of observing in this child the same action under similar circumstances.

The extensive field for observation afforded me at the Children's Hospital has enabled me further to investigate the matter; and in order to have as few disturbing influences as possible, the remedy was always prescribed alone, the parents being directed to put a pinch of the powder into each article of food the child received; in this way four or five grains were administered each time. The class of cases for which it was ordered were wherever, from an unhealthy character of the motions, the visceral secretions seemed to be abnormal. The first point that became apparent was, that in a great majority of cases where the colour of the stools was abnormal, the natural yellow appearance was restored. This was the only appreciable result in many cases; but, at the same time, it was observed that some forms of diarrhœa at once yielded to its action. Several cases of chalky motions, similar to *the one* narrated, have been treated with the same beneficial results; and

now, in this disorder, I have entirely discarded gray powder, and rely on the phosphate alone. Thus far an action of the liver is clearly indicated, which becomes confirmed by the result in the following cases of jaundice:—

Case 1.—Wm. P., a robust child, 4 years of age, was brought to the hospital suffering from jaundice. The skin and conjunctiva were of a deep yellow colour. Four months before, he had an attack of scarlatina, and since then he has often complained of pain in the belly, his bowels being more or less deranged, motions sometimes white, never a natural colour. Six days ago he turned sick, and vomited for two days; no purging; great thirst. No bilious-looking matter in the vomit. On the second day of the attack his skin was observed to be yellow, his stools white, and urine high coloured, staining whatever it touched. Ordered 30 gr. phos. sod. in his food each day. The next morning his motion was slightly yellow, and water much clearer. On presenting himself the following day, a marked improvement in his colour was perceptible, and in a few days he had quite recovered.

Case 2.—The notes of the following case were supplied by Dr. Ramsay, my assistant, who attended the child:—

On the 1st of February, I was called to visit a little boy of 2 years of age, whom I found to be suffering from well-pronounced jaundice. The mother believed the jaundice to have been caused by the child's "taking a draw" from his father's tobacco-pipe. His skin and conjunctiva were deeply tinged with bile. The urine was of dark porter colour, and the stools were pale and clay-coloured. He was languid and dull, but complained of no local uneasiness. I ordered him to be fed upon bread and milk for a day or two, and to have as much of the phosphate of soda as lay on a threepenny-bit night and morning. At next visit, *i. e.*, on the following day, I found that, having lost the prescription, my patient had had none of the phosphate. He was still more deeply jaundiced than when I first saw him, and the lassitude had increased. Again ordering the phosphate, I left, and did not return till the next day but one. There was a marked improvement. The skin and conjunctiva were nearly normal in appearance, the urine was of a natural colour, and the stools had regained the appearance of healthy fæces. I only saw him once subsequently, when I found him in perfect health.

Jaundice in children, however, may pass away of itself; too much weight, therefore, must not be placed on such cases. The improvement in these two instances was certainly immediate upon the administration of the medicine, and may accordingly be taken into consideration along with other cases of a different character where the liver is involved.

In the children we find diarrhœa associated with inaction of the liver, as indicated by the uniform white colour of the motions. The following cases illustrate this condition:—

Case 3.—A boy, 3 years and 4 months old; can neither walk nor speak, and was 14 months before he cut his first tooth. He has been very subject to diarrhœa, but looks well, and is not emaciated, although soft in the flesh. 19th January. Has had diarrhœa for six weeks; motions uncountably frequent, quite watery, pale white, never yellow, and having a strong smell. Portions of egg have been observed to pass through the bowels; vomits occasionally; has a very poor appetite and great thirst. Ordered phosphate of soda.

24th. Bowels have been moved only three times in the twenty-four hours; discharges still watery in the morning, but stiff through the day, slightly yellow, and more natural in appearance. He is taking his food better, and has not so much thirst.

2nd February. Bowels still rather loose in the morning, otherwise natural; he has no thirst; appetite good, and he is rapidly gaining strength.

Case 4.—A girl, 18 months old; weaned at 7 months. 10th April. Has had diarrhœa for two months; six or eight motions each day for the last two weeks; they have been uniformly white in colour and very watery; vomits occasionally. Ordered phosphate of soda.

12th. An hour after taking the powder, the motions became yellow; they

are still watery, but only twice in the day. Vomiting continues. To continue the powder, and give pepsine wine after each meal.

17th. Vomiting stopped; bowels again loose and stools white. The phosphate of soda had not been stopped, but the quantity lessened. Ordered to increase the dose.

19th. Motions are again healthy in colour, and now of natural consistence. —*Edinburgh Medical Journal, Oct.*, 1867, *p.* 337.

29.—ON THE INHALATION OF THE VAPOUR OF CHLORIDE AMMONIA IN ITS NASCENT STATE.

By Dr. Hermann Beigel, Physician to the Metropolitan Free Hospital.

[Ammonia is a remedy of great value in all catarrhal diseases of mucous membranes in general, and of those of the respiratory tract especially. Galen used to use the remedy locally, but in a very primitive way. He directed his patients to burn pieces of hartshorn and inhale the vapour proceeding therefrom.]

It was Alfred Smee who not only gave distinct indications for the local application of ammonia, by pointing out the diseases in which he had applied it with benefit, but who likewise constructed an apparatus for its convenient application. Mr. Smee's paper and woodcut of the apparatus were published in the London Medical Gazette of 1843 (vol. ii., p. 59). "The inhalation of so stimulating a gas as ammonia," says Smee, "is well known to be at first sight perfectly startling to those who have never either tried it on their own persons, or never seen it applied by others; but it is really in many cases, with proper management, a simple and one of the most delightful remedies that can be employed." The diseases in which Smee principally used the local application of ammonia were, chronic hoarseness, and old standing cases of asthma; "especially in those in which the medical man considers that the internal use of the sesquicarbonate of ammonia is indispensable, in which the extremities are cold, the pulse feeble, and the general vital powers depressed, the local application of ammonia is particularly grateful; the patients feeling, as they describe it, a glow after its exhibition, and the warmth first imparted to the lungs extending by degrees over the whole system." Smee likewise recommends the gas as affording great relief and comfort in a relaxed, swollen, and apparently semi-œdematous state of the mucous membrane, which supervenes from remaining in crowded, overheated, and ill-ventilated rooms, where every person not only inhales his own breath over and over again, but is under the necessity of breathing his neighbour's also.

Powerful and beneficial as ammonia is as a remedy, yet it is connected with many inconveniences when applied as vapour spontaneously exhaled from solutions of ammonia. In this state, even if sufficiently diluted, violent symptoms follow each application; and, the gas being rapidly absorbed by the mucous membranes of the respiratory apparatus, the local effect cannot easily be controlled.

A far better plan of applying ammonia to the organs of respiration, therefore, is the one of Dr. Lewin, consisting in making use of the nascent state of chloride of ammonia, in which state it forms a vapour, and can readily be inhaled. But this vapour consists, in fact, of the atoms of a solid body, which, when inhaled, penetrate into the minutest vesicles of the lungs, and, becoming precipitated on the mucous membranes of the organs of respiration, afterwards get absorbed, thus exercising both a local and general effect.

This affords many important advantages. But before enumerating them, it will be necessary to explain the construction of the apparatus which I employ for the above purpose. It consists mainly of three bottles, as used in chemistry for the evolution of gases: one, containing some liquor ammonia, and another, containing some hydrochloric acid—according to their chemical *proportions,—are connected* with a third, which is about half filled with pure

water. From the last bottle the inhaling tube projects. The whole apparatus is put into a box for portability.

The patient raises the mouthpiece of the tube to his mouth and uses it exactly in the same manner as a pipe is used by a smoker. With each draught, ammonia, escaping from the first bottle, and by hydrochloric acid gas from the second, combine, the moment they meet in the third, to form chloride of ammonia, which gets washed in the water of the last, and deprived of the superfluous particles of the two constituents.

The apparatus, which I have had made by Messrs. Krohne and Sesemann, of Whitechapel-road, is extremely simple, and requires no other attention than to renew from time to time the fluids. The advantages of this mode of local application of ammonia are various.

1. The vapour of chloride of ammonia thus smoked *in statu nascente*, may be swallowed or inhaled without any inconvenience: in the former instance becoming absorbed by the stomach; in the latter, being in contact with the mucous membranes of the mouth, pharynx, larynx, trachea, bronchial tubes, and, according to the force of inspiration, it penetrates into the minutest vesicles of the lungs.

2. The vapour being precipitated only after a few seconds' stay in the respiratory organs, it is in our power either to exhale it again after acting for a short time on the mucous membrane, or, by keeping the breath for a few seconds, to allow the vapour to precipitate on the mucous membranes.

3. A deep inspiration being required to drive the vapour forth to the finest air-tubes and lung-vesicles, it is to a certain degree in our power to act only on the pharynx and larynx, or to extend the action to the bronchial tubes and lungs.

4. By altering the proportions of the hydrochloric acid and the ammonia, the latter may be allowed to exist in a free state, and to exercise irritating effects. Should it, on the other hand, be required to deprive the vapour of all superfluous particles of ammonia, it would only be requisite to add some hydrochloric acid to the water in the third bottle; thus establishing two sources for originating chloride of ammonia.

5. If balsamic mixtures or volatiles be added to the water—for instance, essence of turpentine, camphor, &c.,—the efficacy of the smoke may be increased in appropriate cases.

6. The smoking and inhaling of chloride of ammonia is not in the least disagreeable.

It is true that a certain amount of practice is required in order to inhale the smoke well; but this practice is acquired very soon indeed. The effect of these inhalations, in many cases, shows itself very rapidly; and I have seen instances of bronchial catarrh in which relief was felt after inhaling a few times only. Hoarseness, proceeding from a catarrhal state of the vocal cords, has likewise been relieved in some instances in which local application of astringent solutions have been without any effect. Difficult expectoration becomes often soon easy; and the vital capacity increases if it has been diminished by an undue action of the mucous membranes of the organs of respiration.—*Lancet, Oct.* 26, 1867, *p.* 512.

30.—ON INHALATION OF ATOMISED FLUIDS.

By Dr. Hermann Beigel, Assistant-Physician to the Metropolitan Free Hospital.

[Dr. Beigel, in Chapter ii., Part I, of the work from which the following extract is taken, after discussing the question as to whether the atomised fluids really enter the minute passages, observers:]—

"*It is, therefore, confirmed beyond doubt that atomised fluids enter the respiratory tract and penetrate into the very cells of the lungs; that, therefore, by means of inhalation, remedies most appropriately and successfully may be applied to the organs of respiration.*"

[In Chapter v. he observes:]—

The medicaments *which can be atomised*, and which have been in that con-

dition found to be beneficial in diseases of the respiratory organs are, according to Dr. Beigel:—*Nitrate of silver*, dose three to five grains in an ounce of distilled water, serviceable in inflammatory conditions of the pharynx and larynx; *nitrate of aluminium*, two to five grains in an ounce of water, first used by Dr. Beigel for inflammation, and also in nervous affections of the larynx and trachea; *tannin* astringent and styptic, dose one to ten grains in an ounce of water: *alum*, one to twenty grains as above; *sesquichloride of iron*, five to twenty-five minims in an ounce of water; *acetate of lead*, three to ten grains as above; *sulphate of zinc*, one to ten grains; *common salt*, *tincture of opium*, *liquor arsenicalis*, *pure water*, *glycerine*, *lime-water*, one part of lime to one hundred of water; and *cod-liver oil*. Dr. Beigel further mentions the salts of iodine, chlorine, and bromine, as recommended by other authors.—*Dublin Quarterly Journal*, *Aug.*, 1867, *p.* 116.

DISEASES OF THE ORGANS OF DIGESTION.

31.—ON DIPHTHERIA.

By Dr. James Alderson, President of the Royal College of Physicians, Physician to St. Mary's Hospital.

I will first review the symptoms of the disease. The first setting-in very rarely comes within the observation of the physician. As far as we can tell, it always begins like an attack of severe cold accompanied with fever, and sore-throat. The illness progresses very rapidly, with a feeling of great prostration. When medical aid is first called in, the appearance of the throat varies much in different cases. In the greater number of them it is only a superficial ulceration of the tonsils; these are the mild cases, called, for the name's sake, diphtheria, but which I should call the sore-throat of mild scarlet fever, or in some instances quinsy, and which require the treatment proper to those complaints. Some of these cases terminate in suppuration. Upon the naturally to be expected recovery of these cases many of the reputations which are claimed for the cure of diphtheria are really founded. In many instances, however, the treatment by caustics and stimulants, which have been used in compliance with the idea that diphtheria especially required them, will have made only bad throats out of simple cases which would have subsided under gentler treatment.

It is not, however, of these simpler cases that I wish to speak, but of those of true diphtheria, in which the symptoms advance with a degree of rapidity which calls for prompt and vigorous measures. The real characteristics of this disease are the formation of a false membrane covering the tonsils and extending over the soft palate, and the quickness with which the membrane forms, threatening an extension to the pharynx and larynx. The patient at this period breathes with difficulty, and has no power to swallow. Liquids are immediately returned by the nose, and solid food of course is not attempted. The fauces are swollen, and in a state of what we used to call, and what I still call inflammation; it is a sort of inflammatory œdema, which is accompanied by the usual pain attending such a condition. There is a large secretion of mucus, and the glands of the neck are frequently affected; there is always a quick small pulse; in short, every evidence of fever localised in the throat. This tumefaction of the throat, extending to the cellular tissue outside, with glandular enlargement, is the most unerring mark of internal lesion to a serious extent.

The specific character of diphtheria is thus described by M. Bretonneau; you will observe how it coincides with the symptoms of the case which you have seen so lately:—"At the beginning of the disease a circumscribed redness is perceived, covered with a semi-transparent mucus. The first layer, which is slight, supple, and porous, may be still further raised up by some portions of unaltered mucus, in such a manner as to form vesicles. Often in *a few hours the* red spots extend perceptibly from one to another, by *continuity or by contact*, like a liquid which is effused on a flat surface. The

concretion becomes opaque, white, and thick; and it assumes a membranous consistence. At this period it is easily detached, and does not adhere to the mucous membrane, except by some very slender prolongations of concrete matter, which penetrate into the muciparous follicles. The surface which it covers is generally of a light-red tint, with points of a deeper red. This tint is more vivid at the periphery of the spots. If the false membrane leaves the surface uncovered, the redness which was obscured by the exudation returns, and the points of a deeper red allow the blood to transude. The concrete membrane is renewed, and becomes more and more adherent upon the points which had been first attacked. It often acquires a thickness of several lines, and passes from a yellowish-white colour to brown or black; and blood oozes out still more freely. All attempts to remove the membrane at this stage cause blood to ooze out freely."

Now, since we have seen that the one marked state of throat which tends so rapidly towards destruction is the formation of a false membrane, we ought to inquire how this membrane is formed, and to what morbid change it is to be attributed. It is my opinion that its first beginning must be referred to the inflamed—that is, gorged—state of vessels; and that this engorgement is only an aggravated condition of that same state of inflammation which, in a slighter degree, terminates in the milder form of sore-throat. The usual explanation of the existence of the membrane is that it is an exudation from the mucous glands and follicles. No doubt that is correct; still this statement affords no clue to the nature of the first stage of the disease, and, therefore, affords no instruction as to a rational mode of preventing its rapid progress. To my mind the gorged and inflamed vessels ought to be relieved in the first instance. As the progress is very rapid, it will of course not be possible to attempt this relief by any slow or gentle means; still the system of relief must be followed in opposition to that of irritation.

Topical depletion by the application of leeches has reason to commend it, and the testimony of success in all the cases in which I have tried it. Even after the formation of the membrane I have often seen the happiest results. The two cases which are the subjects of this lecture are both most successful instances; but I shall refer to the particulars of these cases, with the accessory treatment, afterwards, as I wish now to insist on the broad principle of treatment by relieving the inflammatory state in the early stage, in opposition to that of irritants, stimulants, and tonics.

It is not desirable to continue the depletion longer than is called for to check the inflammatory action. The tendency in this disease to extreme debility and exhaustion is very great, but at the same time it has been most strikingly evident, in all cases which I have treated in this manner, that the rebound towards strength and convalescence succeeded immediately on relief attained through the leeches.

It seems a strange idea that return to a healthy condition of parts could be induced by setting up a fresh inflammatory action,—the application of strong caustics and stimulants and corroding acids being at the same time especially objectionable to a part so closely contiguous to the larynx, to which any extension of inflammatory action would be destructive to life.

The use of tonics, if less dangerous, has no reasonable excuse to recommend it. It seems, in fact, that to trust to this last class of remedies must be a mere delusion, because, in true diphtheria, as you have seen in the instance now before you, there is no power whatever to swallow; all attempts to pour down liquids merely result in their being returned through the nose. It is natural enough for surrounding friends to crave that the failing strength may be supported by nourishment and wine, and it is still more natural that they should believe the suggestions of medical attendants, that it is absolutely necessary to pour in sustenance; but it is worse than wasting time, in the early stage of the disease, to try to get in succor by a channel through which it could not possibly proceed. In fact, the patient is not in danger from want of food, but from the progress of the malady.

Whilst deprecating the recourse to caustics, it is but fair to state the theory on which those *proceed who* adopt that plan. The idea is to destroy

the mouths of vessels which are in a diseased state, and to set up a new and healthier action. This is an old practice, founded on sufficient reason—a practice which, in indolent sores, where vital parts are not immediately concerned, has much success; but in this case, where the disease is in an active state, caustics applied to highly gorged and inflamed vessels must aggravate the diseased condition. Here, again, I must call your attention to the immediate proximity of vital parts, obviously rendering the use of caustics and stimulants a dangerous expedient, for the mere swelling and irritation of surrounding parts might be attended with serious consequences. Besides this the great temporary distress to the patient is much to be deprecated in the excited and febrile condition.

French works are full of specific treatment—viz., by strong hydrochloric acid and by caustics, with stimulants and tonics. I do not think the French sufficiently look back to a comprehensive classification of disease; they fly off to the consideration of new mysterious forms, and, having laid down certain dogmas as to treatment, bolster up their practice by unlimited assertions.

The reasons given by Bretonneau for the abandonment of the depletory system are at least curious. He gives, amongst others, two cases which are very significant:—

"A little girl 6 years old, of delicate constitution, had twenty-two leeches applied, at twice, with a short interval; the blood flowed abundantly for twelve hours. Two pounds of ipecacuanha syrup, with tartar emetic, were administered in the space of three days; after the sixth day, two grains of calomel were given every half hour, and during the night mercurial ointment was rubbed in under the arms and back of the neck." After twenty hours of slow asphyxia life was quietly extinguished. The last portions of calomel were not swallowed, and the skin had not sufficient warmth to dissolve the mercurial ointment.

"Another child, 6 years old, had twelve leeches applied, with the result of an abundant flow of blood and some relief to deglutition. Encouraged by this, a second application was ordered, and blood continued to flow for several hours. The following day, there was no longer any doubt that the air passages were attacked by diphtheritic inflammation. The child died."

It would almost be an affront to point out to you this absurdity. I have no doubt you are already aware of the great danger of over-leeching children. Vigorous as I should myself desire to be in the endeavour to arrest the progress of croup, destructive measures such as these can scarcely be credited, were it not for the published testimony.

As an example of the effects of the application of corroding, concentrated acids, I quote the following case from the same work. The child was only eight months old, and several (four) of the same family had already died of diphtheria:—

"On the third day the back of the mouth was covered with a grayish incrustation. A sponge moistened with concentrated hydrochloric aid was applied to the isthmus of the throat, and two leeches to the mastoid processes. The same treatment was followed for three days, and the application of concentrated acid repeated night and morning. Death took place at the commencement of the sixth day. It was suspected that the applications of hydrochloric acid had been too frequent and too strong."

I need scarcely detail to you the line of practice that I adopt as the rational sequence of these my opinions regarding the nature of the disease. General depletion in diphtheria would be wholly out of place; no one would think of it when collapse of power follows so rapidly on the inflammatory stage. But topical relief to the gorged state of the vessels by the moderate application of leeches is perfectly available. The leeches should not be placed too near the part affected, in order to avoid the consequence of effusion beneath the bites. A slight puncture of the membrane by a guarded lancet, with a view to give exit to the bloody serum and mucus, and to permit the ingress of a cleansing lotion, is also very useful. The lotion I

generally use is a single grain of the sulphate of copper to eight ounces of water, applied as a gargle.

I would recommend the greatest care in handling the throat, and especially to avoid removing the membrane. It stands to reason that it must be an irritating measure to reduce the inflamed part to the state of an exposed abraded surface. It has always been observed that when the membrane is removed, a new one is immediately formed; thereby the process of restoration having to be recommenced. I should deprecate the use of every irritating application in the early stage; though when that gorged state of the parts, having an œdematous character, threatening immediate suffocation, has begun to subside, a certain amount of irritating applications may be admissible, and even necessary. Febrifuge medicines are wholly futile, on account of the inability to swallow; watching, however, for the turn of the disease, accompanied by the restored capacity to swallow, we may then have recourse to quinine and every form of support and moderate stimulus—proceeding, in short, as during recovery in fever. At this time caustics in the solid form may be needed: I say solid, in opposition to the more general applications, especially of strong hydrochloric acid; since the solid caustic can be confined to the parts which are ulcerated.

A favourite and a much followed plan which I have met with in private practice is to give ammonia, the excuse being to support the system. This is, in fact, nothing but stimulus in a form objectionable on account of its local action. Suppose you had a blister, and you were to remove the skin and to apply ammonia, what would you expect to be the result?

I should include the use of chlorides, given with a view to depurate and deodorise, in the same category of dangerous applications. Chlorides immediately act upon the windpipe, and no amount of caution can render them otherwise than injurious. The allusion I have just made to the similarity of treatment to that for fever reminds me once more to speak of the real character of the disease which we call diphtheria. In every case which I have seen I have been able to trace the actual presence of scarlet fever in the house or in its immediate neighbourhood.

I fully acknowledge that in diphtheria of the present day there seems to be this peculiarity, that the course of the attack proceeds with a rapidity and violence that have not been recorded in former times. This comparison, however, of the present with the past may be in a great measure exaggerated. We scarcely take into consideration the virulence of the old putrid sore-throat, of which there are records favouring a conjecture that it must have been as fatal as our present diphtheria; and in regard to the number of fatal cases at the present time I fear that, without being uncharitable, we may attribute many of them to the sort of procedure recorded by M. Bretonneau.

We must, however, not omit to notice certain distinctive signs in the sequelæ of diphtheria as compared with scarlet fever. In scarlet fever there is frequently albuminous urine and anasarca, and general dropsy. In diphtheria we have a local, sometimes even a general paralysis, though most frequently accompanied by albuminous urine. The albuminous urine attaches diphtheria very significantly to scarlet fever, the anasarca in the former and paralysis in the latter being distinctive. A closer investigation, however, greatly reduces this apparent difference. The anasarca belongs to the milder attacks of scarlet fever. It would, therefore, not be looked for in diphtheria, which we consider the most aggravated form. The paralysis of the throat, which we find in diphtheria, is to be referred in a great measure to loss of structure, and partly to excessive prostration. It is never permanent, but gradually disappears during the convalescence; and the recovery is assisted by time, tonics, and nourishment.—*Lancet, May* 4, *v.* 533.

32.—ON IODINE INHALATION IN THE TREATMENT OF DIPHTHERIA.

By Dr. J. Waring-Curran, Bexhill, St. Leonards.

[The first three cases which occurred in Dr. Waring-Curran's practice proved fatal. They were treated by free stimulation, cauterisation of the throat, and chlorine gargles.]

After an interval of several days, six new cases commenced, just as the consternation was beginning to subside, and when we had hoped the disease had left us; these I treated by local cauterisation with nitrate of silver where it was practicable, and the inhalation of iodine in hot vinegar and sage, prescribing large quantities of wine and ammonia, strong beef-tea, and ice, and counter irritants over the chest, as in the first three. Some of these were truly formidable cases, manifesting symptoms which, writers on the subject tell us, once evinced, are never recovered from; but all ultimately recovered, with ten others which subsequently occurred. The wives and children of the artillery-men stationed in the Martello towers in the neighbourhood in several instances became affected; some deaths happened, and the disease only yielded to treatment when the doctor in attendance, at my suggestion, altered his plan, and had recourse to the iodine inhalation, which eventually turned out as efficacious with him as with me. Of its value in this disease I speak fearlessly, and truthfully consider that many of the cases I had under my charge must assuredly have succumbed had it not been for the service rendered by the iodine. To those to whose lot may unfortunately fall the treatment of diphtheria, I would earnestly recommend a fair and early trial of inhalation of iodine combined with sage and hot vinegar.

The formula which I employ for inhalation purposes is the same as that formerly used by Sir Charles Scudamore in pulmonary phthisis—viz.: iodine, iodide of potassium, of each four grains; alcohol, four drachms; water, four ounces. Of this, for each inhalation, commencing, I take a drachm; add to it a pint of vinegar infused with it a handful of dried garden sage, placed in a common inhaling jar, steadily increasing the quantity of iodine solution until I arrive at half an ounce each inhalation. The circumstances of the case, the age and strength of the patient, and the severity or mildness of the attack, guide me with regard to the number of inhalations, and the time occupied by each. For an average case, occurring in a healthy patient, I would say twelve inhalations at least per diem, with eight to twelve minutes (an interval allowed to rest) for each. The loss of valuable time in country practice, occasionally, will not permit the delay of sending and waiting for an apparatus; accordingly not unfrequently I have been compelled to make my inhalation in the following manner:—Having boiled the vinegar and sage, place it in a teapot with a long spout, and when the patient is prepared to inhale, add the iodine, cover the lid of the vessel with a cloth, keep up the temperature by a spirit-lamp placed underneath, and holding the vessel by the handle, allow the patient to inhale through the spout. Laryngeal irritation is in a great measure prevented by the small quantity at first used, through the patient becoming gradually accustomed to it.—*Lancet, Sept.* 21, *and Oct.* 26, 1867, *pp.* 357, 513.

33.—ON DROPSY OF THE PERITONEUM: ASCITES.

By Dr. S. O. Habershon, Physician to Guy's Hospital.

The habit of designating disease by the name of one of its symptoms is fraught with many disadvantages; but this is, unfortunately, a practice too frequently adopted. Jaundice is merely a *symptom;* but it is the name given to a *class* of diseases, although produced by a variety of conditions. Albuminuria and Bright's disease are terms of general significance; so of others in ordinary use. Thus, also, dropsy of the peritoneum, or ascites, is *often designated dropsy;* and we admit that there is some excuse for apply-

ing such an appellation to this infusion into the largest of the serous membranes. Ascites is often associated with anasarca; and it thence constitutes a part of the general dropsy. But the varieties of peritoneal effusion are worthy of especial consideration; and it is to this *local* dropsy, in its several aspects, that I invite attention.

We distinguish several forms of peritoneal effusion—

1. From *atrophy:* as in senile wasting; in exhaustive cachexiæ; and in simple anæmia.
2. *Ascites from obstruction:* as in cirrhosis; in heart disease; in chronic bronchitis; in any obstruction in the vena-portæ or vena cava.
3. Ascites from *renal* disease.
4. From *glandular disease*, whether affecting the spleen or lymphatic glands, &c.
5. *Inflammatory* ascites.
6. *Strumous* ascites.
7. *Cancerous* ascites.

And although in each of these forms of peritoneal effusion the malady may be produced by some general ailment affecting the whole system, in the last three we have a more especial *local* manifestation of disease; and I have found these varieties overlooked or imperfectly understood.

1. *Atrophic ascites* is often present at the close of wasting disease. We find it in the feebleness of old age; in exhaustive cachexiæ; and in simple anæmia. The effusion is of a passive kind; but it is sometimes sufficient to call for notice. The circulation in these cases is retarded, or has almost ceased, from failing power, or from fibrinous coagulation in the veins; and slow extravasation then takes place into the serous cavity and areolar tissue. It is a kind of exosmosis, and closely resembles the passage of serum into dependent parts of the body after the circulation has stopped.

2. A second form of ascites may be correctly designated *dropsy from obstruction.* Any mechanical impediment to the passage of blood from the portal system of vessels produces this variety of ascites. In several instances I have seen cancerous disease extending directly into the inferior cava, and reaching to the right ventricle, thus preventing the exit of blood from the liver, and causing engorgement of the portal circulation; and in another instance this extension took place into the vena portæ itself, so that the whole structure of the liver was injected with cancerous product. In these rare cases ascites was present from mechanical hindrance to the course of the blood. More frequently we find obstructive ascites caused by chronic disease of the liver, or of the heart, or of the lungs and bronchi. In the latter varieties the legs become anasarcous, and so also in many cases of hepatic disease, as cirrhosis; for the obstruction affects the whole inferior cava, and in cirrhosis it will be found that contraction at the lobulus Spigelii hinders the free passage of blood from the inferior cava close to the heart. This form of effusion may be merely serous in its character, but, from the long-continued congestion of the capillary vessels of the peritoneum, the nutrition of the serous membrane is generally more or less interfered with; the membrane becomes thickened, granular, and in colour opaque; and, what is of still greater importance to remember (especially in the consideration of operative relief by paracentesis), acute inflammatory changes are very easily induced, and fibro-albuminous product is quickly poured out. I would, *en passant*, refer to a rare form of passive ascites from obstruction which I have witnessed on two occasions, in which the effusion had a milky aspect, and was in part of a chylous character. In one of these cases the pressure involved the thoracic duct; and in the other the mesenteric lacteal vessels were very much distended, from obstruction in the mesenteric glands.

3. A third form of ascites is that connected with *renal* disease. Acute albuminuria, whether following scarlet fever or from other cause, is often accompanied by serious effusions into the peritoneum, as one of its symptoms, in common with general anasarca; and there is a great tendency to serous inflammation in this disease, apparently from the presence of urea in the serum. The peritoneum shares in this disposition; so that in an analysis of

instances of peritonitis I found that, out of 500 fatal cases, 63 were in connexion with renal disease. These instances of ascites with general anasarca do not call for special treatment. As the renal affection subsides the fluid becomes absorbed; and the best mode of treatment is that directed to the relief of the original malady. But there are conditions in which we find renal disease with ascites without general anasarca: I refer to renal with hepatic disease, whether it be chronic contraction of the liver and kidney, as cirrhosis, or lardaceous disease. The ailment is of a chronic kind, and the improvement very slow; whilst the ultimate recovery depends on the state of the constitution.

4. A *fourth* variety of ascites might justly be called *glandular*. The peritoneal effusion is secondary to disease of important glands, by which the composition of the blood is changed. The ascites may be only part of a general dropsy. These instances arise from affections of the spleen and of the lymphatic glands, whether the change be one of congestion, of inflammation, of lardaceous, or other disease. The effusion is of a passive kind, and the treatment wholly of a constitutional character.

In other instances the ascites may be regarded as more especially of peritoneal origin, although the constitution is also at fault. The local treatment is of greater value than in the preceding forms of disease, for it is in the serous membrane that we have the manifestation of morbid action.—*Lancet, May* 4, 1867, *p.* 535.

34.—INTERESTING CASE OF PERSISTENT HEMORRHAGE FROM THE BOWELS, OCCURRING PERIODICALLY.

By Dr. M'Gregor, Medical Superintendent Barnhill Hospital, Glasgow.

[The patient, who was 33 years of age, was seized four years ago by an attack of hemorrhage from the bowel, but with rest and astringents internally it shortly ceased. Since then she has had a similar attack every six or eight months, leaving her on every occasion in a pale and weakly state of health. On the last occasion of all, six months before this paper was written, the loss of blood became really alarming, the patient passing every night (always in the night) half or three-fourths an ordinary chamber-pot full of pure blood. This continued several nights in succession, till the patient had become so weak and prostrated, that it was clear if some relief was not obtained the hemorrhage must speedily prove fatal. No cause could be assigned whatever.]

The remedies, usually successful, had on this occasion no effect in checking the nightly recurring gush of blood. The patient was only kept alive by the liberal supply of brandy, wine, and beef-tea, along with light and nutritious articles of diet. Large quantities of ice were used to check the vomiting that was now setting in from sheer exhaustion and loss of blood, there being also vertigo, cold clammy sweats, and other symptoms of collapse present.

In this way the case went on for more than three weeks, the hemorrhage re-appearing every time the patient rallied, and reaction set in. She was perfectly conscious of her condition, though extremely weak, and could always tell when a fresh gush of blood might be expected by *hearing* and *feeling* it accumulating within the abdomen with a rushing noise from each of her sides towards the middle of the belly, about the umbilicus. Immediately upon hearing the gurgling noise, a feeling of pain, swelling, and uneasiness set in, pressing so much on the stomach and other parts as to cause vomiting, and an urgent desire to go to stool. No rest could be obtained till this desire was yielded to, when blood in alarming quantities would gush in a stream from the bowel, unmixed as a rule with any excrementitious matter. Instant relief was obtained on its evacuation, and the patient continued easy till the same process again set in on the following night, attended by symptoms in all respects similar to the preceding attack.

Several medical gentlemen of standing saw the patient with me, and *every remedy that could* be suggested was put in use. Astringent injections

of various kinds were employed, including perchloride of iron and astringent powders and mixtures by the mouth. Every position and posture that was likely to be of use, such as raising the pelvis by placing pillows below it, &c., were tried, but all were of little avail so far as checking the ever recurring hemorrhage was concerned. Strange to say, the only position that seemed to exercise any marked control over the quantity of blood passed was placing the patient on her elbows and knees. She was convinced herself that not only were the intervals between the attacks thus lengthened, but the quantity of blood was not so large when at any time she felt able to maintain this position. But she was so weak as to render it impossible for her to remain long in so awkward a posture, so that its prolonged effects could not be decided on.

Having at last despaired of finding a remedy or discovering a solution of the cause of such unusual symptoms, I was advised by my friend, Dr. Thomas, of Glasgow, to give the secale cornutum a trial, as he had noticed good effects from it in some cases of hemorrhage from the bowel.

I had little faith in the ergot after so many other agents had failed, but thinking at least that no harm was likely to arise from its use, I determined to try it, and began with half dram doses every four hours.

The patient had had an attack the previous night of the usual character, and there were no signs whatever of an amendment when the ergot was begun. From that day to this, not a drop of blood has been lost to the patient's knowledge. The ergot was continued, the same as above, for a week. Then for a second week in the same dose three times a-day, when it was stopped, and tincture of steel with other restoratives given instead. The patient has gone on gaining more and more strength ever since, and has been at her usual employment for several months.

Whether the hemorrhage will recur after a few months, as it has repeatedly done before, or whether in the event of its doing so ergot of rye will prove successful in subsequent attacks, is a matter of equal uncertainty; but I think there can be very little doubt that the cessation of the bleeding in this attack was decidedly due to the use of the ergot.

It would be interesting to know exactly the cause of the hemorrhage as well as the modus operandi of the drug just named in this particular case.

It is but right to state that the patient has shown recent symptoms of incipient phthisis, but the history of the case would indicate phthisis to be the effect rather than the cause of the hemorrhage. For it is reasonable to suppose that such repeated loss of blood for several years was calculated to lower the system and impoverish the blood. Even allowing abdominal phthisis to be the cause, it would be difficult to reconcile the repeated attacks at long intervals, extending over four years, with softening of tubercle or glandular ulceration of the bowels, without at least other evident symptoms of mischief developing themselves before now, which is not the case.

The patient is somewhat of a relaxed habit, and never was very robust, but there were no other symptoms of the hemorrhagic diathesis, usually so-called, in her case.

It would seem to be due rather to a relaxed state of the capillaries, occurring periodically as the result of some want of tone in the nervous system, and it is likely, I think, that the ergot must have acted by a direct influence on the blood vessels counteracting this defect in the nervous system, or it acted on the nerves themselves, leading to the same result. It is well known that ergot has a powerful effect in lessening congestion of the spinal cord, and it is very likely that it does so by its influence on the nervous system, regulating the vascular supply by contracting the blood vessels, and thus lessening the amount of blood to the part. It is probable that it acted in a similar manner here, by giving tone to relaxed muscular fibre directly, or by its effect in the nervous system regulating the vascular, in such a way as to stop the hemorrhage.

I shall note the future history of the patient, if possible, and report a recurrence of the bleeding, with the result of treatment by the ergot in any subsequent attack. Meantime I would recommend a trial of the ergot in frequent

doses in obstinate bleeding from the bowels, especially if the ordinary remedies fail to arrest it, as they did in the present instance.

Note.—The above account was written in April last year. The patient is at this date quite well, and stronger than I have seen her for a couple of years past. The bleeding has not since troubled her.—*Glasgow Medical Journal, June*, 1867, *p.* 46.

35.—DR. MARCET'S PROCESS OF ARTIFICIAL DIGESTION.

We direct the attention of our readers to a pamphlet by Dr. Marcet which, like everything emanating from the same source, contains matter well worth general attention. Dr. Marcet first alludes to the importance of supplying nourishment in sufficient quantity to the system in cases where, from some peculiarity, the stomach is incapable of fulfilling its functions aright. Beef-tea and milk are often the only articles of diet the stomach will bear, and even the latter frequently disagrees, whilst beef-tea alone does not supply sufficient pabulum. It therefore struck Dr. Marcet that the process which naturally goes on in the stomach might be imitated artificially, and so supply the food in a partially digested condition. He, accordingly, took hydrochloric acid and some pepsine, added these along with water to a quantity of meat, allowing the whole to simmer over a water-bath at about the temperature of the body. When the meat was sufficiently broken up, it was strained, and the acid neutralized by carbonate of soda, when it was ascertained that the product was of a most agreeable character, easily digestible, and containing a vast deal more nourishment than common beef-tea. The proportions he recommends are 58 grains of hydrochloric acid, sp. gr, 1.1496, in a pint (20 oz.) of water, with 15 grains of Boudault's pepsine, and 81 grains of bicarbonate of soda to a pound of meat (weighed raw), the chemicals costing about sevenpence. Where pepsine is unattainable, strips of calves' stomachs answer very well, or we do not see why the rennet prepared from it and used for curdling milk should not be employed. The food thus prepared keeps well until neutralized, but not so well afterwards. One point to be noticed is that no metallic vessel should be used in the process, lest the acid act upon it; but for full particulars we must refer our readers to Dr. Marcet's little work.*—*Medical Times and Gazette, June* 8, 1867, *p.* 633.

36.—VOMITING, WITH SARCINÆ VENTRICULI.

[The following short paragraph is taken from a report on the practice at University College Hospital:]

For these vegetable growths the sulphite, bisuiphite, or hyposulphite of soda is commonly used, but Dr. Reynolds prefers using the pure sulphurous acid. He recommends the sulphurous acid to be made in the ordinary way, and to be passed through water until the latter has dissolved up as much as it will take. This saturated solution is to be diluted until the acid is no longer disagreeable to swallow, when an ounce should be given before each meal. The sarcinæ may be thus destroyed; yet they are apt to return, as will other parasites, until the conditions on which their presence depends have been removed.—*Medical Times and Gazette, July* 27, 1867, *p.* 91.

DISEASES OF THE URINARY ORGANS.

37.—ON VARIOUS POINTS OF ANALOGY BETWEEN THE LIVER AND KIDNEYS, AS REGARDS THEIR STRUCTURE, FUNCTIONS, AND PATHOLOGY.

By Dr. George Johnson, Professor of Medicine in King's College.

The diseases of the *kidney* are much better understood, as regards their morbid anatomy, their pathology, and their diagnosis, than those of the

* "On a New Process for Preparing Meat for Weak Stomachs." By W. Marcet, M. D., etc. London: *John Churchill and Sons.*

liver. There are several reasons for this: 1st, the structure of the kidney is more simple than that of the liver; 2nd, its secretion can readily be obtained for chemical and microscopical examination; and 3d, most of the morbid materials are formed within the uriniferous tubes, and thus escape with the urine. So that the nature and the stage of each disease may commonly be determined with great accuracy during life.

There are analogies as to structure, and function, and pathological conditions between the liver and the kidney, and a knowledge of renal pathology affords much assistance in clearing up some obscure points relating to diseases of the liver.

Let me give you some examples of analogy between the liver and kidney. In both organs the secreting cells are the essential elements of the gland, the agents by which the materials of each secreting are separated from the blood. In both organs the gland-cells are contained within a tubular membrane. This is quite obvious in the kidney; and no unprejudiced observer who has examined Dr. Beale's specimens can doubt that the ducts are continued into the lobules of the liver, and that the cells are contained within the tubes. There is within the lobules "a cell-containing network" of tubes.

One difference between the two glands is that the bile is secreted chiefly from venous blood, the urine from arterial blood. Yet again, there is this remarkable resemblance between the liver and the kidney, that both the bile and the urine are secreted from blood which has previously passed through one set of capillary vessels. The blood of the portal vein has passed through the capillaries of various abdominal organs before it enters the liver. The blood of the inter-tubular renal plexus has passed through the Malpighian capillaries within the kidney.

Each efferent vessel is, as Mr. Bowman states (in his original paper, "On the Structure and Use of the Malpighian Bodies of the Kidney" in the "Philosophical Transactions,") *truly a portal vein in miniature*, and it resembles the portal vein in the fact that it intervenes between two capillary networks, the first of which answers to that in which the vena porta originates, the second to that in which the vena porta terminates.

One interesting question relating to the secretion of bile has been much debated amongst physiologists and pathologists. It is this: Do the constituents of the bile, or any part of them, exist ready formed in the blood, or are they formed entirely by the liver? Some physiologists maintain that the bile is formed exclusively by the liver. Their doctrine is that the liver-cells withdraw from the blood certain materials, which they separate into bile and glucose or sugar; the bile then passes into the ducts, and the sugar into the blood of the hepatic vein. In opposition to this doctrine, it is asserted that some, at least, of the bile-constituents exist ready formed in the blood, and that the office of the liver-cells is mainly to separate these materials from the blood during its transit through the gland. This question is one which concerns not only the physiologist but also the pathologists and the practitioner. Our views as to the pathology and diagnosis, and even the treatment of certain forms of liver disease, must be influenced by the opinions which we entertain upon this physiological question.

I will briefly refer to the main facts and arguments in favour of either view. The analogy of secretion by the kidney is in favour of the doctrine that some at least of the bile-constituents exist ready formed in the blood, and require only to be separated by the liver-cells. The chief of the urinary solids—urea and uric acid—are known to exist in the blood. The kidney *secretes or separates*, but does not *form* them. And when the kidneys are destroyed by disease in the human subject, or when in the lower animals the glands are extirpated or their functions suspended by ligature of the artery or duct, death speedily results from an accumulation of urinary constituents in the blood.

In opposition to this argument from the analogy of the renal secretion, when applied to the secretion of bile, it is asserted that chemistry fails to detect in healthy blood any bile-constituents. But this failure to detect bile-constituents in the blood is no proof of their non-existence. Bile is

continually reabsorbed from the intestines, and returns into the circulation; yet chemistry fails to detect it in the blood. A chemical examination of jaundiced blood finds only the biliary colouring-matter, and not the biliary acids. And, further, it is acknowledged by Frerichs that even when large quantities of bile have been injected into the blood, so as to obviate the difficulties which there might be in detecting small quantities, there is an equal want of success; the colouring-matter only can be detected. The biliary acids appear to undergo some rapid transformation; at any rate they escape detection. On chemical grounds, therefore, there is no reason for rejecting the theory which assumes that some at least of the constituents of bile—the colouring-matter, if not the bile acids—pre-exist in the blood, and require only to be separated by the liver-cells.

Most physiologists agree in the opinion that there is a close and intimate relationship between the colouring-matter of the bile and urine and that of the blood. It is probable that the two former may be derivatives from the latter.

The doctrine of the formation of bile exclusively by the liver has led to some fanciful hypotheses as to the pathology of jaundice occurring without any apparent obstacle to the escape of bile through the ducts. For instance, jaundice has frequently resulted from obstruction of the *portal vein*. How is this to be explained on the assumption that bile is formed only in the liver? An impeded flow of blood to the liver should result in a *lessened formation* of bile, and not in any accumulation in the blood. Frerichs explains the jaundice in these cases by what he calls "abnormal diffusion." "In consequence of obstruction of the main trunk, or of the larger branches of the portal vein, the tension of the capillary vascular system is diminished, and the entrance of the biliary contents of the hepatic cells into the blood is facilitated." This appears to me a very clumsy, mechanical, and unsatisfactory explanation. On the other hand, admit—what cannot be disproved—that at least the colouring constituents of the bile pre-exist in the blood, and the jaundice which results from obstruction of the portal vein receives a ready explanation. In consequence of the obstructed flow of blood there is suppressed secretion, and therefore an accumulation of biliary materials in the blood; just as from ligature of the renal artery there occurs suppressed secretion of urine.

I am decidedly of opinion that the weight of evidence preponderates on the side of those who maintain that some of the bile-constituents, like those of the urine, exist ready formed in the blood, and that jaundice may result, as Dr. Budd teaches, 1st, from an impeded escape of bile through the ducts, and its consequent reabsorption into the blood; or 2dly, from suppressed secretion consequent on impeded circulation, or destructive changes in the secreting cells. It is probable that as the bile is in part a mere secretion and in part an actual formation by the liver, so the urinary constituents are in part formed by the kidney. This, at any rate, is rendered probable by an experiment performed by Dr. Oppler, of Berlin, who found after ligature of the uterus of a dog much more urea in the blood and muscles than after extirpation of the kidneys: and he believes that this excess is due to the actual formation of urea by the kidneys; so that they not only separate urea which has been ready formed, but they have also a large share in forming it. With reference to this experiment, however, it would be important to know whether the dogs whose kidneys were extirpated did not die sooner than those whose ureters were tied. If so, the excess of urea in the blood and tissues after ligature of the ureters might be simply a result of the greater duration of life after the functions of the kidneys had been suspended.

In opposition to the theory of the pre-existence in the blood of some of the bile-constituents, and to the theory of jaundice from suppression, it is argued that jaundice would be a more constant result than it is of extensive degeneration of the secreting tissue of the liver, such as occurs, for instance, in the advanced stages of cirrhosis.

To this argument the reply is, that jaundice in a greater or less degree *is a common result of advanced* cirrhosis; and its frequent occurrence in these

cases, when there is no evidence of impediment to the escape of bile through the ducts, is more difficult of explanation on the hypothesis of bile-formation exclusively by the liver, than is its occasional absence on the hypothesis of the pre-existence of some bile-constituents in the blood.

The observation is common to the two organs—the liver and the kidney—that a *slow* degeneration and obstruction of their secreting tissues may reach a very advanced stage without giving rise to any of the ordinary symptoms of suppressed secretion. As cirrhosis of the liver occurs without jaundice, so atrophic granular degeneration of the kidney may reach an advanced stage without dropsy or signs of uræmia. This is partly accounted for by the vicarious elimination through other organs. Biliary materials are excreted by the kidneys, urinary excreta by the liver, and both by the intestines, and perhaps in a less degree by the skin. In these cases sudden suppression occasionally happens as a result of some additional disturbing cause. A superabundant or indigestible meal causes suppression of bile and jaundice, in the case of cirrhosis. Exposure to cold induces suppression of urine, dropsy, and uræmia in the case of advanced degeneration of the kidney. In some of these cases there has been no suspicion of serious disease until within a very few hours or days of the fatal termination.

It is probable that some of the cases which have been recorded as examples of *acute atrophy of the liver*—cases in which the liver is supposed to have shrunk to one-half or one-third of its original size within a few days—have, in reality, been cases in which a slow atrophy and disorganisation of the gland have been insidiously making progress long before the sudden development of the serious symptoms which have been supposed to indicate the actual commencement of the hepatic disease.

[It is in the discharge of their functions as excretory glands that the liver and the kidney, in most instances, become diseased, and thus disease in both occurs most frequently in the *secreting cells.*]

The secreting cell is the essential part of the gland—its most vitally active and sensitive part; and the secreting cell is the first structure to show a deviation from the normal condition when morbid materials are conveyed to the gland through the blood.

There is much that is mysterious in the act of secretion, as in every vital process; but it is a well-established fact that certain cells exert a kind of affinity for particular materials which are conveyed to them by the blood, and, by virtue of this affinity, the cells first abstract the materials from the blood, and subsequently discharge them into the ducts of the gland. The cells of each gland are endowed with the power of excreting certain products rather than others: the liver-cells secrete bile, and the kidney-cells urine. We know little of the precise nature of this power; but we see clearly that it is associated with a particular form of cell. The normal appearance of the gland-cells is as peculiar and characteristic as are the physical and chemical properties of their respective secretions. The cells of the kidney and the liver, while they are alike in all particulars which constitute them gland-cells, yet differ from each other as much as do their respective secretions—as much as urine differs from bile. It is evident, then, that there is an intimate relation between the structure and appearance of the cells and the nature and composition of their secreted products.

The cells of each gland, besides the constituents of their own proper secretion, have also the power of separating from the blood certain materials which do *not* form a part of their normal secretion.

Many inorganic salts and many odorous and colouring matters derived from the vegetable and animal kingdoms, when introduced into the blood through the stomach, are readily eliminated by the kidneys, and apparently without injury to the secreting cells of the gland. The elimination of other substances is attended with an alteration of the structure and appearance of the cells. For instance, the long-continued secretion of sugar in cases of diabetes generally induces changes in the renal gland-cells. In these cases the epithelium of the kidney becomes opaque, and often contains a large amount of finely granular material with oil. In some instances the urine becomes

albuminous, and contains casts of the uriniferous tubes. The persistent secretion of sugar by the kidneys changes the appearance and the vital properties of the renal cells, so that, in adapting themselves, as it were, for the secretion of sugar, they become gradually unfitted for the secretion of urine. The circulation through the glands is impeded; then the kidney becomes congested and the urine albuminous. Suppression of urine is not unfrequently the immediate cause of death in cases of diabetes. The injury which the secreting cells of the kidney sustain in consequence of having to eliminate for a long period large quantities of sugar is one amongst other weighty reasons for keeping a diabetic patient upon a diet which shall tend as much as possible to lessen the formation of sugar.

In cases of jaundice, some of the biliary constituents are excreted by the kidneys; but during the process of eliminating these new products, the nutrition of the renal cells becomes modified, and the cells are often shed by a process of desquamation. Sometimes, too, the urine contains albumen and tube-casts, in consequence of the congestion of the kidney, which results from the presence of bile in the blood, and its slow and difficult elimination by the renal gland-cells. A short time since I attended a gentleman whose urine for a few hours contained a very large amount of bile, consequent upon obstruction of the gall-duct by a calculus. So long as the urine contained bile, it also contained a large amount of albumen. As soon as the gall-stone passed, the bile resumed its usual course; and as soon as the kidney ceased to secrete bile, the urine ceased to be albuminous.

Various morbid poisons, or the products of morbid poisons, exert a similar influence on the renal epithelium while they are being excreted—*e. g.*, the poison of scarlatina, diphtheria, and erysipelas.

The cells of the liver are liable to precisely analogous changes under the influence of various morbid materials which are conveyed to them by the blood. These changes in the liver-cells, however, cannot, like those in the kidney, be recognised during the life of the patient, and even after death they are less easily demonstrated in the liver than in the kidney; yet they can be shown to occur, and the recognition of these changes in the gland-cells affords the only means of truly interpreting the pathology of the liver and the kidney.

There is a very close resemblance between the diseases of the liver and those of the kidney, comparing them only as regards their general outward character.

A. Fatty degeneration occurs in both, and in both much of the fat accumulates in the gland-cells.

B. Both organs are liable to the same forms of inflammation.

1. Suppurative inflammation is more common in the liver, chiefly because the blood of the portal-vein is especially liable to be contaminated by morbid fluids from ulcers in the intestines, &c., and by the products of inflamed hemorrhoidal and other veins.

2. There is an acute and a chronic form of inflammation which is *not suppurative*, and which in the kidney I have called *desquamative*.

C. That remarkable form of degeneration which has been called *scrofulous*, *lardaceous*, *waxy*, or *amyloid*, occurs in both organs, and has essentially the same characters in both.

D. The so-called fibrinous deposits occur in both, and these deposits are liable to undergo a fatty transformation, and thus give rise to an appearance which has been called local fatty degeneration of the liver and kidney.

E. Cancerous and hydatid tumours occur in both organs; but these are both more common in the liver than in the kidney.

F. Lastly, concretions may be formed out of the secretion of each organ: gall-stones in the liver; urinary calculi in the kidney.

Postcript.- A correspondent has directed my attention to the statement which I have made in the early part of this lecture, "that both the bile and the urine are secreted from blood which has passed through one set of capillary vessels." And he objects that this statement is in opposition to Mr. *Bowman's theory, that* the liquid portion of the urine is derived mainly from

the Malpighian capillaries. What I intended to say was, that the *peculiar solids* of the urine are secreted from blood which has previously passed through one set of capillaries—namely, those of the Malpighian tuft. It is probable that the urine, as it is secreted by the convoluted tubes, has nearly the same consistence as the bile, and that this thick liquid is diluted by the water, which is continually poured from the Malpighian capillaries into the extremities of the tubes.—*Lancet, August* 24 *and* 31, 1867, *pp.* 221, 256.

38.—ON THE CURABILITY OF BRIGHT'S DISEASE.

By Dr. Arthur Hill Hassall, Senior Physician to the Royal Free Hospital.

[Dr. Hassall has for some time been engaged in investigating cases of Bright's disease in order to determine the extent of the aid, in diagnosis, prognosis, and treatment, to be derived from a careful and rigorous microscopical and chemical examination of the urine. In the first case the microscopical examination of the urine showed that though the case was granular degeneration of the kidneys, and therefore serious enough, yet that since it was not one of fatty degeneration there was a fair prospect of amendment, if not of absolute recovery. The mean amount of albumen lost per diem was 70·4 grains, but under the treatment adopted, in two months it was only 4·86 grains. The next case was one of fatty degeneration of the kidneys, and the amount of albumen excreted in the twenty-four hours was reduced in nine months, during which the treatment could hardly be said to be very thoroughly carried out owing to the man's own imprudences, from 310 to 163 grains daily. Many other interesting cases are related, and the success attending their treatment seemed to hinge upon the presence or absence of *fatty* degeneration.]

The treatment adopted in all the above cases, although varying in details, was similar in its principles. The free action of the skin was maintained by the occasional use of the vapour bath. Congestion of the kidneys, when present, was relieved by means of dry-cupping. Tonics and astringents were administered, consisting chiefly of the tincture of the sesquichloride of iron, sulphate of quinine, tannic and gallic acids, and when the effusion was very great, occasional doses of hydragogue cathartics and diuretics were given; but above and before all, great attention was paid to the diet. This was rendered highly nitrogenous; milk and eggs were freely given, and meat two, and even three times a day; indeed it is to the diet prescribed, and the rest from toil and anxiety experienced by the patients while remaining in the hospital, that I chiefly attribute the successful results of the treatment. The absence of wasting, notwithstanding the enormous loss of albumen, is remarkable in most cases of Bright's disease, and was especially noticed in the cases above recorded, the majority of the patients throughout their illness retaining a considerable degree of plumpness and flesh.

The results obtained by the treatment of the several cases, so far as the albumen contained in the urine is concerned, stand thus:—

Case 1.—Francis S. In this case the mean amount of albumen excreted, after his admission into the hospital on the 20th of October, 1864, was 70 grs. per day, but when he left, at the end of January, it was reduced to 4·86 grs.

Case 2.—Edward G. The mean quantity of albumen in the urine at the period of his admission, on October 29th, 1864, was 310 grs. per day, which was reduced on quitting the hospital, in the early part of January, to an average of 195 grs.; but at the end of July a still smaller quantity was found —namely, 161·4 grs., showing a total diminution of 148·6.

Case 3.—Joseph J. This patient, shortly after his admission, on April 19th, 1865, was passing 357 grs. of albumen per day, and on leaving the hospital of his own accord the amount still remained high, being 362 grs., notwithstanding which his condition had greatly improved; but at the end

of July the mean quantity was only 202·9 grs., being a reduction of 154·1 gr.

Case 4.—John C. The average daily amount of albumen passed after admission, on Jan. 12th, 1865, was 184·05 grs.; this was considerably decreased by the end of March, when he was discharged from the hospital, 124·4 grs. being then excreted. This quantity, however, was still further reduced when the urine was next examined, on Aug. 5th, 77 grs. being found, showing a diminution of 107 grs.

Case 5.—Martin H. In this case the patient, at the period of his admission, on Feb. 15th, was excreting 245 grs. of albumen daily; but on July 9th, just before he was sent to the Convalescent Institution, only 6·16 grs. were found for the day, the patient being in fact almost completely cured.

I must confess that the very striking facts recorded in this paper have greatly surprised me. The impression I had derived from books in reference to Bright's disease was that it was nearly always a fatal and incurable affection, whereas it really is much more amenable to treatment than could have been anticipated—so much so, indeed, as in my belief to fully justify the title I have given to this communication. The particulars recorded appear also to prove that the disease is not exclusively local and organic, but is to a great extent a blood disease.—*Lancet*, *Dec.* 18, 1865, *p.* 670.

SURGERY.

BONES, JOINTS, &c.—AMPUTATIONS, FRACTURES, DISLOCATIONS.

39.—ON A NEW METHOD OF TREATING COMPOUND FRACTURE, ABSCESS, &c.

By Joseph Lister, Esq., F.R.S., Professor of Surgery in the University of Glasgow.

[In our last volume we printed Prof. Lister's former papers on this subject. The *method of treating abscess*, described in the following article, has been found extremely satisfactory.]

It is based, like the treatment of compound fracture, on the antiseptic principle, and the material employed is essentially the same—namely, carbolic acid, but differently applied in accordance with the difference of the circumstances. In compound fracture there is an irregular wound, which has probably been exposed to the air for hours before it is seen by the surgeon, and may therefore contain in its interstices the atmospheric germs which are the causes of decomposition, and these must be destroyed by the energetic application of the antiseptic agent. In an unopened abscess, on the other hand, as a general rule, no septic organisms are present, so that it is not necessary to introduce the carbolic acid into the interior. Here the essential object is to guard against the introduction of living particles from without, at the same time that a free exit is afforded for the constant discharge of the contents. The mode in which this is accomplished is as follows:—

A solution of one part of crystallised carbolic acid in four parts of boiled linseed oil having been prepared, a piece of rag from four to six inches square is dipped in the oily mixture, and laid upon the skin where the incision is to be made. The lower edge of the rag being then raised, while the upper edge is kept from slipping by an assistant, a common scalpel or bistoury dipped in the oil is plunged into the cavity of the abscess, and an opening about three-quarters of an inch in length is made, and the instant the knife is withdrawn the rag is dropped upon the skin as an antiseptic curtain beneath which the pus flows out into a vessel placed to receive it. The cavity of the abscess is firmly pressed, so as to force out all existing pus as nearly as may be (the old fear of doing mischief by rough treatment of the pyogenic membrane being quite ill-founded); and if there be much oozing of blood, or if there be a considerable thickness of parts between the abscess and the surface, a piece of lint dipped in the antiseptic oil is introduced into the incision to check bleeding and prevent primary adhesion, which is otherwise very apt to occur. The introduction of the lint is effected as rapidly as may be, and under the protection of the antiseptic rag. Thus the evacuation of the original contents is accomplished with perfect security against the introduction of living germs. This, however, would be of no avail unless an antiseptic dressing could be applied that would effectually prevent the decomposition of the stream of pus constantly flowing out beneath it. After numerous disappointments, I have succeeded with the following, which may be relied upon as absolutely trustworthy. About six tea-spoonfuls of the above-mentioned solution of carbolic acid in linseed oil are mixed up with common whitening

(carbonate of lime) to the consistence of a firm paste, which is in fact glazier's putty with the addition of a little carbolic acid. This is spread upon a piece of sheet block tin about six inches square; or common tin-foil will answer equally well if strengthened with adhesive plaster to prevent it from tearing, and in some situations it is preferable, from its adapting itself more readily to the shape of the part affected. The putty forms a layer about a quarter of an inch thick; it may be spread with a table-knife, or pressed out with the hand, a towel being temporarily interposed to prevent the putty from sticking to the hand or soiling the coat-sleeve. The tin thus spread with putty is placed upon the skin so that the middle of it corresponds to the position of the incision, the antiseptic rag used in opening the abscess being removed the instant before. The tin is then fixed securely by adhesive plaster, the lowest edge being left free for the escape of the discharge into a folded towel placed over it and secured by a bandage. This dressing has the following advantages: — The tin prevents the evaporation of the carbolic acid, which escapes readily through any organic tissue such as oiled silk or gutta-percha. The putty contains the carbolic acid just sufficiently diluted to prevent its excoriating the skin, while its substance serves as a reservoir of the acid during the intervals between the dressings. Its oily nature and tenacity prevent it from being washed away by the discharge, which all oozes out beneath it as fast as it escapes from the incision; while the extent of the surface of the putty renders it securely antiseptic. Lastly, the putty is a cleanly application, and gives the surgeon very little trouble; a supply being daily made by some convalescent in an hospital, or in private practice by the nurse or a friend of the patient; or a larger quantity may be made at once, and kept in a tin canister. The dressing is changed, as a general rule, once in twenty-four hours; but if the abscess be a very large one, it is prudent to see the patient twelve hours after it has been opened, when, if the towel should be much stained with discharge, the dressing should be changed, to avoid subjecting its antiseptic virtues to too severe a test. But after the first twenty-four hours, a single daily dressing is sufficient. The changing of the dressing must be methodically done, as follows:—A second similar piece of tin having been spread with the putty, a piece of rag is dipped in the oily solution, and placed on the incision the moment the first tin is removed. This guards against the possibility of mischief occurring during the cleansing of the skin with a dry cloth and pressing out any discharge which may exist in the cavity. If a plug of lint was introduced when the abscess was opened, it is removed under cover of the antiseptic rag, which is taken off at the moment when the new tin is to be applied. The same process is continued daily till the sinus closes.

The results of this treatment are such as correct pathological knowledge might have enabled us to predict. The pyogenic membrane has no innate disposition to form pus, but does so only because it is subjected to some preternatural stimulus. In an ordinary abscess, whether acute or chronic, the original cause that led to suppuration is no longer in operation, and the stimulus that determines the continued pus formation is derived from the presence of the pus pent up in the interior. When an abscess is opened in the ordinary way this cause of stimulation is removed, but in its place is substituted the potent stimulus of decomposition. If, however, the abscess be opened antiseptically, the pyogenic membrane, freed from the operation of the previous stimulus without the substitution of a new one, ought, according to theory, to cease to suppurate, while the patient should be relieved from any local or general disturbance caused by the abscess, without the risk of irritative fever or hectic.

Such, accordingly, is the fact. Abscesses of large size have, after the original contents have been evacuated, furnished no further pus whatever, the discharge being merely serum, which in a few days has amounted only to a few drops in the twenty-four hours. Whether the opening be dependent or not is a matter of perfect indifference, the small amount of unirritating *fluid being all evacuated* spontaneously by the rapidly contracting pyogenic

membrane. At the same time, we reckon with perfect certainty on the absence of all constitutional disturbance.

As an illustration, I may mention the last case which has come under my care. It is that of a young woman, 25 years old, with psoas abscess, which had of late been rapidly on the increase, and caused a large swelling below Poupart's ligament, communicating with a fluctuating mass, dull on percussion, reaching to a considerable distance up the abdomen, the femoral vessels being raised over the communication between them. Six days ago I opened, in the manner above described, the swelling in the thigh at the anterior part of the limb where it was nearest the surface, giving exit to twenty-seven ounces of pus, thin, but containing numerous large curdy masses. I introduced a piece of lint, dipped in the carbolic acid and oil, into the incision; and this prevented any discharge from escaping during the next twenty-four hours, when, on removal of the plug of lint under an antiseptic rag, three ounces turbid serum escaped. For the next three days there was scarcely any discharge, the deeper parts of the incision having cohered. On firm pressure, however, the product of seventy-two hours escaped, and amounted to four drachms of serum. Meanwhile the girl's general health, which had not been interfered with by the abscess, continued perfectly good, neither pulse, tongue, appetite, nor sleep having been disturbed.

In this case, though there is no deformity of the spine, there is great probability that caries of the vertebræ is present. But even though such be the case, there is good reason to hope for a favourable issue. Regarding caries as merely the suppurative stage of chronic inflammation in a weak form of tissue, I have been not surprised, though greatly rejoiced, to find that it exhibits the tendency of inflammatory affections generally—viz., a disposition to spontaneous cure on the withdrawal of irritation. Hitherto, in surgical practice, caries has had to contend against the formidable irritation of decomposing matter, which, under circumstances of weakness, is often sufficient to cause ulceration, even in soft parts; yet, in spite of this irritation, caries is often recoverable in the child where the vital powers of all the tissues are stronger. If, therefore, this serious complication can be avoided, there seems nothing in theory against the probability that caries may prove curable in the adult. And even should portions of necrosed bone be present, as is not unfrequently the case, our experience of the treatment of compound fracture with carbolic acid has taught us that dead bone, if undecomposed, not only fails to induce suppuration in its vicinity, but is liable to absorption by the granulations around it.

Such were the hopes which I ventured to express several months ago to my winter class. Since that time I have opened numerous abscesses connected with caries of the vertebræ, the hip, knee, ankle, and elbow, and in all cases I have found the discharge become in a few days trifling in amount, and in many it has ceased to be puriform after the first twenty-four hours. Finally, three days ago—viz., on the 4th inst., (July, 1867,) I had the inexpressible happiness of finding the sinus soundly closed in a middle-aged man, in whom I opened in February last a psoas abscess, proved to be connected with diseased bone by the discharge, on one occasion, of an osseous spiculum. For months past we had persevered with the antiseptic dressing, although the discharge did not amount to more than a drop or two of serum in the twenty-four hours, well knowing by bitter experience that so long as a sinus existed the occurrence of decomposition might produce the most disastrous consequences; and at length our patience has been crowned with success.

Hence I no longer feel any hesitation in recommending the early opening of such abscesses, because, while they remain unopened, the disease of the bone is necessarily progressive, whereas when opened antiseptically, there is good ground to hope for their steady, though tedious, recovery.

The putty of the strength above recommended, though it generally fails to excoriate the skin, sometimes produces this effect when long continued. In such case it may be reduced in strength so that the oil contains only one part to five or six without disadvantage when the discharge is very small in amount.

The application prevents the occurrence of cicatrisation in the little sore caused by the incision, and perpetuates a trifling discharge from it. Hence it is impossible to judge whether or not the sinus has closed, except by examining it from time to time with a probe, which should be dipped in the antiseptic oil, and passed in between folds of the antiseptic rag. This may seem a refinement, but if we could see with the naked eye a few only of the septic organisms that people every cubic inch of the atmosphere of an hospital ward, we should rather wonder that the antiseptic treatment is ever successful than omit any precautions in conducting it.

The putty used in treating abscesses has proved very valuable in simplifying the treatment of compound fracture, and enlarging the range of its applicability, and also in dealing with incised wounds on the antiseptic principle. But I must defer a notice of these matters to a future occasion.—*Lancet, July* 27, 1867, *p.* 95.

40.—ON THE ANTISEPTIC PRINCIPLE IN THE PRACTICE OF SURGERY.

By Joseph Lister, Esq., F.R.S., Professor of Surgery in the University of Glasgow.

[The following paper by Prof. Lister, was read before the British Medical Association, in Dublin, on the 9th of August, 1867:]

In the course of an extended investigation into the nature of inflammation, and the healthy and morbid conditions of the blood in relation to it, I arrived, several years ago, at the conclusion that the essential cause of suppuration in wounds is decomposition, brought about by the influence of the atmosphere upon blood or serum retained within them, and, in the case of contused wounds, upon portions of tissue destroyed by the violence of the injury.

To prevent the occurrence of suppuration, with all its attendant risks, was an object manifestly desirable; but till lately apparently unattainable, since it seemed hopeless to attempt to exclude the oxygen, which was universally regarded as the agent by which putrefaction was effected. But when it had been shown by the researches of Pasteur that the septic property of the atmosphere depended, not on the oxygen or any gaseous constituent, but on minute organisms suspended in it, which owed their energy to their vitality, it occurred to me that decomposition in the injured part might be avoided without excluding the air, by applying as a dressing some material capable of destroying the life of the floating particles.

Upon this principle I have based a practice of which I will now attempt to give a short account.

The material which I have employed is carbolic or phenic acid, a volatile organic compound which appears to exercise a peculiarly destructive influence upon low forms of life, and hence is the most powerful antiseptic with which we are at present acquainted.

The first class of cases to which I applied it was that of compound fractures, in which the effects of decomposition in the injured part were especially striking and pernicious. The results have been such as to establish conclusively the great principle, that *all the local inflammatory mischief and general febrile disturbance which follow severe injuries are due to the irritating and poisoning influence of decomposing blood or sloughs;* for these evils are entirely avoided by the antiseptic treatment, so that limbs which otherwise would be unhesitatingly condemned to amputation may be retained with confidence of the best results.

In conducting the treatment, the first object must be the destruction of any septic germs which may have been introduced into the wound, either at the moment of the accident or during the time which has since elapsed. This is done by introducing the acid of full strength into all accessible recesses of the wound, by means of a piece of rag held in dressing forceps and dipped in the liquid.* This I did not venture to do in the earlier cases; but

* *The addition of a few drops of water to a considerable quantity of the crystallised acid induces it to assume permanently the liquid form.*

experience has shown that the compound which carbolic acid forms with the blood, and also any portions of tissue killed by its caustic action, including even parts of the bone, are disposed of by absorption and organisation, provided they are afterwards kept from decomposing. We are thus enabled to employ the antiseptic treatment efficiently at a period after the occurrence of the injury at which it would otherwise probably fail. Thus I have now under my care in the Glasgow Infirmary a boy who was admitted with compound fracture of the leg as late as eight and a half hours after the accident, in whom nevertheless all local and constitutional disturbance was avoided by means of carbolic acid, and the bones were firmly united five weeks after his admission.

The next object to be kept in view is to guard effectually against the spreading of decomposition into the wound along the stream of blood and serum which oozes out during the first few days after the accident, when the acid originally applied has been washed out, or dissipated by absorption and evaporation. This part of the treatment has been greatly improved during the last few weeks. The method which I have hitherto published consisted in the application of a piece of lint dipped in the acid, overlapping the sound skin to some extent, and covered with a tin cap, which was daily raised in order to touch the surface of the lint with the antiseptic. This method certainly succeeded well with wounds of moderate size; and, indeed, I may say that in all the many cases of this kind which have been so treated by myself or my house-surgeons, not a single failure has occurred. When, however, the wound is very large, the flow of blood and serum is so profuse, especially during the first twenty-four hours, that the antiseptic application cannot prevent the spread of decomposition into the interior unless it overlaps the sound skin for a very considerable distance, and this was inadmissible by the method described above, on account of the extensive sloughing of the surface of the cutis which it would involve. This difficulty has, however, been overcome by employing a paste composed of common whitening (carbonate of lime) mixed with a solution of one part of carbolic acid in four parts of boiled linseed oil, so as to form a firm putty. This application contains the acid in too dilute a form to excoriate the skin, which it may be made to cover to any extent that may be thought desirable, while its substance serves as a reservoir of the antiseptic material. So long as any discharge continues, the paste should be changed daily; and, in order to prevent the chance of mischief occurring during the process, a piece of rag dipped in the solution of carbolic acid in oil is put on next the skin, and maintained there permanently, care being taken to avoid raising it along with the putty. This rag is always kept in an antiseptic condition from contact with the paste above it, and destroys any germs that may fall upon it during the short time that should alone be allowed to pass in the changing of the dressing. The putty should be in a layer about a quarter of an inch thick, and may be advantageously applied rolled out between two pieces of thin calico, which maintain it in the form of a continuous sheet, that may be wrapped in a moment round the whole circumference of a limb, if this be thought desirable, while the putty is prevented by the calico from sticking to the rag which is next the skin.* When all discharge has ceased, the use of the paste is discontinued, but the original rag is left adhering to the skin till healing by scabbing is supposed to be complete. I have at present in the hospital a man with severe compound fracture of both bones of the left leg, caused by direct violence, who, after the cessation of the sanious discharge under the use of the paste, without a drop of pus appearing, has been treated for the last two weeks exactly as if the fracture were a simple one. During this time the rag, adhering by means of a crust of inspissated blood collected beneath it, has continued perfectly dry, and it will be left untouched till the usual period for removing the splints in a simple fracture, when we may fairly expect to find a sound cicatrix beneath it.

* In order to prevent evaporation of the acid, which passes readily through any organic tissue, such as oiled silk or gutta-percha, it is well to cover the paste with a sheet of block tin, or tinfoil strengthened with adhesive plaster. The thin sheet-lead used for lining tea-chests will also answer the purpose, and may be obtained from any wholesale grocer.

We cannot, however, always calculate on so perfect a result as this. More or less pus may appear after the lapse of the first week; and the larger the wound, the more likely is this to happen. And here I would desire earnestly to enforce the necessity of persevering with the antiseptic application, in spite of the appearance of suppuration, so long as other symptoms are favourable. The surgeon is extremely apt to suppose that any suppuration is an indication that the antiseptic treatment has failed, and that poulticing or water-dressing should be resorted to. But such a course would in many cases sacrifice a limb or a life. I cannot, however, expect my professional brethren to follow my advice blindly in such a matter, and therefore I feel it necessary to place before them, as shortly as I can, some pathological principles, intimately connected not only with the point we are immediately considering, but with the whole subject of this paper.

If a perfectly healthy granulating sore be well washed and covered with a plate of clean metal, such as block tin, fitting its surface pretty accurately, and overlapping the surrounding skin an inch or so in every direction, and retained in position by adhesive plaster and a bandage, it will be found, on removing it after twenty-four or forty-eight hours, that little or nothing that be called pus is present, merely a little transparent fluid, while at the same time there is an entire absence of the unpleasant odour invariably perceived when water-dressing is changed. Here the clean metallic surface presenting no recesses, like those of porous lint, for the septic germs to develop in, the fluid exuding from the surface of the granulations has flowed away undecomposed, and the result is absence of suppuration. This simple experiment illustrates the important fact, that granulations have no inherent tendency to form pus, but do so only when subjected to a preternatural stimulus. Further, it shows that the mere contact of a foreign body does not of itself stimulate granulations to suppurate; whereas the presence of decomposing organic matter does. These truths are even more strikingly exemplified by the fact, which I have elsewhere recorded, that a piece of dead bone, free from decomposition, may not only fail to induce the granulations round it to suppurate, but may actually be absorbed by them; whereas a bit of dead bone soaked with putrid pus infallibly induces suppuration in its vicinity.

Another instructive experiment is to dress a granulating sore with some of the putty above described, overlapping the sound skin extensively, when we find in the course of twenty-four hours that pus has been produced by the sore, although the application has been perfectly antiseptic; and, indeed, the larger the amount of carbolic acid in the paste the greater is the quantity of pus formed, provided we avoid such a proportion as would act as a caustic. The carbolic acid, though it prevents decomposition, induces suppuration—obviously by acting as a chemical stimulus; and we may safely infer that putrescent organic materials (which we know to be chemically acrid) operate in the same way.

In so far, then, carbolic acid and decomposing substances are alike—namely, that they induce suppuration by chemical stimulation, as distinguished from what may be termed simple inflammatory suppuration, such as that in which ordinary abscesses originate, where the pus appears to be formed in consequence of an excited action of the nerves, independently of any other stimulus. There is, however, this enormous difference between the effects of carbolic acid and those of decomposition—viz., that carbolic acid stimulates only the surface to which it is first applied, and every drop of discharge that forms weakens the stimulant by diluting it. But decomposition is a self-propagating and self-aggravated poison; and if it occurs at the surface of a severely injured limb, it will spread into all its recesses so far as any extravasated blood or shreds of dead tissue may extend, and, lying in these recesses, it will become from hour to hour more acrid till it acquires the energy of a caustic, sufficient to destroy the vitality of any tissues naturally weak from inferior vascular supply, or weakened by the injury they sustained in the accident.

Hence, it is easy to understand how, when a wound is very large, the crust *beneath the rag may* prove here and there insufficient to protect the raw

surface from the stimulating influence of the carbolic acid in the putty, and the result will be, first, the conversion of the tissues so acted on into granulations, and subsequently the formation of more or less pus. This, however, will be merely superficial, and will not interfere with the absorption and organisation of extravasated blood or dead tissues in the interior; but, on the other hand, should decomposition set in before the internal parts have become securely consolidated, the most disastrous results may ensue.

I left behind me in Glasgow a boy, 13 years of age, who between three and four weeks previously met with a most severe injury to the left arm, which he got entangled in a machine at a fair. There was a wound six inches long and three inches broad, and the skin was very extensively undermined beyond its limits, while the soft parts generally were so much lacerated that a pair of dressing forceps introduced at the wound, and pushed directly inwards, appeared beneath the skin at the opposite aspect of the limb. From this wound several tags of muscle were hanging, and among them there was one consisting of about three inches of the triceps in almost its entire thickness, while the lower fragment of the bone, which was broken high up, was protruding four and a half inches, stripped of muscle, the skin being tuck ed in under it. Without the assistance of the antiseptic treatment, I should certainly have thought of nothing else but amputation at the shoulder-joint; but as the radial pulse could be felt, and the fingers had sensation, I did not hesitate to try to save the limb, and adopted the plan of treatment above described, wrapping the arm from the shoulder to below the elbow in the antiseptic application, the whole interior of the wound, together with the protruding bone, having previously been freely treated with strong carbolic acid. About the tenth day the discharge, which up to that time had been only sanious and serous, showed a slight admixture of slimy pus, and this increased till, a few days before I left, it amounted to about three drachms in twenty-four hours. But the boy continued, as he had been after the second day, free from unfavourable symptoms, with pulse, tongue, appetite, and sleep natural, and strength increasing, while the limb remained, as it had been from the first, free from swelling, redness, or pain. I therefore persevered with the antiseptic dressing, and before I left, the discharge was already somewhat less, while the bone was becoming firm. I think it likely that in that boy's case I should have found merely a superficial sore had I taken off all the dressings at the end of three weeks, though, considering the extent of the injury, I thought it prudent to let the month expire before disturbing the rag next the skin. But I feel sure that if I had resorted to ordinary dressing when the pus first appeared, the progress of the case would have been exceedingly different.

The next class of cases to which I have applied the antiseptic treatment is that of abscesses. Here, also, the results have been extremely satisfactory, and in beautiful harmony with the pathological principles indicated above. The pyogenic membrane, like the granulations of a sore, which it resembles in nature, forms pus, not from any inherent disposition to do so, but only because it is subjected to some preternatural stimulation. In an ordinary abscess, whether acute or chronic, before it is opened, the stimulus which maintains the suppuration is derived from the presence of the pus pent up within the cavity. When a free opening is made in the ordinary way, this stimulus is got rid of; but the atmosphere gaining access to the contents, the potent stimulus of decomposition comes into operation, and pus is generated in greater abundance than before. But when the evacuation is effected on the antiseptic principle, the pyogenic membrane, freed from the influence of the former stimulus without the substitution of a new one, ceases to suppurate (like the granulations of a sore under metallic dressing), furnishing merely a trifling amount of clear serum, and, whether the opening be dependent or not, rapidly contracts and coalesces. At the same time any constitutional symptoms previously occasioned by the accumulation of the matter are got rid of without the slightest risk of the irritative fever or hectic hitherto so justly dreaded in dealing with large abscesses.

In order that the treatment may be satisfactory, the abscess must be seen

before it has opened. Then, except in very rare and peculiar cases, there are no septic organisms in the contents, so that it is needless to introduce carbolic acid into the interior. Indeed, such a proceeding would be objectionable, as it would stimulate the pyogenic membrane to unnecessary suppuration. All that is necessary is to guard against the introduction of living atmospheric germs from without, at the same time that free opportunity is afforded for the escape of discharge from within.

I have so lately given elsewhere a detailed account of the method by which this is effected, that it is needless for me to enter into it at present, further than to say that the means employed are the same as those described above for the superficial dressing of compound fractures—namely, a piece of rag dipped in the solution of carbolic acid in oil, to serve as an antiseptic curtain, under cover of which the abscess is evacuated by free incision; and the antiseptic paste, to guard against decomposition occurring in the stream of pus that flows out beneath it: the dressing being changed daily till the sinus has closed.

The most remarkable results of this practice in a pathological point of view have been afforded by cases where the formation of pus depended upon disease of bone. Here the abscesses, instead of forming exceptions to the general class in the obstinacy of the suppuration, have resembled the rest in yielding in a few days only a trifling discharge; and frequently the production of pus has ceased from the moment of the evacuation of the original contents. Hence it appears that caries, when no longer labouring, as heretofore, under the irritation of decomposing matter, ceases to be an opprobrium of surgery, and recovers like other inflammatory affections. In the publication before alluded to I have mentioned the case of a middle-aged man with psoas abscess depending on diseased bone, in whom the sinus finally closed after months of patient perseverance with the antiseptic treatment. Since that article was written I have had another instance of success, equally gratifying, but differing in the circumstance that the disease and the recovery were both more rapid in their course. The patient was a blacksmith who had suffered four and a half months before I saw him from symptoms of ulceration of cartilage in the left elbow. These had latterly increased in severity, so as to deprive him entirely of his night's rest and of appetite. I found the region of the elbow greatly swollen, and on careful examination discovered a fluctuating point at the outer aspect of the articulation. I opened it on the antiseptic principle, the incision evidently penetrating to the joint, giving exit to a few drachms of pus. The medical gentleman under whose care he was (Dr. Macgregor, of Glasgow) supervised the daily dressing with the carbolic-aid paste till the patient went to spend two or three weeks at the coast, when his wife was entrusted with it. Just two months after I opened the abscess he called to show me the limb, stating that the discharge had for at least two weeks been as little as it then was—a trifling moisture upon the paste, such as might be accounted for by the little sore caused by the incision. On applying a probe guarded with an antiseptic rag, I found that the sinus was soundly closed, while the limb was free from swelling or tenderness; and, although he had not attempted to exercise it much, the joint could already be moved through a considerable angle. Here the antiseptic principle had effected the restoration of a joint which on any other known system of treatment must have been excised.

Ordinary contused wounds are of course amenable to the same treatment as compound fractures, which are a complicated variety of them. I will content myself with mentioning a single instance of this class of cases. In April last a volunteer was discharging a rifle, when it burst, and blew back the thumb with its metacarpal bone, so that it could be bent back as on a hinge at the trapezial joint, which had evidently been opened, while all the soft parts between the metacarpal bones of the thumb and forefinger were torn through. I need not insist before my present audience on the ugly character of such an injury. My house-surgeon, Mr. Hector Cameron, applied carbolic acid to the whole raw surface, and completed the dressing *as if for compound fracture*. The hand remained free from pain, redness, or

swelling, and with the exception of a shallow groove, all the wound consolidated without a drop of matter, so that if it had been a clean cut, it would have been regarded as a good example of primary union. The small granulating surface soon healed, and at present a linear cicatrix alone tells of the injury he had sustained, while his thumb has all its movements and his hand a firm grasp.

If the severest forms of contused and lacerated wounds heal thus kindly under the antiseptic treatment, it is obvious that its application to simple incised wounds must be merely a matter of detail. I have devoted a good deal of attention to this class, but I have not as yet pleased myself altogether with any of the methods I have employed. I am, however, prepared to go so far as to say that a solution of carbolic acid in twenty parts of water, while a mild and cleanly application, may be relied on for destroying any septic germs that may fall upon the wound during the performance of an operation; and also that for preventing the subsequent introduction of others, the paste above described, applied as for compound fractures, gives excellent results. Thus I have had a case of strangulated inguinal hernia, in which it was necessary to take away half a pound of thickened omentum, heal without any deep-seated suppuration or any tenderness of the sac or any fever; and amputations, including one immediately below the knee, have remained absolutely free from constitutional symptoms.

Further, I have found that when the antiseptic treatment is efficiently conducted, ligatures may be safely cut short and left to be disposed of by absorption or otherwise. Should this particular branch of the subject yield all that it promises, should it turn out on further trial that when the knot is applied on the antiseptic principle we may calculate, as securely as if it were absent, on the occurrence of healing without any deep-seated suppuration, the deligation of main arteries in their continuity will be deprived of the two dangers that now attend it—viz., those of secondary hemorrhage, and an unhealthy state of the wound. Further, it seems not unlikely that the present objection to tying an artery in the immediate vicinity of a large branch may be done away with; and that even the innominate, which has lately been the subject of an ingenious experiment by one of the Dublin surgeons, on account of its well known fatality under the ligature from secondary hemorrhage, may cease to have this unhappy character, when the tissues in the vicinity of the thread, instead of becoming softened through the influence of an irritating decomposing substance, are left at liberty to consolidate firmly near an unoffending though foreign body.

It would carry me far beyond the limited time which, by the rules of the Association, is alone at my disposal were I to enter into the various applications of the antiseptic principle in the several special departments of surgery.

There is, however, one point more that I cannot but advert to—namely, the influence of this mode of treatment upon the general healthiness of an hospital. Previously to its introduction, the two large wards in which most of my cases of accident and of operation are treated were amongst the unhealthiest in the whole surgical division of the Glasgow Royal Infirmary, in consequence, apparently, of those wards being unfavourably placed with reference to the supply of fresh air; and I have felt ashamed, when recording the results of my practice, to have so often to allude to hospital gangrene or pyæmia. It was interesting, though melancholy, to observe that, whenever all, or nearly all, the beds contained cases with open sores, these grievous complications were pretty sure to show themselves; so that I came to welcome simple fractures, though in themselves of little interest either for myself or the students, because their presence diminished the proportion of open sores among the patients. But since the antiseptic treatment has been brought into full operation, and wounds and abscesses no longer poison the atmosphere with putrid exhalations, my wards, though in other respects under precisely the same circumstances as before, have completely changed their character; so that during the last nine months not a single instance of pyæmia, hospital gangrene, or erysipelas, has occurred in them.

As there appears to be no doubt regarding the cause of this change, the

importance of the fact can hardly be exaggerated.—*Lancet, Sept.* 21, 1867, *p.* 353.

41.—ON THE USE OF CARBOLIC ACID IN WOUNDS, COMPOUND FRACTURES, &c.

By Dr. J. R. Wolfe, Aberdeen, and Sir J. Y. Simpson, Edinburgh.

[The merit has been given to Prof. Lister, of Glasgow, of having been the first to apply carbolic acid to wounds, compound fractures, &c. Dr. Wolfe, however, states that he had experimented on the use of carbolic acid, both externally and internally, since the summer of 1864, as will be seen from the following passage from his paper published in the *Med. Times and Gazette* of Nov. 25, 1865, which will be found in *Retrospect*, Vol. 53, p. 108.]

"The only objection to the use of carbolic acid is its strong and rather disagreeable smell. To obviate this I have made some experiments, and find that cotton wool saturated in the pure acid, and then pressed to get rid of the excess of acid, and dried, leaves enough of the acid to act upon the wound without leaving so much as to make the smell disagreeable. The cotton so prepared is kept in a tin pot to prevent volatilisation of the acid. As far as my experiments have gone I am satisfied with its beneficial results. It causes a healthy granulating surface, contracting the tendency to hyperplasia and suppuration. But my study of the subject is not sufficiently advanced to enable me to enter into particulars in this communication. I may express it, however, as my opinion, that its employment for dressing wounds will greatly lessen the risk of purulent and erysipelatous infection in our surgical wards."

It was, therefore, with the highest satisfaction that I read those interesting papers by Prof. Lister in the Lancet of the current year, referred to in your leading article of last week; knowing that, with the vast materials at his disposal in the Glasgow Infirmary, that eminent surgeon would be able to do ample justice to that important subject.

Yet I must add that my belief in the efficacy of carbolic acid is not founded upon the theory of M. Pasteur; for after the counter experiments by MM. Joly and Pouchet, the *panspermatism* hypothesis rests, in my opinion, upon debatable ground. I am rather inclined to attribute the action of carbolic acid entirely to its antiseptic properties—that is, of preserving organisable matter from passing into a putrescent condition. Indeed, the remarkable cases of Professor Paget, quoted in your article referred to, seem to militate against the idea of basing its beneficial action upon M. Pasteur's doctrine; for, seeing that "the best results in case of pyæmia were in patients that had been kept day and night in a current of wind," this tends to show that it is not the atmosphere, fraught with sperms or sporules, which acts deleteriously, but rather the contaminated *hospital air* which commits such havoc upon wounds, against which the carbolic acid is a valuable protector and modificator.—*Lancet, Sept.* 28, 1867, *p.* 410.

[Sir James Simpson also disputes the originality of Prof. Lister's views on this subject, and he gives good reasons for what he says. Prof. Lister's papers have all been published in 1867, but in 1863, Dr. Lemaire, of Paris, published a work on the same subject. Sir James says:]

Originally in France carbolic acid was employed in surgery in the form of a powder made of coal-tar and lime, as suggested by MM. Corne and Demeaux in 1859. Shortly after its introduction surgical experiments were made with it in the hospitals of Paris and in the French army in Italy after the battles of Magenta and Solferino. These experiments gave it, to use the language of Dr. Lemaire, "un grand retentissement." "Le fait principal," he adds, "la désinfection, était admis et vanté avec enthousiasme." M. Cabannes subsequently suggested the mixture of coal-tar with other mineral and various vegetable powders, besides gypsum; but their introduction was, as stated by Lemaire, difficult when the wounds were anfractuous or fistulous. *Afterwards,* M. Le Boeuf made a still more elegant and useful preparation

of the coal-tar, in the form of a fixed emulsion with saponine; a form which Dr. Lemaire seems still to prefer, in some surgical complications, to the pure carbolic acid, diluted or otherwise modified.

At last, Mr. Calvert, in England, and Parisel and Bouchardat, in France, showed by their chemical investigations that carbolic acid is the essential principle to which coal-tar owes its antiseptic properties, as opium owes its soporific properties to morphia, cinchona its febrifuge properties to quinine, &c.

Mr. Lister remarks (Lancet, Sept. 21st, 1867), that when it had been shown, by the researches of Pasteur, that the septic property of the atmosphere depended on minute living organisms suspended in it, "it occurred to me," to use his own words, "that decomposition in the injured part might be avoided, without excluding the air, by applying as a dressing some material capable of destroying the life of the floating particles. Upon this principle I have based a practice. . . . . The material which I have employed is carbolic or phenic acid, a volatile organic compound, which appears to exercise a peculiarly destructive influence upon low forms of life."

Now the very same theory, the very same practice, and the very same means of reducing that theory to practice, have all been worked out and published on the continent years ago by Lemaire, Déclat, Küchenmeister, and others. The strongest averments cannot alter the simplest dates, and I fear that it would not redound to the credit of English surgery to claim what most certainly does not pertain to it.

In his work on Carbolic Acid, Dr. Lemaire enters elaborately and philosophically into the question of septic infusoria in the atmosphere, and discusses the opinions of Schultze, Schwann, Pasteur, Pouchet, Helmholtz, Milne-Edwards, Bernard, Gratiolet, and others, upon the subject. His conclusions are that the air does contain these alleged low vital organisms; that when admitted to wounds, ulcers, &c., they produce a decomposition in the part and secreted fluids, aiding the formation of pus; that this decomposition is effected by a vital action similar to the production and multiplication of organisms in the process of fermentation; and that carbolic acid—even in very small doses—has the power of preventing and of arresting any such decomposing effects from these organisms by at once and immediately destroying the life of the organisms themselves.

Dr. Lemaire has applied carbolic acid to the treatment of numerous *Other Surgical Complications* and diseases besides the three especially mentioned by Mr. Lister. In his work he has adduced, for example, cases and remarks to show its utility as an antiseptic and healing agent in simple scrofulous ulcers, &c.; in burns; in venomous bites; in dissection wounds; in sloughing and gangrene of the soft tissues, (the ravages of gangrene, he says, have often thus been stopped as if by enchantment;) in inflammation, caries, and necrosis of bone; in inflammation and abscess of joints; in whitlows; in carbuncles, (where its action, he avers, is marvellous, probably from its containing living organisms, which he thinks he has detected with the miscroscope;) in lupus; in cancerous discharges; in ozæna and otorrhœa; in chancre; in gonorrhœa; in catarrh of the bladder as an injection, and as an injection also in fistulæ, and into the cavities of cysts, &c. He gives various disquisitions, with cases illustrative of its employment in different forms of skin diseases, particularly in those which arise from the presence and irritation of low animal and vegetable organisms. It aborts pustules of cow-pox and small-pox when applied to them. Internally, again, he has used it in the form of vapour, solution, &c., to aphthæ, angina, diphtheria, croup, whooping-cough, &c., and in some diseases of the intestinal canal, as dyspepsia, (would not the form complicated with sarcinæ be benefited by it as much as by sulphurous acid compounds?), in dysentery, cholera, &c. And he suggests further trial of it internally in some contagious diseases, as scarlatina and measles; in agues and malarious diseases, and various other maladies, where low vegetable or animal organisms may be possibly supposed to exist. This rapid and imperfect enumeration shows in itself the great thought and attention which Dr. Lemaire has bestowed on the thera-

peutical action and applications of carbolic acid; in that respect, as well as in originality, leaving Prof. Lister very far behind him. On one or two points—but these of a most doubtful character—Prof. Lister goes, perhaps, further, for he seems actually to believe that the use of carbolic acid may lead to the absorption of a piece of necrosed bone, or of a silken ligature when cut short and left in a wound—a kind of surgical belief in which I think he will get few or no disciples to join him at the present day, though such physiological deeds were credited by some of the olden surgical pathologists.

They seem to have used carbolic acid, therapeutically and surgically, earlier in Germany than in France. In the Year-Book of the Sydenham Society for 1860, at p. 486, we find a notice of the disinfection of the sewage of Carlisle by carbolic acid, and brief analyses of Demeaux's disinfecting charpie, and of Corne and Demeaux's disinfecting powder; and at p. 487 a notice of the disinfectant powers of carbolic acid itself, by Dr. Küchenmeister of Dresden. "Under the name of spirol," it is said, "Küchenmeister describes phenic or carbolic acid as a colourless crystalline body, which fuses at 34° (cent.), and boils at 187°; and is obtained from oil of coal tar, as well as by the distillation of salicine with lime. Küchenmeister has applied the carbolic acid with the most satisfactory results, both in medical and surgical practice, and as a means of arresting putrefaction and *preventing the development of fungi.*" This last remark is the more interesting as it preceded the observations of Pasteur.

One great and most laudable object which Professor Lister evidently has in view in using carbolic acid as a local dressing to wounds is to close these wounds entirely by the first intention, and without any suppuration; so far, this means, averting the mischances of surgical fever. But these paramount objects have been attained in the hospital of Aberdeen by the use of acupressure. In the Surgical Section of the Association to which Mr. Lister read his paper in Dublin, Dr. Pirrie, of Aberdeen, stated that, for example, out of twelve cases of excision of mammary tumours treated with acupressure in his practice during the last two or three years, eight out of the resulting wounds had closed entirely by the first intention, and without the formation of a single drop of pus.—*Lancet, Nov.* 2, 1867, *p.* 546.

[Nevertheless, we think that great credit is due to Professor Lister for so prominently bringing this treatment before the profession, and although there seems to be "nothing new under the sun," it is almost as creditable to bring an old thing into practice as to originate it at first.]

42.—THE EMPLOYMENT OF CHLORIDE OF ZINC AND CARBOLIC ACID IN WOUNDS.

Case under the care of JOHN WOOD, Esq., at King's College Hospital.

[During the two or three months immediately preceding the publication of the following short article, carbolic acid and chloride of zinc have been extensively used by the assistant-surgeons at King's College Hospital. The former is used to suppurating wounds, and the latter to the parts involved in formidable operations.] (See article by Mr. De Morgan, *Retrospect*, Vol. 53, p. 147.)

On Aug. 31st a man was admitted into the hospital, whose hand had just been severely injured by the explosion of a box of detonating powder. The wrist and palm were very much lacerated, two of the metacarpal bones were comminuted, and the skin and tendons were stripped off from the phalanges of the fingers. The hand was amputated at the wrist joint, and a thin slice removed from the ends of the radius and ulna. Before the flaps were brought together, a solution of chloride of zinc, thirty grains to the ounce, was applied to every part. Although the incisions were made in close *proximity* to the seat of injury, and damaged tissue was included in the *flaps, the wound rapidly* healed. There was very little suppuration, and on

Sept. 4th the man was discharged from the hospital. On Sept. 12th the wound was completely healed, and the man presented a sound, firm, and painless stump.

On Sept. 8th a large and rapidly growing tumour was removed from the gluteal region of a man, 25 years of age. This growth was of eighteen months' duration, larger than the adult head, and evidently of a malignant character. Two days before the operation the man had a febrile attack, the tumour became painful, and the skin covering it inflamed and glossy. When the growth was exposed by extensive skin incisions, its upper and superficial part was found to have degenerated into a black, semifluid mass, of an intolerably fetid odour; this was scooped out by the hand, and the deep-seated parts, which were white, firm, and marked with all the external characteristics of encephaloma, were carefully dissected away from the recesses of the gluteal region. The sacro-sciatic ligament were exposed, and round this a lobe of the tumour passed into the interior of the pelvis. The black and unhealthy-looking muscular tissue forming the thickness of the flaps was then swabbed with the solution of chloride of zinc. The man has progressed favourably since the operation, and without a single bad symptom. A drainage-tube was passed through the wound shortly after, but, notwithstanding the presence of this, the edges of the incision quickly united, and the wound is now healing with remarkable rapidity considering its depth and extent, and without any discharge of pus. Carbolic acid has been applied superficially, and the drainage-tube was passed in order to prevent putrid and unhealthy fluids from collecting in the deep parts of the gluteal region.—*Lancet, Oct.* 12, 1867, *p.* 455.

43.—SULPHUROUS ACID AS APPLIED TO WOUNDS AND SORES.

By Dr. James Dewar, Kirkcaldy.

It is obvious that any means of promoting the speedy union of parts which have been divided during surgical operations would materially contribute to the patient's advantage, and lessen the risks to which he is otherwise exposed. The following case, as showing that the existence of pus is not only an unnecessary but a preventible accident, has an important bearing upon the subject of surgical fever, and would encourage the hope that even over it we may be able to exercise some control by anticipating the evils associated with its existence. I have long been impressed with the fact of there being an antagonism between sulphurous acid and pus, but have not till now had an opportunity of fully testing its value. Miss —, a young woman with a tumour in her breast; she had chloroform, and the tumour, which was about the size of a half-closed fist, was removed by a wound of about six inches long. There was little bleeding, and there was no occasion for ligature. The raw surface was carefully sponged with sulphurous acid, and the edges adjusted by four silver sutures. A piece of lint soaked in the liquid was laid over the wound, and this covered with gutta-percha, the dressing being changed every six hours. From that time till now the patient has never had the slightest uneasiness in the wound, which she could bear to have handled without apprehension; indeed, she said that if she did not see the wound she would not be aware of its existence. Union was complete within twelve hours. The stitches were removed on the third day, and two days thereafter my patient was in the garden. She assures me that there never was a stain upon the dressing.

A young man had his hand severely cut when working at a circular saw. The wound was closed in the usual way, and the acid applied, as in the preceding case. The pain instantly ceased, and the edges have since united without any appearance of pus.

I may mention that some months ago Professor Syme had a case in which the result was very similar. He told me that the sulphurous acid spray was applied to the raw surface, and that when he went to dress it for the first

time he was delighted to find the healing process in an unusual state of forwardness.

I hope that some of your readers will be induced to give this mode of treatment a trial, and have every confidence that their experience of it will be satisfactory. Sulphurous acid is superior in efficiency to carbolic acid, and is entirely free from the objection applicable to the latter—viz., of being a powerful irritant and having a disgusting smell.—*Medical Times and Gazette, Sept.* 21, 1867, *p.* 318.

44.—ON THE TREATMENT OF INCISED WOUNDS WITH A VIEW TO UNION BY THE FIRST INTENTION.

By James Syme, Esq., F.R.S.E., Surgeon to the Queen in Scotland.

[Observations lately made as to the effect of atmospheric atoms in causing decomposition, has led to a complete revolution of ideas on the subject of the treatment of wounds and abscesses. The observations were originally made by M. Pasteur, and to Prof. Lister, of Glasgow, is due the credit of first applying them in the surgical treatment of disease.]

In cases of bruise, fracture, dislocation, and even operations of tenotomy, large quantities of blood are frequently effused more or less deeply under the integuments without causing any bad effect, and quickly disappear by means of absorption. How, then, does it happen that blood collected in the cavity of a wound should be productive of so much mischief? It can only do so, as Mr. Lister has shown, through the decomposing influence of atmospheric air, loaded with its myriads of organic atoms, and, therefore, if protected from this agency, would be no more hurtful than in the circumstances just mentioned. He has accordingly found, as stated in the preceding numbers of this journal, that wounds of the most formidable character may be divested of all their alarming features by means of carbolic acid, applied so as to prevent the impure air from entering.

This remarkable fact has led me to consider the expediency of resorting more frequently than heretofore to the use of "torsion" for the suppression of hemorrhage. Ever since this method was proposed by the late M. Amussat I have employed it in wounds of the cheek and lips, and also occasionally in more serious operations, to let my pupils see that it was quite effectual, but have been prevented from adopting it for general use, by fearing that the absence of ligatures, which serve as conductors of the discharge, would increase the risk of blood or serum being pent up in the cavity. Feeling now happily relieved from any apprehension of evil being thus produced, I think "torsion" may in many, if not in all cases be employed with advantage, instead of the ligature. In order to perform the process effectually, it is necessary that the artery should be seized by catch-forceps, and twisted until they become loose. It has been alleged that such a liberty with the vessel must cause it to slough, and thus disturb the adhesive action. But as this objection is altogether theoretical and contradicted by experience, it is unworthy of notice.

In illustrations of what has been said, the two following cases may be mentioned as sufficient to substantiate the facts—

1st. The torsion effectually restrains the hæmorrhage of ordinary-sized arteries.

2nd. That its action upon them does not prevent union by the first intention.

3rd. That protection from the air prevents decomposition of the blood.

Case 1.—On the 27th of May I amputated the middle finger of a young woman in the hospital at the metacarpal joint for disease of three years' standing, and arrested the bleeding by torsion of the arteries, which were three in number. A mixture of carbolic acid and linseed oil, in the proportion of one to five, having then been freely applied to the surface of the wound, I tied the adjoining fingers together so as to retain the cut edges in contact, and placed over them a piece of lint soaked in the carbolic mixture. *The result at the end* of three days was complete and perfect union without *any discharge of matter*.

Case 2.—On the 11th of June Mr. Annandale removed a tumour from the chest of a male patient by an incision ten inches in length, and employed the torsion for five arteries, two of which were of considerable size. He then applied the carbolic mixture freely to the wound, brought its edges together by silver sutures, and covered them with lint soaked in the protective fluid. Three days afterwards I saw the wound soundly healed without a drop of matter.

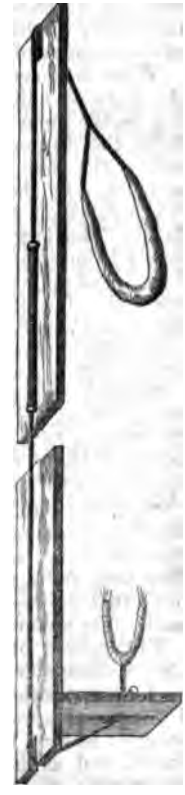
The following case is sufficient to prove—if proof were wanting—that ligatures do not prevent primary union beyond the extent which they occupy.

Case 3.—On the 11th of June a young man was admitted into the hospital on account of a large brachial aneurism, the size of an orange, at the bend of his elbow, which had resulted from the thrust of a file two months before the time of application. I laid open the sac to its full extent, removed the blood and coagulum, tied the humeral artery on both sides of the orifice, twisted a small vessel that bled, rinsed out the cavity with the carbolic mixture, and applied a piece of lint soaked in it on the wound. There did not follow the slightest local or constitutional disturbance; and complete adhesion took place, except at the small point where the ligatures lay, from which a few drops of pus were discharged. On the fifth day one of the ligatures came out, and on the sixth the other; so that on the seventh day the recovery might be regarded as perfect.—*Lancet, July* 6, 1867, *p.* 5.

45.—ON THE TREATMENT OF FRACTURES BY ELASTIC EXTENSION.

By H. Worthington, Esq., Late House-Surgeon to the Middlesex Hospital.

The many cases of fracture of the lower extremities admitted into this hospital, and the labour and pain necessitated in their reduction and successful treatment, induced the trial of elastic extension. Its mode of application is easy, and the results, as far as they have gone, have proved highly satisfactory. The following is a description of the way in which elastic extension is applied at the Middlesex Hospital.



The splint employed is of suitable size, and a modification of that recommended by Mr. Barwell for extending the leg in hip-joint disease. It consists of a long, straight, stout splint, with a piece of wood about a foot in length let in at right angles to it about six inches from its lower end. The long arm of the splint is well padded on its inner side. An inch or two from the upper end of the splint is a small pulley, fixed in an aperture made for the purpose: a similar one is placed at the lower end. That part of the splint which is let in at right angles, is provided with a slit extending nearly its whole length, admitting another pulley of similar size, which can be shifted to various parts by means of an iron pin passing through holes bored in the sides of the cross-piece. A strong piece of strapping is now applied to the sides of the leg, leaving a loop at the sole of the foot as in the ordinary way, care being taken that the loop shall be large enough to admit a tightly rolled leg-bandage; this being placed transversely at about the middle of the sole of the foot, so that it is enabled to receive all lateral pressure when extension is applied. Without this the strapping would cut the hollow and outer side of the foot, and give rise to considerable pain and inconvenience. In cases of fracture of the leg, the strapping should not come above the seat of injury. The foot should now be bandaged, so as to retain the strapping firmly. A piece of strong cord is then fastened to the loop of

plaster, so as to include it and the roller, and this gives us a fixed point to pull upon from below. The cord, thus fastened, is next passed over the pulley which lies in the slit in the cross-piece, then over the pulley at the lower end of the splint, and is carried to about the middle of the outside of the long side-piece. Here it is attached to one end of a patent elastic door-spring, or accumulator. To the upper end of the elastic is attached a piece of wood, about four inches long, one inch broad, and half an inch thick, perforated by three holes equidistant, and just large enough to admit the cord used. By one of these holes it is secured to the elastic. A well-padded perineal band is now applied, to the upper part of which a separate piece of cord is fastened. This is brought over the pulley at the upper end of the splint, and then made to perforate successively the two remaining holes in the piece of wood attached to the elastic. It will now be evident that, by slipping this piece of wood up the cord fastened to the perineal band, the elastic will be put on the stretch, and, in this way, extension may be regulated to a nicety.

The limb may now be brought into position, and extension applied as far as is deemed advisable. It does not appear necessary to reduce the fractured limb to its normal length on the immediate application of the splint; for if this be not directly obtained, the constant extension which is kept up gradually overcomes the contraction of the muscles; and with very little adjustment, the limb will be found to have gained its proper length in the course of two or three days, the extension being increased or diminished according to the necessities of the case.

There are many advantages besides this. Apart from doing away with the forcible extension necessary to overcome a spasmodic contraction of the muscles, which often gives rise to so much pain and subsequent excessive irritability of the parts, there are many reasons for urging the trial of the above-mentioned plan, more especially in fractures of the thigh.

With the exception, perhaps, of an occasional twist once or twice round the knee or ankle to steady the limb, no bandaging is required; or a sand-bag on the inner side of the extremity will generally effect all that is wanted. With the dispensing of bandages, not only is the patient relieved of much pain at the time of putting up the fractured limb, but also of all distress occasioned by their subsequent renewal and adjustment. The surgeon's time is saved, which is no small gain if he has several such cases to attend to daily. The large surface of skin remains exposed. Thus its excreting power is unimpaired; and the irritation which is occasioned by its excretions being pent up, and which at times becomes almost unbearable, is altogether avoided. The limb can be cleansed daily without interfering with the appliances; and every practical surgeon knows the value of this measure. Moreover, with a momentary glance, it can be ascertained whether the position of the limb is good, and any irregularity remedied.

Another very great advantage is, the being able to place the splint and perineal band on the sound side, as has been for some time practised by Mr. De Morgan in cases of hip-disease. This, of course, will be especially useful when there is abrasion or bruising of the injured side, which would be aggravated by anything coming into contact with it. When the splint is used in this way, a sand-bag on each side of the limb will be sufficient to steady and support it. With the old Liston-splint, and its mode of application, requiring, as it at times does, great force in reducing the fracture, and then even failing to obtain proper length, it has frequently occurred to my mind that the separated fragments are often either too far apart, or form with each other a more or less obtuse angle.

The amount of force used, and the mode of fixing above and below, seem almost incapable of bringing the ends of the fractured bone into close and perfect apposition. The present mode of using the elastic is calculated, I think, to insure this. The extension is equalised; the power above being as great as that below the seat of fracture, over-lapping is more easily corrected, and, as the contraction of the muscles is overcome, the elastic, by its *contraction, tightens* the cords, so that sufficient extension is always kept

up. As the limb lengthens, the elastic of course shortens; and this shortening of the elastic is very essential; for, as the limb attains its normal length the extension ought to be diminished, otherwise the fractured ends might possibly be pulled too far apart. This required diminution of tension is brought about by the lengthening of the limb. Both objects are effected at the same time, the one by means of the other. When once the separated ends of the bone are placed in good apposition, the length of the limb is easily maintained, slight tension being required to steady them on the one hand, while the contraction of the muscles assist in doing so on the other. The result is close apposition of the parts. This, of course, facilitates and hastens union, and prevents anything like shortening.

Mr. Nunn is of the opinion that, by means of this extension, the normal co-ordinated tension of the muscles is able to keep the bones in proper position.

The appended cases will show with what results this mode of elastic extension has been used. If space allowed, several could be added, besides which we have many more now under similar treatment, all promising like success. This plan could not, of course, be adopted in cases complicated with delirium of any kind, but there appears no reason why it should not be tried in oblique compound fractures, where so much difficulty is experienced in their reduction.

Case 1.—James E., aged 16, was admitted under Mr. Nunn's care on January 11th. Right leg short by an inch and a half; foot everted; oblique fracture of femur at junction of lower with middle third; very considerable effusion and tension around seat of fracture; swelling around, and effusion into knee-joint. Elastic extension on sound side, owing to an ulcer in the right groin; ice to knee; proper length obtained. Feb. 12th. Splint removed; firm union, but slight thickening; length perfect.

Case 2.—Charles H., aged 13, was admitted February 6th, under the care of Mr. Moore. Right leg short by two inches; foot everted; crepitus at junction of middle with upper third of right femur. Elastic extension was applied. Shortening less by one inch. February 7th. Shortening less by one inch and a half. February 9th. Length normal. March 9th. Splint removed; firm union; length normal. Discharged March 10th.

Case 3.—Thomas K., aged 44, was admitted under Mr. Nunn on January 12th. Very oblique fracture of tibia, with fractured fibula; lower fragment of tibia riding, but thinly covered with skin; considerable ecchymosis and swelling. Ordinary back iron-splint was applied; remained on this splint fifteen days; constant adjustment and alteration required to keep fragments in position, but without success. Elastic extension. Fragments reduced to proper position. In three weeks, there was firm union; line and length of tibia quite normal, but slight thickening of tibia to indicate seat of fracture.

Case 4.—John W., aged 43, was admitted under the care of Mr. Nunn on January 10th. Oblique fracture of tibia at middle third, with fractured fibula; lower fragment drawn up above upper one. Back splint and lateral splint were in succession applied, with very unsatisfactory results. February 7th. Elastic extension applied. Fragments easily reduced to their normal position. On the 21st, the splint was removed. Firm union; length normal; line of tibia almost uninterrupted. On the 27th, he was getting about with the help of sticks.

Case 5.—William C., was admitted on February 19th. Transverse fracture of tibia and fibula about four inches above the malleoli; considerable displacement, with shortening and bruising. Elastic extension was applied on March 18th. Firm union; but slight thickening on inner side of tibia, to indicate seat of fracture.—*British Medical Journal, July* 20, 1867, *p.* 42.

46.—ON FRACTURES OF THE NECK OF THE THIGH-BONE.

By Samuel Solly, Esq., F.R.S., Senior Surgeon to St. Thomas's Hospital.

[In Guy's Hospital museum are contained a number of specimens of united fracture of the neck of the thigh-bone. These were presented by Mrs. Hodgson, the widow of Mr. J. R. Hodgson, of Chesham, Bucks. They have been previously described by him in the Guy's Hospital Reports.]

The treatment in these cases was the use of the double inclined plane. The plane was made of pillows sewed together; the foot was fixed to the sacking at the foot of the bed by a handkerchief folded round the ancle and over the instep and tied to the sacking. The thigh and leg were fixed by shawls folded obliquely round the limb above and below the ham, and secured to the under pillow of the inclined plane. This was maintained for fourteen or sixteen weeks.

I attribute Mr. Hodgson's success in these cases to his not disturbing the impacted fragments. His words show this. "I would also remark that, in making out the diagnosis, it is not at all necessary to feel crepitus." The whole paper is worth perusal, and as I was not aware of its publication until I went to the college to examine the specimens of this injury, I feel sure that I am not wasting your time in calling your attention to it now.

I will next give the history of a case which, though not illustrated by a post-mortem examination, is, I consider, equally conclusive as to the possibility of obtaining union in these fractures.

About five years ago I had under my care in private practice a well-marked case of this injury. The subject of the accident was a lady between fifty and sixty, but in some respects old for her years. She slipped on a flagstone in the garden, and fell suddenly on the right hip. She was carried with some difficulty into the house. A general practitioner in the neighbourhood was sent for, and, without making a sufficiently searching examination, perhaps from false delicacy, said that he would go home and get his pulleys. The lady, who was a very intelligent woman, not liking the idea of the process, then suggested that I should be sent for. This was acceded to at once. The general practitioner met me at the station, and told me the injury was a dislocation on to the pubes. Now, although, as a rule, fracture of the neck of the thigh-bone is the lesion which we have most reason to expect in old age when the injuring force is applied to the hip, I have seen dislocation on the pubes in a man aged seventy. He was the rector of Seaford, and I was called to the case by my friend Mr. Sanger, of Alfreston. With the help of pulleys and muscular power from the coast-guard station, we managed to return the bone into the socket. Our reverend friend got quite well, and lived four or five years in the perfect enjoyment of a sound limb.

On examination, I found no dislocation, very slight shortening, extreme pain on the slightest attempt to move the thigh-bone or the hip-joint, slight yielding at that spot, but no positive crepitus. I handled it very gently, for being now convinced that there was fracture of the neck of the thigh-bone, I took care not to displace the fragments. I treated her on the plan I have described. I moved her once a week from the couch to have it cleaned and made comfortable, invariably lifting the patient myself; and by keeping one hand over the trochanter I prevented the slightest movement taking place. At the end of eight weeks I allowed her to sit in a chair, and in another week to stand. At first there was very little motion in the joint under the action of her own muscles. On moving it with my own hands it appeared to lock, like a hinge of a door into which a nail or screw has accidentally slipped, stopping the normal circle of motion. This impediment evidently arose from the amount of uniting callus which had been poured out, interfering with the hinge. I employed passive motion gently but freely, and I was rewarded by seeing her at last walk perfectly. There was no more than a quarter of an inch of shortening. Her sister told me that at the end of nine months she astonished and frightened her by saying, "See, Mary, what I *can do," and then running* down a flight of stairs like a girl of eighteen.

Thirty years before this accident had occurred, the then proprietor of the house, which was a fine old family mansion in the country, was the subject of the same injury. He was a patient of Sir Astley Cooper, and lived several years after the injury, but never recovered the use of his leg. His injury most probably differed from the case just related in being a fracture within the capsule, and would therefore be more difficult to cure. No surgeon, however skilfully he may treat these cases, can *secure* union; but that is no reason for not doing our best to effect it.

I was young in the profession when I learned, from personal experience, that you may examine a case of this kind of fracture and not discover the nature of the injury, My patient was an oldish lady—by oldish I mean about sixty—who actually walked into my consulting room. There were none of the usual symptoms of fracture—I suppose from its complete impaction—and I did not treat it as such. She was very anxious to go down to Edinburgh, to which I gave my consent. When there she was worse, and consulted Mr. Lizars, who detected fracture; and when he heard that I had pronounced a different opinion, he said, "Yes, it is very easy for me now to detect this injury, for the bones are in a very different condition to what they were when Mr. Solly saw you." The journey to Edinburgh had jolted them asunder. I do not think I have made the same mistake since, and my principal guide has been the severity of the pain and its persistence.

In some of these cases of fracture there is considerable shortening, and it has been recommended that the limb should be drawn down by means of a weight hung over the end of the couch. This manœuvre must be adopted with great care, lest you detach the fractured portions; and you must remember that a shortened but firm limb is better than an ununited fracture and a feeble, almost useless limb.

This beautiful specimen of united fracture of the neck of the thigh-bone was given me by a surgeon, who witnessed the accident, traced it at the time, and examined the joint after death, which occurred about ten years subsequently. The patient wore an extra sole 1¾ in. thick to his shoe on this side. This drawing I made from the specimen, and have since deposited one half in our museum, and the other half in that of the College of Surgeons. I give the history, a little curtailed, which I have received.

S. N., aged 44, bricklayer, fell into a cesspool, from the giving way of a slate which he had placed across the top. The left leg went down first, and the right leg was doubled up under him. Surgical attendance was immediate. The appearances presented were those of dislocation on to the hip. This was reduced by the aid of pulleys, and the patient placed in bed. About an hour afterwards he complained that, though he had kept perfectly quiet, the bone was out again. On this occasion it was clear that the injury was a fracture of the head of the thigh bone extending into the trochanter major. Now, my friend who has given me this account, a very good surgeon, firmly believes in this order of events. I doubt it, and believe that it must have been a fracture from the first, the projection of the displaced trochanter giving the appearance of dislocation. I have seen the neck of the thigh-bone broken by the surgeon during a futile attempt to reduce a dislocation; but when that occurred the dislocation was not reduced, and the fracture was transverse, not impacted. The accident was recognised immediately. I feel certain that it would not have occurred in the case without my friend immediately discovering the mischief he had done.

R. W. Smith, speaking of these cases and the difficulty of diagnosis, says: —"I have seen seven examples in which the foot was turned inward, in five of which the fracture was external to the capsular ligament. Such cases merit the closest attention, for they are especially liable to be confounded with luxations. I have seen this mistake committed more than once. . . . Mr. Stanley has tersely observed that among the more complicated injuries to which the hip-joint is liable, that of fracture of the trochanter major, combined with fracture of the neck of the femur, has, under certain circumstances, a strong resemblance to dislocation of the head of the bone. . . . When, from the direction *of the fracture*, one portion of the trochanter has

been drawn by the action of the muscles towards the great ischiatic notch, no crepitus will then be discoverable. A direct source of mistake will then arise from the positive resemblance of the fractured portion of the trochanter to the head of the femur; the former occupying the same place which the latter would do in dislocation; and if with these circumstances there should happen to be inversion of the injured limb, the difficulty of the diagnosis must be considerably increased."

This same author narrates a case of a middle-aged man falling in the street, his hip striking the kerb-stone. The immediate consequences were that the limb was inverted and shortened to the extent of an inch, and no crepitus could be discovered. It was presumed that a dislocation had occurred, and accordingly an extension of the limb was made. The constitutional irritation occasioned by repeated trials to reduce the supposed dislocation was such that the man died five months from the time of the occurrence of the accident. In the dissection of the hip a fracture was found, extending obliquely through the middle of the neck of the femur, but entirely within the capsule. A portion of the fibrous and synovial membranes on the anterior side of the neck of the bone had escaped laceration.

We must, after this digression, continue our narrative of the patient. He was placed on an Amesbury, or the double inclined bed similar to that which I now call Alderman's. Complete union took place, with about two inches of shortening, which was to some extent remedied by a thick sole to his shoe. He died ten years afterwards of phthisis. At the post-mortem examination the head of the thigh-bone was found in the acetabulum.

The shortening of the neck, the depression of the head, the immense amount of callus thrown out, some of it extending like a blunt spine in the course of the capsular ligament, accounts for the shortening of the limb, and the impeded motion in the joint.

We will next sum up the signs or symptoms by which you can distinguish intra-capsular from extra-capsular fractures. In giving you these landmarks, do not mistake me, and use them, as our forefathers did, to indicate a different line of treatment; or rather I should say a line of treatment in the one case and a line of no treatment, or the *do-nothing* system, in the other.

I have shown, by reference to the preparations in the College of Surgeons' museum, and also in our own, that fractures of the cervix within the capsule will unite, though not so frequently as those without. The treatment must be the same in both. You might, perhaps, as idle students, say, "Why should I bother myself with the distinctions!" For this reason, that in all surgical practice, though, perhaps, it is more important in private than in hospital practice, as your reputation may be more affected in the one than in the other, it is a matter of the greatest importance for any patient, whether a lady of high degree or a working peasant, to know after the receipt of any accident what are the prospects of recovery; whether it is likely they will be able to walk or work again, or whether they will be cripples for life. If you can diagnose that the fracture is an impacted fracture of the cervix, then you may with tolerable confidence predict complete union and sound limb; but if you have reason to believe that it is a fracture within the capsule, you must give a very guarded opinion, promising to do your best, but that you cannot honestly say that your endeavours will be crowned with success. You must acknowledge the imperfection of your art and the uncertainty of the result. You may also add that such fractures have united, when properly treated, as you intend to treat the case before you. Of course the age and constitutional power of the patient will be an essential element in favor or against the probability of union. The two extremes may be thus put: Fracture within the capsule in a very old and feeble person scarcely ever unites; fracture without the capsule in a strong and healthy person must be very badly treated if it does not unite.

Fractures within the capsule may generally be distinguished by the fact of a fracture existing being almost unequivocal; by the signs of shortening; *easy production* of crepitus after reducing the limb by traction to its normal *length; by the eversion* of the foot; by the age of the patient being greater

usually than in the instance of the impacted form. It is true that inversion of the foot does occur in the impacted, as in the case just related, but still it is not so frequent.

In impacted and extra-capsular fracture the symptoms are often obscure, as I have already shown, so much so as even to deceive well-educated surgeons into the belief that there is dislocation and no fracture. The foot, instead of being everted, is not unfrequently inverted, and all attempts to turn it inwards give rise to excruciating pain. The shortening is scarcely perceptible. Locomotion is not impossible. Patients can often stand and walk, and raise the limb in bed as they lie upon their back.

In the complete fracture within the capsule, the limb looks like a paralysed limb, flabby, powerless, and immoveable, with its whole expression altered.—*Lancet, Aug.* 17, 1867, *p.* 187.

47.—REPORT ON THE TREATMENT OF UNUNITED FRACTURES

[Dr. BIGELOW, Professor of Surgery at Harvard University, has been extremely successful in the treatment of ununited fractures. The following is a description of his usual mode of operating.]

The extremities of a false joint should be attacked where they approach nearest to the surface, unless vascular or nervous trunks are in the way; for example, in the arm, upon the outside; a free incision being contrived in each case with especial reference to the ready exit of pus. In the arm, the musculo-spiral nerve, which is often displaced and tied by the lymph, is to be carefully looked for and avoided, and were it not for the care here requisite the bone might be exposed by a single incision.

The principal bony extremities being found, the interval, which is sometimes quite irregular and interlocked, is gradually divided, and the ends turned out, the dissection being materially aided by an assistant powerfully flexing the false joint. Care must be taken to prevent the muscles being stripped from the periosteum. When one extremity is fairly exposed, a crucial or other regular incision is made in the ragged callus overlying the periosteum at its tip, which should be then seized by strong-toothed forceps, and efforts made to tear it out of the rugous inequalities of the formerly inflamed bone. As the flaps begin to yield, the terminal adhesions are detached, and the sound bony shaft is reached, the periosteum is easily stripped from the bone, requiring great care lest the shaft be denuded higher than the intended section.

The soft tissues being now protected by spatulæ, or flexible strips of copper, the end of the shafts is removed by a common saw, the length of this fragment being determined by the amount of periosteum, it has been necessary to detach. Half an inch of good cylindrical periosteum, with half an inch more of ragged tissue hanging at its extremity, has usually covered from three quarters to an inch and a half of bone. Perhaps half an inch of sound shaft, with an irregular or conical extremity varying in extent, is a good rule for the excised piece in most cases. The other extremity is now to be turned out and treated in the same way, and this terminates the dissection.

There now only remains the wire to be inserted. For this purpose holes are bored in each extremity with a good bone drill, larger than the wire, at a little more than half an inch from the end and through one wall only. A pure silver or plated copper wire is inserted from without inward in one end, and inversely entered in the medulla from within outward in the other. The ends are now brought together accurately, and the wire twisted long enough to protrude at the external wound. It is now only necessary to bring the edges of the wound together, leaving an aperture large enough to allow of the free escape of pus, and then to apply a suitable apparatus for retaining the proper position of the parts.

The apparatus for the humerus used by Professor Bigelow, consists of a firm concave splint of iron and leather, made to fit the top and outside of the shoulder as low as the axilla, and thence horizontally to the neck. This is

secured by a strap around the opposite axilla. A similar gutter receives the elbow and fore-arm flexed at a right angle; the two are united by a narrow iron strap on the back, and another on the front of the humerus, adjustable as to its length. The splint can be thus shortened when in place, so as to keep the extremities of the bone in contact, and nearly immovable, in spite of the great leverage of the arm upon the wire, while the dressing can be readily applied in the open interval, without disturbing the apparatus.

For the thigh, a pasteboard splint is moulded to the anterior aspect of the thigh and leg, and then stiffened with dextrine, an interval for the wound being left. The whole limb is then secured to this by bandage; and surmounting the whole, a Smith's anterior splint is applied, by which the leg is suspended from a railway on a framework over the bed.

Professor Bigelow employs water-dressings at first, and poultices or oakum to absorb the discharge afterwards. He keeps the patient in bed for several weeks, in fact till some stiffening has taken place, after which he prescribes fresh air as an invigorating and osteoplastic agent, together with a diet as generous as the appetite will bear, and not unfrequently the use of phosphates.

The wire is allowed to remain in place until the bone is firmly united, generally during several months; in no case has evidence of any ill effects from its presence been observed either in producing necrosis or undue inflammation. In some cases it has remained quietly in its place after the arm was in use. In one instance it remained for two years.

To remove the wire, the loop was divided with cutting pliers, and forcibly drawn out; hence an advantage in flexible wire. This loop is sometimes quite superficial, but in other cases is so deep as to require an incision to reach it.

It has been observed that a partial stiffening, dependent on the inflammation of the soft parts, sometimes takes place in a few weeks, but the bone afterwards becomes gradually loose if the periosteum fails to do its duty.

The one great point in treatment, on which Professor Bigelow has uniformly insisted in his lectures, is the prevention of abscess, or, in other words, the early and free evacuation of imprisoned pus, by large and dependent incisions, which here, as elsewhere, are less injurious to the tissues than the burrowing of pus. Again, the formation of an abscess is always attended with a certain amount of fever, and the consequent loss of appetite and debility may materially interfere with the reparative process.

From this circumstance, the first signs of inflammation should be vigilantly watched for, and an opening should in all cases be made as soon as any evident pointing occurs.

The most favourable cases for the operation are those in which the patient is in vigorous health, and the bony extremities are of the natural size. In chronic cases the ends of the bone are often tapered from a sort of local atrophy. On this account the incisions may have to be longer, and the whole operation more formidable.

Softening of the bone from interstitial absorption may be looked upon as contraindicating the operation, since we find that the only case in which Prof. Bigelow has completely failed, is one in which that unfavourable condition existed, and could not be removed by any treatment, including, of course, the free use of phosphates. There is one accident incidental to some of these operations which occasionally supervenes and demands a moment's consideration. We allude to the supervention of paralysis from division of, or injury to, the musculo-spiral nerve. The paralysis will, of course, be more or less complete according to the extent to which the fibres of the nerve have been severed. As the nerve winds around the outer side of the arm it is difficult always to avoid it, and this difficulty is increased in those cases in which deformity has to some extent displaced it. Masses of indurated lymph may also enclose the nerve. It may also be inadvertently injured where so much congestion exists that the wound cannot be kept dry during the dissection. It has further been found at times concealed in a groove *of new bone, in separating* which it must be divided. This accident has

occurred to Professor Bigelow twice. In both cases the power of the paralysed muscles was ultimately restored. It is but fair to remark that the same accident may equally occur in the course of the other operations that have been recommended for the cure of this lesion. In cases of gunshot wound so much damage is often done to the bone that necrosis is almost inevitable. In reference to this we may remark that Professor Bigelow seems to consider his operation as almost contraindicated where there is complete necrosis, and most surgeons will probably consider other methods of treatment more appropriate to such cases.—*Medical Press and Circular, August* 21, 1867, *p.* 175.

48.—ON EXCISION OF THE ANKLE JOINT.

By Henry Hancock, Esq., Surgeon to Charing Cross Hospital.

[Resection of almost every joint except the ankle has been carried into practice, yet there is no reason why so valuable a member as the foot should be sacrificed, as it is when Syme's or Pirogoff's operations are performed, when simple excision would suffice.]

I have now performed this operation five times—four times successfully, once unsuccessfully, the patient dying some six months after the operation from lung disease, the result of a life of dissipation. In no instance has there been sloughing. There need not be a single tendon or artery divided. There is afterwards very little deformity—comparatively little shortening; the foot is preserved, and, as we shall see, the patients are able to walk and run with perfect ease. Surely an operation presenting so many advantages should not lightly be cast aside nor condemned by theoretical objections without a trial. But so little consideration has there been given to the matter, that in England, even in these days of conservative or pathological surgery, as many as thirty-four legs were amputated in one year for disease reported to have been restricted to the ankle joint, without even the improvements afforded by Syme's or Pirogoff's methods having ever been attempted. Very exaggerated notions appear to prevail with reference to the difficulties attending the performance of this operation.

Mr. Furneaux Jordan, of Birmingham, whose opinions merit our greatest respect and attention, has kindly favoured me with the following observations:—"A very strong objection to the excision of the ankle is this, that the astragalus is not a long bone with an epiphysis in which the chief disease may lie, but a short bone consisting of a mass of cancellous tissue, throughout which the disease is more or less diffused. And as in the ankle disease only rarely extends from the synovial membrane to the bone, the diseases here, as in all allied cases (the extremities of long bones for instance), begins in the centre of cancellous tissue, and consequently can only be removed with the whole bone. I have seen two cases only; both confirmed the conclusions here arrived at. The pathological inferences are the result of a great number of examinations of cases in the ankle and foot. Of course, I infer that when disease begins in the extremities of long bones excision is inadmissible."

The histories given by those labouring under disease of the ankle-joint would lead us to infer that disease commences much more frequently in the synovial membrane than is here allowed by Mr. Jordan; and if we turn to the writings of Mr. Syme, we find that eminent surgeon dwelling upon the frequency of the disease originating in the joint between the calcis and astragalus as an argument in favor of his operations. We do not for one moment deny that in patients of strumous habits the disease, for the most part, begins in the cancellated tissue either of the astragalus or of the ends of the long bones, or of both. What I would urge is, that it is not by any means an invariable rule that persons suffering from ankle-joint disease are scrofulous, and that when this condition does obtain it is no argument against the operation of "excision of the ankle-joint."

Granted that disease commencing in the centre of the cancellous tissue of

the astragalus necessitates the removal of the entire bone, is this a sufficient reason for sacrificing the whole foot? I have, as you will find in one of my cases, removed not only the entire astragalus, but a considerable portion of the os calcis also, and still the patient recovered completely. Nor am I disposed to admit the inference that when disease begins in the extremities of the long bones excision is inadmissible. It is not so admitted in excisions of the knee-joint; why should it be so fatal an objection to excision of the ankle? We have here the epiphysis of the tibia, which frequently sets limits to the disease; and the increase in the amount of bone to be removed is a matter of small importance indeed, when compared with the benefit conferred upon the patient by the preservation of the foot.

With regard to the mode of performing excision of the ankle-joint, I would offer a few suggestions which appear to me of practical importance.

Some surgeons make their incisions through the skin according to the plan proposed by the elder Moreau—namely, a longitudinal incision behind each malleolus, with a short horizontal one connected with it at right angles below the process. The late Dr. Buchanan, again, limited his incisions to one extending from the peroneus tertius to the tendo Achillis, whilst he attacked the joint entirely from its outer side. I have never seen the operation in this way, but I have seen it done according to Moreau's plan, and I infinitely prefer the one semilunar incision extending across the front of the joint to behind the malleoli on either side, penetrating only to the fascia below, as in my own proceeding. When the flap thus made is reflected we have plenty of room wherein to manipulate; we can see the condition of parts and what we are doing; we can detach the tendons sufficiently from their grooves behind the malleoli to do away with the necessity of cutting them across; we greatly facilitate the eversion of the foot; and, as we can see what we are about, we are able to avoid wounding the posterior tibial artery—a point of vital importance to the success of the proceeding, since it mainly depends on the anterior and posterior tibial arteries being preserved in their integrity. The advantage of this method, moreover, is greatly enhanced when, from long-continued disease, the soft parts round the joint are rendered thick and unyielding.

Again, if we meddle with an ankle-joint at all, no matter how limited the disease may seem to be, it is better to excise the entire joint than to leave any portion behind. It is true that I shall have presently to relate a most successful case of Mr. Wood's, which seems to controvert this doctrine; but this case is so exceptional in its results that it proves the general rule and the soundness of the proposition. It is not always easy to judge from the appearance of the surface to what extent the mischief in the cancellated structure has proceeded; and however carefully and completely we may endeavour to gouge the disease out, we rarely succeed so entirely that some portion is not left behind. In partial excision we expose our patients to the dangers attending wounds of joints wherein so much mischief occurs from the impediments to the free exit of matter; in complete excision, on the contrary, we free the entire joint, remove these impediments, and get rid of the joint structure—bone is brought in direct contact with bone, and the process of cure is rendered more certain and more simple. These observations apply with equal force to those cases in which one or both of the malleoli have been allowed to remain.

Mr. Statham excised the astragalus. A year afterwards he removed the two malleoli. The parts never healed, and the man suffered so much that at his own request Mr. Bowman amputated the leg three years after the first operation.

Mr. Price gouged away the astragalus and horizontal surface of the tibia, and left the malleoli. Eight months after he was obliged to operate a second time.

In another case Mr. Price gouged away the astragalus and articular end of the tibia, the posterior end of the cuboid, and a portion of the fibula. Three months afterwards he was obliged to amputate the leg in consequence of *abscess*.

In June, 1852, Mr. Solly gouged away the external malleolus, a portion of

the astragalus, and the horizontal articulating surface of the tibia of the left foot. On July the 2nd he performed a similar operation on the right foot of the same patient. On October 2nd he again operated on the left foot, and in April, 1853, he again operated on the right. The patient, however, ultimately recovered. On June 18th, 1852, Mr. Solly gouged away carious bone from the end of the tibia and from the astragalus. On July 10th he was obliged to amputate the leg.

In Dr. Buchanan's case the external malleolus and astragalus were removed, but the articular surface of the tibia was "scooped out with a gouge, and the internal malleolus left." The patient died of phthisis.

The younger Moreau in 1796 gouged away from a youth, aged 17, the astragalus, the internal malleolus, and about an inch and a half of the lower end of the tibia. "Above this," he says, "the solid part of the bone was sound, but not so the cancelli, which were so much diseased that I was obliged to introduce the gouge, and take away about two inches of them. The result was not so favourable as was expected. The wounds were a long time in healing; the fibula, having no assistance from the tibia in sustaining the weight which it was obliged to support, sank down upon the external side of the foot, which was thrown inwards, so that the lad rests now on the outside of his foot; that, however, does not prevent him from walking. I allowed the inferior extremity of the fibula to remain, thinking that Nature, by reproducing that portion of the tibia which I took away, would finally give by both bones a solid support to the patient; but the event was not what I expected. In a similar case now I would cut off as much from the fibula as from the tibia."

Mr. Hussey also, as we have seen, sawed through the fibula and removed the external malleolus, but he gouged away the horizontal portion of the tibia and upper surface of the astragalus, leaving the internal malleolus. Six months afterwards he was obliged to amputate the leg.

The cases here quoted appear to me very conclusively to prove my proposition, that if we meddle with the ankle-joint at all we should remove it entire, and not a portion only of the joint; as, considering the comparatively few times that these operations have been performed, the results have been most grievous when compared with those wherein the opposite plan was pursued. And I would add that, where practicable, the bones should always be removed by the saw, and the employment of the gouge avoided as much as possible. With the saw we are enabled to cut through the bone with very little violence to the cut surface, which is smooth, and for the most part retains its vitality. But with the gouge these advantages are lost; we never can tell with any degree of certainty whether some amount of disease may not be left behind; we inflict an amount of violence, bruising, and crushing which the parts, already weakened by the pre-existing mischief, cannot repair; and thus the operation fails. The disease either proceeds unchecked or with increased rapidity, or, if arrested, the parts are too much damaged to take on the process of reparation. When this occurs it is commonly ascribed to extension of the mischief, to want of power in the patient, to the existence of scrofula or some other taint in his system or constitution; but that these supposed causes are not always the true ones is strongly exemplified in the case of the foot amputated by Mr. Hussey. Here the end of the fibula, separated by a clean cut of the saw, was surrounded by a large mass of bone structure, thrown out, there can be but little doubt, from the periosteum and cut end of that bone itself; but the gouged opposing surfaces of the tibia and astragalus, we are told, were rough, without any deposit of new bone and without any appearance of commencing anchylosis. We must therefore admit, in this case at all events, that either the diseased portions of the tibia and astragalus were not removed at the time of the first operation, or that the injury done to the parts was so excessive as to be beyond their power of reparation. The result cannot be charged against the weakness of the patient, or the presence of scrofula or some other taint in his constitution. Had either of these latter reasons obtained, it would have affected the fibula equally with the tibia and astragalus; and we should scarcely have had an entire

absence of all effort at reparation in the latter, whilst in the former the process not only of recovery but of reproduction progressed so vigorously and abundantly.

I have very little doubt that when this operation is better understood, and the cases to which it is applied are more carefully selected—that when the vast benefits it is capable of affording to the patient are more extensively known and appreciated, the prejudice with which it has hitherto been regarded will fade away, and that it will be more generally adopted, and take its proper position in the rank of the standard operations in surgery.

We have seen that of the seven cases which terminated fatally, four died of consumption, and one of syphilis. Such cases can scarcely be adduced as evidence against this proceeding, as death would have equally followed any other operation to which they had been submitted. Even after the most patient and painful inquiry we cannot always obtain a satisfactory previous history, or one on which reliance can be placed. Patients too frequently deceive the surgeon, or deny and conceal facts of great importance in relation to their ultimate recovery, and it is only when too late to repair or to obviate the mischief that the truth is revealed. This was the case with the patient on whom my friend and colleague, Mr. Barwell, operated for excision of the ankle-joint. All who know Mr. Barwell are aware of the great care and talent which he brings to bear upon the practice of his profession; still, in spite of his inquiries and caution, it was some days after the operation had been performed, and when too late to remedy the mischief, that the patient admitted that which too surely proved her destruction. If therefore there be any trace of syphilis, neither this nor any other operation should be performed. In Mr. Barwell's case the parts bled very freely when first incised, nor did the pressure made by Mr. Hird on the femoral artery produce any appreciable diminution of the hemorrhage; but all the surfaces ceased bleeding after they had been exposed a few minutes to the air. Successive hemorrhages took place from day to day till she died, ten days after the operation, and it was remarked that the blood discharged seemed to be putrefied, and had a most offensive odour. It was not till after her death that the fact of her having suffered from syphilis transpired, as there were no general symptoms of the disease. As bearing on this subject. I would allude to a valuable and interesting case related by Mr. Cæsar Hawkins before the Pathological Society on Oct. 15th, 1850:—

"The joint between the os calcis and astragalus was partially anchylosed, and *although the astragalus itself was quite healthy*, the joint between it and the tibia showed the commencement of ulceration of the cartilage, demonstrating the manner in which the inflammation of the soft parts sometimes conveyed disease from one tarsal or carpal joint to another, and causing the removal of pieces of necrosed bone, or the excision of even an entire bone, to be not always successful." The case was remarkable, however, more from its history, as illustrative of the difficulty occasionally observed in distinguishing between simple scrofulous and malignant affections. The patient, a young woman, was admitted into St. George's Hospital with a large phagedenic ulcer of the inside of the ankle, excessively painful, and requiring large doses of opium for its relief, with much thickening of the soft parts; looking, in the sections, not unlike scirrhus, and having in the centre some warty granulations, resembling the cancer of cicatrices (of which Mr. Hawkins has written a description), in the centre of which the probe passed down into the joint, between the astragalus and os calcis, without touching dead bone. After some attempts to heal these parts, amputation was proposed, but declined by the patient. After a few months a mass of glands appeared low down in the groin, as large as an orange, as if enlarged by the contamination from the apparent cancer below, which did not ulcerate like strumous glands, but sloughed gradually away after previous ulceration of the skin; and, as some were destroyed, fresh masses formed by their side, which went through the same process of sloughing during two or three months. About the same time there formed at the lower part of the femur, on its inside, a *firm elastic tumour, obviously* connected with the periosteum, but without

bony deposit, about four inches in length and perhaps two inches in height. Soon after this a growth took place on the left side of the frontal bone, soft in the centre, and consisting apparently of a morbid growth in the diploe, making its way by pushing out the outer table, with a little absorption of the bone in the centre. Thus was formed a continuation of appearances seeming to confirm the supposition of the malignant nature of the original disease, and quite sufficient to deter Mr. Hawkins from operating, for which the woman was now desirous. In two or three months, however, and, as it seemed, chiefly from the use of iodide of potassium and sarsaparilla, all these latter growths disappeared; the tumour of the thigh gradually subsided, the glandular masses all sloughed out, and the part healed; the growth of the diploe disappeared, the bone recovered its former level, and the skin lost its former sallow hue. Amputation was performed, and the patient was quite well a year and a half afterwards.

Although the above case is described as one of strumous disease of the os calcis, femur, and cranium, resembling malignant disease, it seems to me to have been neither the one nor the other, but a very severe case of syphilitic mischief presenting its local manifestation at the various parts indicated; and I am led to this conclusion by a somewhat similar case about which I was consulted some six years ago.

I was requested to see a gentleman residing in the neighbourhood of Blackheath, said to be suffering from malignant disease of the ankle-joint, especially with reference to performing amputation of the limb. He had been suffering for four months, was extremely irritable, anxious, and emaciated; and described his sufferings as almost unbearable, entirely depriving him of rest, so that, as he said, he was almost worn out. He had no appetite; his countenance was leaden and sallow; and his pulse was extremely rapid, and at the same time feeble. The ankle was much swollen, as was also the foot. Over the inner ankle was a large uneven ulcer, presenting all the appearance of a malignant sore, the surrounding skin being of a bluish-brown red, and the discharge thin and very offensive. He described the pain as a dull, burning, gnawing pain, implicating the bone. But I was much struck with the manner in which he dwelt upon its increasing intensity towards night, adding that it always subsided towards the morning. He denied having ever suffered from syphilis, although he admitted having had a slight excoriation about twelve months before, of which he had not taken any notice beyond touching it with caustic. Under these circumstances I suggested the probability that the disease, after all, was syphilitic rather than malignant; and that it was advisable to submit him to a regular course of treatment for the former disease before having recourse to operation. I accordingly ordered the iodide of potassium with sarsaparilla, and five grains of blue pill every night; and the black wash to be applied to the wound. This gradually healed under the treatment, and the patient recovered completely. He is now walking about as well as ever he did.

Lastly, as regards excision of the ankle-joint, I would lay great stress upon the necessity of ensuring a free exit for the matter; and this is an additional reason for sawing away the whole joint rather than gouging away a portion only. By the former method we ensure a clean, smooth plane across the joint, clear of those inequalities which can scarcely be avoided when the gouge is employed, but which serve as so many barriers behind which the discharge is prone to collect, causing irritation, increasing the discharge, and delaying the cure. It is here that one of the benefits of making the flap, as practised in my operation, is exemplified. By its looseness and freedom we are enabled to ensure the wound behind the ankles being kept open, which we cannot do when the right-angled incisions of Moreau are only employed. These, when the bones are removed, do not correspond to the resulting cavity in the ankle, whilst the flap thus made, free only in a portion of its extent, unable consequently to yield to the swelling and tension of the secondary inflammation, forms a valve, as it were, closing the external outlet, and deprives the patient of one of the principal sources of cure. Whilst, therefore, the wound in

front of the joint may be closed and secured by wire sutures, the portion of the wound over the sides of the foot should be left entirely free.

The following case, for which I am indebted to Mr. Tudor, of Dorchester, is a good illustration of the value of preserving these free openings :—

H. K., aged 23, a Hanoverian and a sailor, was admitted on board the *Dreadnought* for the second time on the 13th of April, 1859, with disease of the ankle of two and a half years' standing. The previous history, in March, 1858, when first admitted, was an injury to the foot, which was jammed between two ships. He continued to work upon it for about a year, when he first felt painful pricking sensations in front of the joint, accompanied with swelling. He was treated for eight months in the British hospital at Buenos Ayres without benefit; was sent home to England, and admitted on board the *Dreadnought*. The ankle was then very much swollen and painful, but there was no wound nor sinus. He remained in the hospital four months, and was discharged much better, but still unable to use the limb. He was re-admitted on board the *Dreadnought* on the 13th of April, 1859, when his ankle was found swollen and puffy, with fluctuation, and a distinct grating could be felt between the astragalus and tibia. There were now two sinuses discharging pus, one situated in front of the joint, a little external to the mesial line, through which a small patch of denuded bone could be felt on the upper surface of the astragalus, whilst the second, situated midway between the tendo Achillis and external malleolus, impinged upon the same point from behind.

June 9th. Under chloroform an incision was made in front of the joint from the external border of the extensor communis in a curved line, about an inch behind the external malleolus. About an inch of this bone was sawn off, and the ligaments, as well as the tendons of the peronei muscles, were divided. The external and anterior surfaces of the astragalus were found denuded of cartilage, and the bone diseased. About three-fifths of the bone were removed by saw and gouge, and the parts quietly brought together with sutures.

11th. The wound looking red and congested, with a tendency to slough, the sutures were removed, and free openings made for the discharge.

Healthy inflammation soon set up, and the man's health quickly improved, and he subsequently left the hospital with every promise of a useful joint.

Although as a general rule I strongly advocate the removal of the entire joint in preference to a portion thereof, I am free to admit that exceptional cases have occurred where "partial excision of the ankle-joint" has been attended with success. The case—the most worthy of record with which I am acquainted—is the following, operated upon by Mr. Wood, at King's College Hospital :—

A single woman, aged 25, was admitted June 15th, 1858. Ten years previously she slipped and sprained her right ankle very severely, for which she was an inmate of Guy's Hospital for three months. The joint became stiff, and she could not walk upon it for twelve months. She was then for three years under the care of the late Mr. Lonsdale at the Orthopœdic Hospital, during which time an abscess opened in front of the internal malleolus. This never healed. Some time after this she received a kick on the heel of the affected foot, which caused great pain and swelling.

When admitted there was considerable swelling of right ankle; the foot was slightly movable, but the attempt to move it gave her extreme pain. Three sinuses, left by abscesses, communicated with diseased bone, and passed upwards to the tibial portion of the joint, the largest being in front of the inner malleolus, a second, placed posteriorly, passed between the tendo Achillis and posterior tibial vessels and nerve, whilst the third, placed behind the outer malleolus, was directed inwards and forwards towards the tibia. An oily fluid appeared occasionally with the matter upon the dressings.

On July 27th the patient was placed under chloroform. Mr. Wood made *an incision* two inches long over the inner malleolus, and by gouge and forceps *removed the* whole of the diseased inner malleolus, exposing a piece of

necrosed bone of the size of a chestnut, which included the greater portion of the tibial articular surface, completely denuded of cartilage, which latter lay detached in the cavity of the joint. This was removed, with a portion of gelatinous synovial membrane. The rough articular surface of the astragalus being removed with bone forceps, the bone was found to be quite healthy.

The patient rapidly recovered. The foot had a great tendency to turn inwards, from the lack of support caused by the removal of the inner malleolus, whilst the fibula remained in its place.

On Nov. 23rd, 1859, Mr. Wood again saw this patient. She had become fat, and said she had never felt better in her life. All the sinuses had quite healed. The leg is about three-quarters of an inch shorter than the other. She walks well and firmly, and with the slightest possible limp. The ankle is quite firm and stiff. A deep hollow marks where the malleolus formerly existed, and the inner arch of the foot is somewhat elevated and shortened. She says she can walk six miles without feeling tired, and that she never has pain in the ankle-joint.

Mr. Wood may well be congratulated on the result of this case, which is almost unique, and unusually successful. Nor must I omit to mention a somewhat similar case operated upon by Mr. Moore, at the Middlesex Hospital, on the 2nd of March, 1855.

A pale thin clerk, aged 21, had disease of the ankle early in life, but the foot, though stiff, had been useful and free from active mischief for the last eight years. Eight weeks before an abscess formed, and he was admitted into the Middlesex Hospital, under the care of Mr. Moore, with three sinuses inside the ankle. There was much swelling over the lower part of the tibia and inner side of the joint, a little in front and behind, and none outside. The foot was extended and fixed. After the failure of treatment, Mr. Moore made an angular incision down the back of the tibia, along the lower edge of the inner malleolus, and across the front of the joint, through the tendons and anterior tibial artery. The joint was thus exposed from the tendon of the tibialis posticus to that of the extensor longus digitorum, which were not divided. On removing the internal malleolus, the diseased surfaces of the tibia and astragalus were exposed, and they were each sliced off quite smoothly, in a single cut, with sharp and large cutting pliers. The fibular part of the articulation was not interfered with, as it was quite fixed, and the whole of the diseased parts appeared to be removed. The result was that a gap was left between the end of the tibia and the top of the astragalus. There was little constitutional or local irritation, and the cut surfaces of bone granulated healthily. A fortnight after the operation there was some redness and tenderness over the external malleolus, with pain, which kept him awake one night. This all subsided. In another fortnight a bare piece of bone was felt on probing to the depth of the inner surface of the fibula. A month afterwards it appeared to have separated and been discharged, for the patient spake of a pricking sensation which crossed the joint inwards, and on further probing no bare bone could be felt. By the end of May there was scarcely any discharge, and he soon after went into the country, his ankle feeling strong, though the sinuses were not quite closed, and his health being good.—*Lancet, June* 15 *and July* 27, 1857, *pp.* 729, 91.

49.—ON CICATRICES ADHERENT TO BONE AFTER AMPUTATIONS.

By H. Hancock, Esq., Surgeon to Charing Cross Hospital.

[The two following cases show how important it is to examine whether a cicatrix adheres to the bone underneath, so as to cause the excessive pain which we find in some stumps.]

The first instance was that of a lady between 50 and 60 years of age, to whom I was called in the year 1857, by my friend, Mr. Chapman, of Hounslow.

Mrs. B , when 30 years of age, suffered from suppression of the catamenia, for which she was on several occasions bled in the leg. After the last bleeding, pain occurred in the spot. The pain continued with increasing severity for three years, during which period she was seen by most of the first surgeons of the day. She next consulted the late Mr. Liston, who at once excised the painful spot. The wound healed, and she remained free from pain for several years, when it returned precisely in the same spot, and continued for several weeks very severe, and not relieved by treatment, although there was neither redness nor swelling. Another surgeon of great eminence was then consulted; he proposed to remove the cicatrix, and this was done with relief, which lasted sixteen months, when the pain returned. The cicatrix was again removed, but at the end of six months she was as bad as ever. Her sufferings at this time were so great that she was willing to undergo any operation which held out the probability of effecting a cure. She appeared quite worn out with pain and want of rest. Upon hearing the history of her case I was struck with the fact, that after the last two excisions she remained free from pain so long only as the resulting wounds were open and unhealed, but that directly the cicatrix was completed the pain returned; and when, upon examination of the part, I found the cicatrix was adherent to the periosteum, and perfectly immovable. I concluded that her sufferings were due to this cause, and that they would not be alleviated so long as the parts remained in their then condition. I therefore proposed that the skin should be detached from the subjacent periosteum by subcutaneous division; that reunion should be prevented by moving the skin backwards and forwards from day to day, so that the new skin of the cicatrix should be placed in the same position as that of the surrounding integument. This having been agreed to, on the 15th Sept., 1857, I detached the cicatrix from the bone, the part cut through being cartilaginous, and so hard as to require a sawing motion of the knife. Mr. Chapman, who attended the case subsequently, and carefully prevented readhesion, informed me, in 1859, that the patient was then in good health, and had remained perfectly free from pain during the two years since the operation.

The next case was that of a Mrs. H., aged 30, admitted into Charing-cross Hospital on Nov. 30th, 1858. She had disease of the left knee-joint at ten years of age. At fourteen, the knee being much swollen and very painful, was punctured, and a considerable quantity of blood escaped, but no matter. At sixteen the catamenia first appeared; they left her for two years, and then returned with irregularity. At seventeen years of age she fell, and so injured her knee that she went into the Royal Free Hospital, where the leg was amputated. The stump healed rapidly, but accidentally falling on the floor, she hurt the stump so much that it reopened, and the bone protruded through the wound, preventing its healing. The pain then became so intense that subsequently two inches of the bone were removed. After this she recovered, and remained well until the year 1854, when she felt as though the limb was entire—as if the blood was rushing to every part below the amputation, accompanied by great pain in the nerves. The pain gradually increased until it became so severe and unbearable that she begged me to amputate the leg higher up. Upon her admission (Nov. 1858) I found that the cicatrix at one point was tied down to the end of the bone by a dense band about three-quarters of an inch long, and that any pressure upon this part increased her sufferings to a great degree. The end of the nerve, enlarged into a considerable bulb, could easily be distinguished attached by this band to the bone also. I had in other cases dissected out these bulbs, but with so little success that I was led to doubt very much whether the suffering was so dependent upon this cause as is usually supposed; whilst the result of the case just related induced me to hope that were the cicatrix released from the bone so as to permit of free movement, the patient would be relieved from her sufferings and spared a second amputation. I accordingly detached the cicatrix from the bone by a subcutaneous incision; the connecting tissue being dense, like cartilage. The soft parts were moved *gently over the bone* for a short time every day until the wound was healed

and all trace of tenderness had ceased. The result was most satisfactory, and she left the hospital perfectly cured two months afterwards.

The alteration in the stump was certainly surprising. Prior to the operation the bone covered by the cicatrix projected nearly half an inch, the surrounding skin being flabby and puckered-in around it, and apparently devoid of fat; in point of fact, a conical stump was in process of formation. But when the patient left the hospital all signs of this misfortune had vanished, and the integuments appeared to have again become furnished with fat as in other parts of the body. The face of the stump became movable, soft, and full, and so thoroughly covered the end of the bone as not only to afford a good cushion, but entirely to dissipate all anxiety as to its projection. In proposing this subcutaneous section of the cicatrix I was prepared for the subsidence of pain, but I certainly did not anticipate the great improvement in the development and shape of the stump; and it may become a question of some interest, and one worthy of consideration, how far the adhesion of the cicatrix, and consequent drawing-up of the neighbouring skin to the bone and periosteum, may conduce to the formation of a conical stump, so usually attributed to insufficiency of soft parts and undue muscular retraction.—*Lancet, Sept. 7, 1867, p. 288.*

50.—SUB-PERIOSTEAL RESECTION OF THE OS CALCIS.

By Thomas Annandale, Esq., F.R.S.E., Lecturer on Surgery, Assistant-Surgeon, Royal Infirmary, Edinburgh.

The following case is an illustration of the advantages of saving the periosteum in the re-section of bones.

The method adopted in this case of separating the periosteum and soft parts together, appears to me to be the most useful in such operations. (1st) Because the bone to be removed can in this way be thoroughly exposed; (2nd) Because the blood vessels of the reflected periosteum, and flap or flaps of soft textures, are not in any way injured, except along the line of the incisions.

For these reasons, I prefer this plan of operating to that in which the diseased bone is scraped away or gouged from the periosteum.

In separating the periosteum from the bone, it is of great consequence to save as much as possible of its inner layer, as the experiments and researches of Ollier have shown that this is the chief bone-producing portion.

During the temporary absence of Dr. Gillespie in June, 1865, J. F., aged 17, came under my care in the Edinburgh Royal Infirmary. The patient, a well-grown lad for his age, had suffered from a swelling of his heel for about a year before his admission, suppuration had taken place, and a sinus had in consequence formed over the back part of the os calcis.

On admission, the whole heel was swollen, and very tender to the touch, the os calcis felt enlarged, and there was a sinus immediately above the insertion of the tendo Achillis into this bone. On introducing a probe into the sinus it passed into a carious cavity in the substance of the os calcis. The other bones of the foot were quite healthy. Shortly after his admission the patient's sinus was attacked with sloughing phagedena, which was then prevailing in the wards, and this led to a considerable destruction of the soft tissues around. In the beginning of August, the sinus, which had become converted into a sore nearly as large as a five-shilling piece, began to contract, and on the 14th of that month was reduced to the size of a florin.

At this date the disease of the os calcis was in much the same state as when the patient was admitted, and I therefore thought it time to remove the affected bone.

Operation.—An incision was first made along the outer aspect of the bone, extending from the sore behind to the articulation between the os calcis and cuboid in front. Two short incisions were also made at right angles to and at each end of the first incision, the anterior one of which corresponded

to the direction of the joint between the os calcis and cuboid, and the posterior one to the course of the tendo Achillis.

The incisions were made quite down to the bone; and then, with one of Lagenbeck's instruments, I scraped off the periosteum, and separated it and the flap of soft textures together from the entire surface of the bone. By extending the anterior short incision for a little distance into the sole of the foot, the flap was readily turned downwards and inwards, so as to allow the bone to be detached from its different articular and tendinous connexions. Very little bleeding took place; no artery required to be ligatured.

The flap was re-adjusted, and a gutta-percha splint applied to the anterior aspect of the leg and foot, so as to keep the parts at rest. Six or eight hours after the operation a little oozing took place, but was stayed by stuffing the cavity with lint.

The wound suppurated freely after the operation, but the cavity gradually contracted, and, at the end of two months, the parts were soundly healed.

Four months after the operation, the foot had a very natural appearance—and the patient was able to bear a little weight on it.

A mass of very firm tissue had taken the place of the os calcis, and although this tissue was not as yet osseous, I have no doubt, from my examinations of other sub-periosteal resections, that in time a considerable amount of new bone would be produced in this case. Shortly after the patient returned to his home in the far north, so that I have never had an opportunity of verifying this opinion.—*Glasgow Medical Journal, June*, 1867, *p.* 44.

51.—CLINICAL REMARKS ON CASES OF JOINT INFLAMMATION.

By Richard Barwell, Esq., Surgeon to Charing Cross Hospital.

During the month of March there presented themselves at the above hospital an unusually large proportion of acute and sub-acute joint inflammations, the larger number of these being rheumatic and arthritic. In pointing out this fact to his class a few weeks since, Mr. Barwell made the following remarks:—

"The large proportion of synovitic cases that we have seen to-day and for some days past cannot fail to have impressed you; and that peculiarity of the time must, I think, be ascribed to the inclement weather, and especially to damp combined with cold. We had no run upon joint cases during the dry frosts of January; the damp warmth of February brought forth crops of boils and carbuncles; and the cold, raw wet of this month of March has produced a disproportionately large number of rheumatic and rheumatoid affections. I make these remarks because it does not appear to me that sufficient attention has ever been given to the meteorology of disease. No sort of practice gives so extensive a field for observation as a great out-patient department, and I have for years past observed fluctuations in the class of cases to attend diversities of weather.

"But now I wish to call your attention to some peculiarities in my treatment of different cases. You will have observed that after the violence of inflammatory symptoms is subdued and that little pain with a certain amount of swelling is left behind, I order in some, the smaller number of cases, applications of iodine and bicarbonate of potash; but in others, the larger number, I combine in different proportions iodide of potassium and iodide of lead; for I find that of all absorbent applications, the iodide of lead is probably the most potent in cases of strumous and rheumatic thickening, but it must always be used with care, especially as it acts upon the gouty diathesis as a direct poison.

"Dr. Garrod has described, in his work on Gout, 'the influence of lead as a predisposing cause of gout' (p. 281, *et seq.*) I find, however, an important converse to this view—viz., the influence of gout as predisposing to lead *poisoning. The first* notice which I received of this fact was in private

practice. A gentleman for months laid up with gout, and tired of being overdosed with colchicum, sent for me. I ordered, among other things, an application to the foot, consisting of two parts of iodide of potass and one of iodide of lead. Next day—*i. e.*, in twenty-four hours—he had symptoms of lead-poisoning, by no means slight, and the blue gum line well marked. This fortunately gave me a clue in another case I then had in hand, of a gentleman who always fell ill after residing some days in his town house. He was the only one of the family who suffered, These illnesses were all alike, and his stay in town was always terminated or closely followed by a fit of gout. Having been given the strong hint above mentioned, I thought I could trace somewhat obscure symptoms of lead-poisoning, and on examination the water was found to contain lead. Since the cisterns, &c., have been changed, this regular sequence of events has ceased.

"Now you will observe that if any signs of gout are detectable in your patient, you will avoid the use of lead, and you may substitute a drug which in these cases is more useful—viz., the bicarbonate of potash, which has a chemically solvent action on gouty chalk-stone. You may apply the materials in solution. Let lint dipped into the fluid be placed round the joint, and over this oil-silk; or, if your patient be in bed, put a thick layer of wadding over the wetted lint, and confine the whole with tolerable pressure by means of a roller. The heat will add to the rapidity of absorption, and you will find this mode of application very efficient."

52.—NOTES OF A LECTURE ON DISEASES OF THE JOINTS.

By Holmes Coote, Esq., Surgeon to St. Bartholomew's Hospital.

In speaking of inflammatory diseases of the articulations, I wish it to be distinctly understood that there are but two structures which admit of primary inflammation—namely, the synovial membrane and the articular extremity of the head of the bone. Of the former we have a familiar example in the case of hydrops articuli; of the latter, in the case of ulceration of articular cartilage. But this latter term is falling into disuse, for the ulceration of the cartilage depends upon a morbid condition of the cancellous tissue of the bone, and is consequently a secondary affection. Hence this group of affections is divided into "synovitis" and "disease of the joint;" the former for the most part transitory and easily managed, the latter more obstinate and attended with severe pain. It may be readily surmised how tardy is convalescence in the second instance, for the part of the bone intended to support the weight of the body is no longer in a condition to perform that function; it is painful and tender, and between its free surface and the cartilage now loosened there is a layer of exquisitely sensitive granulations. Hence the limb almost insensibly assumes that position in which there is the least amount of pressure between the opposed surfaces. In the case of the knee the joint is always flexed. In the further progress of such a case we find the ligaments softened by secondary inflammation, and they become elongated, so as to admit of displacement of the bones. During the acute stage any attempt at extension adds greatly to the patient's sufferings, and cannot, indeed, be borne.

The disease may be said to go on until the bone has recovered its healthy state, but it unfortunately often happens that before that time has arrived the articular cartilage has been in great part, if not wholly, destroyed; and then we meet with those cases, so common in the hospital, where the patient recovers with a bent and deformed limb.

In former times a bent limb was often amputated as a useless and inconvenient appendage, but we have now two methods of correcting the deformity; one by forcible extension, the patient being rendered insensible by the use of chloroform; the second a painless proceeding by means of gradual extension, and although cases occur in which one proceeding may be demanded in preference to the other, yet I prefer, as a rule, the gradual extension. You

cannot form any estimate of the amount of laceration of internal parts when force is used, and structures may be damaged which you wish to preserve.

Those cases which do not yield to forcible extension are cases of bony anchylosis—a somewhat rare occurrence—and you may generally ascertain whether the obstacle be bony or not by pulling at the limb. In the case of osseous anchylosis, the parts being immovably fixed, there is no muscular contraction in the muscular structures around. In the case of fibrous anchylosis, the more common form, there is some amount of yielding, and the surrounding tendons are felt to spring from their sheaths on the application of extending force.

In the event of the tendons opposing any serious amount of resistance, they may be readily divided subcutaneously; but this is a measure rarely necessary in the young.

You are aware that I am no advocate for the resection of diseased joints. The successful results which are brought about by soothing remedies, rest, and by patience, have been amply illustrated by cases which you may see in the hospital.

We cannot always arrest morbid action; but we may keep the limb in that position which is easiest to the patient, and thus favour recovery. The restoration of the limb to its proper form is a matter of later consideration.—*Medical Press and Circular, April* 17, 1867, *p.* 358.

53.—TREATMENT OF DISEASED JOINTS BY ESCHAROTICS.

By Dr. Frederick Kirkpatrick.

[In the following paper, which was read at the last Annual Meeting of the British Medical Association, held in Dublin, the treatment recommended in cases of diseased bone and joints is incision, and the deep introduction of caustic, (the potassa c. calce,) into the cancellated structure of the articulating extremity of the bone in the incipient stage, or that of inflammatory congestion, and into the joint itself in the very advanced periods.]

The statistics of conservatory surgery, more particularly of the great revived operation of excision of the knee-joint, are the reverse of satisfactory; the high expectations formed from the successful cases have not been fulfilled by a sufficient percentage of cures, and in many instances the reported cures have not been permanent.

In the last work on joint diseases, published this year by Mr. Holmes Coote, of Bartholomew's Hospital, the following conclusions are arrived at, based on the statistics of Dr. Hodges, of Boston:—"Considering the mortality after the operation, excision for hip disease does not merit a very favourable verdict. Excision at the knee, although occasionally yielding brilliant results, is an operation to be practised with great reserve. Excisions at the wrist-joint being followed by a large proportion of failures, and when successful, the usefulness of the hand being so limited, are operations not sanctioned by sound judgment or conservative surgery. Operations on the foot for strumous disease, usually yield unsatisfactory results."

Dr. Hodges, of Boston, in his "Essay on the Excision of Joints," observes:—"Out of a considerable number of cases, one-third died, and more than one-third are known to have failed. I repeat then that a period has arrived when doubt and uncertainty pervade this most important department of surgery, and it has become a question as to whether there was not a greater saving of human life, when early amputation was the rule, than in the interval that has since passed over. It is also especially worthy of remark, that excision has rarely, if ever, been performed in private practice."

At the commencement of his magnificent address at Chester, Mr. Bowman, that great benefactor of his race, called surgery "the hands of God, the human hands." I may be permitted to add, hands never to be put forth to *the execution* of any operation, but when their possessor can say he would *himself consent to its* performance were he the patient. I believe that the

rigid application of this golden rule in surgical ethics, would very much limit the future performance of operations for excisions, save only at the elbow-joint.

In the surgical charge of the North Dublin Union, the largest chronic hospital with one exception in this country, I have had, during the last twenty-five years, ample opportunity of judging of the effects of rest in the treatment of those affections, and the result of my experience has been most unfavourable.

A certain amount of success may be looked for in the treatment of the upper classes, where the purest air, the best nourishment, the most approved mechanical appliances, together with the means of easy locomotion, can be commanded; but with the lower classes, I have found that the treatment by rest has been a history of failure; disease spreading from bone to bone in the smaller articulations, and before the consolidation of a large joint could be completed, organic disease having, in general, invaded the liver, or some of the other internal organs.

This unsuccessfulness forced me some years ago into the discovery of this treatment by cauterisation which I propose. Having often remarked the healthy reparative action that followed the use of the potassa c. calce in sinuses in the groin, neck, and axilla, I began to introduce it into fistula leading down to diseased bone, at first with caution, then more boldly, and finally disregarding Sir B. Brodie's strong injunctions against letting potassa fusa enter a sinus, I proceeded to carry its action deeply down, converting the small contracted painful orifices into large funnel-shaped openings, and bringing the carious bone into view, and within reach of the further application of the caustic. In this manner, several cases of disease of the carpus and tarsus, and of the flat and superficial bones, were successfully treated, the caustic being reapplied at intervals of a few days, to keep the orifices freely open until the carious bone had disappeared, or was covered over with firm granulations.

In a similar manner, several cases of chronic necrosis were treated, the caustic being very freely used, destroying all foul undermined integument, and leaving, after the removal of the sloughs, large clean circular openings, more than an inch in diameter, and extending deeply down to the sequestrum, into contact with which the caustic, in stick and powder, was freely brought.

In this manner, two cases of necrosis of the fibula, very similar to each other, in which numerous openings led down to diseased bone, and where the patients were reduced to the lowest state by years of suffering, were perfectly cured within six months.

In a case of necrosis of the heel in a delicate lad, who was deformed by the effects of an old hip disease, a caustic perforation was made at each side of the heel, and the powder was brought into contact with the dead bone, until it was so removed that a catheter was passed quite through the heel, no inflammation or constitutional disturbance having been caused or excited.

I can speak with the utmost confidence of the application of this remedy in all such cases of caries affecting the superficial bones.

The caustic perforations may be multiplied in proportion to the extent of the disease, respect being paid to important nerves and vessels, and care being taken not to destroy sound structure, or periosteum beyond the limits of the diseased surface of the bone. Before I speak of the application of this remedy to the arrest of the early stage of joint disease, I may be permitted to allude to the important question as to the structure which is first attacked.

My own opinion accords with those authors who believe that in the great majority of cases the disease commences in the cancellous structure of the heads of the articulating bones. That there are rare cases where it begins in synovial membrane or cartilage, and mixed ones which, seen at a late period, may perplex diagnosis, I fully believe; but I consider that those instances were the fons et origo malli arises in the bone, preponderate so enormously that, as a rule of practice, it cannot be too strongly insisted upon. My own opportunities of obtaining pathological evidence have left

me without a doubt on the subject, and the practice which I propose is based upon that conclusion.

In Sir B. Brodie's great work on the joints, he recognises the fact of the frequent origin of disease in the cancellous structure, and he describes the heads of the bones as distended, with a reddish medullary fluid, then softening of the tissue, and finally suppuration—the matter either forcing through to the cavity of the joint, or reaching the surface at some position more or less remote; but whilst he thus clearly recognises the cause, and graphically describes the disastrous effects, he forbids the remedy, and strongly cautions against an early or premature opening, although he had himself with success trephined the heads and shafts of the long bones in cases of painful and circumscribed abscess.

That he attempted to give relief in a similar manner in those cases of acute articular osteitis, which he so circumstantially describes, is more than probable; and I therefore infer that his caution against the early opening of an inflamed bone, was founded on his experience of ill consequences that followed such a proceeding.

Notwithstanding the teaching of this great authority, I venture to propose interference by operation at the very earliest moment that congestive inflammation of the head of a bone can be fairly diagnosed; and I state with confidence that a perforation made into the cancellous structure, if freely cauterised with the potassa c. calce, will be followed with relief from pain, and that the inflammation which ensues will be only such as is attendant on and accompanies reparative action.

The caustic tunnel may be made at once by cutting down on the bone and piercing the compact tissue with a strong knife, trochar, or small trephine, and then freely cauterising the full extent of the perforation; or, in less acute cases, a small eschar may be first made, the centre of which being incised, the caustic can be introduced, and, by combining its action with the knife, the tunnel can be carried deeper, from day to day in a gradual manner. By means of this combined caustic perforation, I succeeded in arresting disease in its first onset in the head of the radius of a young man, aged twenty-four, in the year 1861. Since that time I have tried it with success in several cases of incipient disease, in carpal, tarsal, and other superficial bones.

I perforated the tibia above the internal malleolus in several instances with curative results, also the great trochanter, in cases where its structure, or that of the head and neck of the femur, was the seat of osteitis, the cavity of the joint being yet unaffected.

On the 26th of last April I exhibited a young man at the Surgical Society of Ireland, into whose trochanter a caustic tunnel had been inserted on the 22nd of March preceding, and which was still freely open. The wasted buttock, obliterated fold of nates, and emaciated state of the entire limb, still existed to proclaim the nature of the disease; but all pain had ceased, the motions of the joint were in a great measure restored, and he walked about before the members of the society without lameness, although for three months previous to the operation his thigh was flexed, he had constant pain in his hip and knee, and he could not admit abduction or any extended motion of the limb. This patient took his discharge to work on the 11th of May, the contour of the limb being almost restored to its natural fulness, and his general health quite recovered.

Although strongly recommending this treatment for the early stages of acute articular osteitis, and also for the very advanced periods when caries is established, or when a whole joint is converted into a foul suppurating cavity, my experience does not warrant me in advising the practice so strongly in the intermediate periods, where the head of a bone may be the seat of a diffused suppuration, possibly communicating with the joint itself. The caustic, I fear, in this condition of parts could not extend sufficiently to *protect* from the constitutional disturbance and risk of pyæmia that might *follow.*

The principal merit I claim for this caustic treatment is, that it is a power-

ful, and, at the same time, the safest means of correcting nature where she is manifestly in error, and of assisting her operations where they are directed right. And here, I have the boldness to start from out the well-worn professional groove of bestowing a blind admiration upon the proceedings of nature, as seen in her efforts to restore and repair diseases of bone. Instead of the vis mediatrix, which is so perfect, and to be relied on in many of our ills, I assert that she exercises in those affections a vis inimica, a vis perniciosa.

From the first onset of inflammation within the cancellous structure, all through the various destructive processes that ensue, she is engaged in hemming up, and confining the engaged bone, and perpetuating its diseased state, and her reparative operations only commence and become effective when, either by man's art or by her own remorseful ulcerations, this imprisonment at the surface is interfered with, and in a greater or less degree removed.

The highest art then of the surgeon is demanded to counteract and remove her mischievous operations from without, and to hasten, open up, and make way for her all powerful reparative actions from within. I am fully convinced that this interference can be more safely and effectually attempted by the combined action of the knife with cauterisation, than by the knife alone, and I commend the treatment to my professional brethren with the greatest confidence, earnestly hoping that they will give it a fair trial, and that it may be attended by the same measure of success in their practice that it has been in my own.—*Medical Press and Circular, Aug.* 21, 1867, *p.* 172.

54.—POWER OF ABSORPTION OF WOUNDS AND ABSCESSES.

M. Demarquay has submitted to the Academy of Medicine of Paris a series of experiments on this subject. He placed upon wounds and into accidental cavities aqueous solutions of iodide of potassium of the strength of 10 per cent. After from six to thirty minutes iodine was found in the urine, and especially in the saliva. The author considers that ulcers and abscesses absorb also noxious gases both from the atmosphere and those formed by the decomposition of blood and pus. He, therefore, advocates the protection of wounds and abscesses from the surrounding air, dressing them with glycerine, alcohol, or disinfectants; and surrounding patients with as pure an atmosphere as possible.—*Lancet, Sept.* 21, 1867, *p.* 360.

55.—NEW MODE OF TREATING COMPLICATED FRACTURE OF THE LOWER JAW.

Case under the care of C. G. Wheelhouse, Esq., at the Leeds General Infirmary.

[In the following case some special mode of treatment was evidently called for. There was a severe contused and lacerated wound of the left cheek, communicating directly with a fracture of the lower jaw, and extending upwards on the face for two inches and a half or three inches. The jaw had sustained three fractures: one at the symphysis, a second immediately in front of the insertion of the masseter muscle, and a third through the ramus of the bone and base of the coronoid process.]

Two silver pins were made, with flat, circular, and *perforated* heads, each pin being about an inch and a quarter in length. Two holes were bored with an Archimidean drill through the substance of the jaw-bone—one between the roots of the incisor and canine teeth of the unbroken side, and the second between the roots of the same teeth of the fractured side. Through these holes the two pins were passed *from behind forward*, the perforated heads, threaded with a good stout silk ligature, resting upon the floor of the mouth under cover of the frænum of the tongue. Having been well thrust forward

through the drill-holes, the points were bent in opposite directions, the loose fragment was placed in good position, the ligature was brought forward over the teeth, and a figure-of-8 suture was then made round the reversed ends of the pins.

By this means perfect apposition was secured. The wound in the soft tissues of the face was allowed to granulate untramelled by external pressure. The patient was enabled to take food easily and well, and, in short, made so excellent a recovery that he left the hospital on the 26th of November, exactly a month from the date of his admission, with his wounds all healed, his jaw moderately firm, and the line of his teeth perfect.—*Lancet, Aug.* 17, 1867, *p.* 195.

56.—ON THE RAPIDITY OF THE PULSE WITH REFERENCE TO RISKS OF OPERATIONS.

By James Paget, Esq., F.R.S.

[The following short paragraph we take from a lecture "On the Various Risks of Operations."]

People with slow pulses bear operations just as well as those who, in all other respects than that of their heart's action, are like them. And people with habitually rapid pulses are not bad patients, if the rapidity of the pulse be not associated with some organic disease. Especially you will find a considerable number of children and young persons, chiefly sensitive girls, whose pulses are rapid enough to frighten you. Observe whether the respirations are in the same proportion rapid; if they are not, the respirations, and not the pulse, must be your guide in judging what is the patient's state. Many a time I have pointed out to you a pulse beating 120 or 140 times in a minute, and said that it meant no mischief, because the respirations were not more than 20 or 25.

And there is a set of cases in which you must always apply this rule of checking the indications of the pulse with those of the breathing,—namely, cases of hemorrhage. After large bleedings, when the patient recovers from their immediate effects, the pulse is usually hastened, and the breathing is retarded; so that with a pulse of 120 or more there may be not more than 10 respirations in the minute.

Mere irregularity of the pulse, if it be habitual, and not connected with valvular disease or degeneration of the heart, does not, so far as I know, affect the chances of recovery from operations. If the structure of the heart, as well as its functions, be disordered, you may judge according to such rules as I have just stated.—*Lancet, Aug.* 10, 1867, *p.* 152.

57.—ON THE REDUCTION OF DISLOCATIONS OF THE HIP-JOINT BY MANIPULATION.

By Thomas Annandale, Esq., F.R.S.E., Assistant-Surgeon, Royal Infirmary, Edinburgh.

[The method of reducing dislocations of the hip-joint by simple manipulation is by no means modern. It was described by Hippocrates, Wiseman, and other early writers. The method is so simple and successful in the majority of cases—so much more easy in its accomplishment than the plan in which the pulleys are used, that in time its use will probably entirely supersede the latter. In the following case the patient was a strong muscular man, about 48 years of age. The head of the femur was dislocated upwards upon the dorsum ilii.]

The patient was placed on a mattress laid on the floor, and then put completely under the influence of chloroform until the muscular system was thoroughly relaxed. The pelvis being steadied, I seized the left ankle with my right hand, at the same time grasping the corresponding knee with my left hand, and flexed the leg to its complete extent upon the thigh, and the *thigh* upon the abdomen. With the limb in this position, the knee and foot *were made to take* a circular sweep outwards as far as they would go, and

then the whole limb was suddenly extended. This movement caused the head of the bone to slip into the acetabulum at once with a distinct jerk.

In some of the dislocations of the hip-joint the direction of the circular sweep should be made inwards, and a little rotation of the limb often assists the head of the bone into its place.

Occasionally a recent dislocation of the hip-joint can be reduced by simple extension and manipulation, without the employment of the pulleys. The next case was treated successfully in this way:—

A miner, aged 48, having sustained a dislocation of the hip-joint backwards into the ischiatic notch, was admitted into the Clinical Surgical wards of the Royal Infirmary in March 1866.

The patient having been placed on a mattress, was put fully under the influence of chloroform, and then by slight extension and manipulation of the limb, Mr. Syme readily turned the head of the bone into the acetabulum.

If the head of the bone should have become fixed in a recent or old-standing dislocation, and cannot be moved by flexion and manipulation, it will, I think, be found useful to make some extension in the proper direction, either with or without the pulleys, until the head of the bone is dislodged, and then, suddenly removing the extending force, to adopt the flexion and manipulation method.

In this way I lately easily reduced the following case of dislocation of the hip-joint upwards on to the pubes.

A man, aged 62, was admitted into the Clinical Surgical wards of the Royal Infirmary on the 2nd of March 1867, with a dislocation of the hip-joint on to the pubes, of three days' standing. Having placed the patient on a mattress, and put him completely under the influence of chloroform, the flexion method was tried, but the head of the bone would not move from its abnormal situation. Extension of the limb in a direction outwards was then made by means of the pulleys, and as soon as the head of the bone could be felt to leave the pubes, the extension was suddenly withdrawn, and at the same moment the flexion plan was adopted, the foot and knee being also rotated inwards. As soon as this was done, the head of the bone slipped into the acetabulum.

As an example of what may be done by manipulation in reducing a dislocation of another joint, let me relate a case which was sent to me a few weeks ago.

A big strong man, aged 50, came from the country, in February last, with a dislocation backwards of the first phalanx of his left thumb. Careful and repeated attempts had been made to reduce the dislocation in the country, but they had proved unsuccessful. Having chloroformed the patient, I first tried extreme flexion, and afterwards extreme extension (in a direction backwards) of the thumb, but without succeeding in reducing the bone. It was then my intention to have divided subcutaneously the agents preventing reduction; but before doing so, I grasped the thumb, slightly flexed it, and after a few gentle rotations and manipulations, I was rejoiced to find that reduction had been accomplished, and therefore my operation was not required.—*Edinburgh Medical Journal, May*, 1867, *p.* 997.

58.—ON MAXILLARY DISLOCATION AND ITS REDUCTION.

By Dillon Kelly, Esq., Medical Officer of the Milltown Dispensary, Co. Westmeath.

If we study the physiology of the muscles of the lower maxillary articulations we shall find that the external pterygoids, in consequence of the direction of their fibres being nearly at right angles with that of the temporals, masseters, and internal pterygoids, must, as a consequence, act at nearly right angles with the action of those latter muscles, they also being the principal muscles in carrying the lower jaw forward; that being assisted to a certain extent, in that office by the superficial portions of the masseters and internal pterygoids, those being the proper grinding muscles, their actions must, as a thing of course, be associated with the motions of those muscles, and act simultaneously with them. Now, Mr. J. P. Vincent has shown very clearly that as

almost all muscular actions are performed in reference to a centre, so when by any accident that centre is deranged, if an abnormal or new centre can be readily formed, a new combination of muscular action is at once developed, as conformable as may be with the centre produced. When dislocation of the jaw takes place, a new centre is formed; and not only, therefore, does the direction of the muscles involved in that dislocation become altered, but their lengths from their new positions become altered also. Thus the external pterygoids become considerably shortened, in consequence of the new positions of the condyles anterior to the transverse roots of the zygomas; whereas the internal must, to a certain extent, become elongated by the throwing backwards of the angles of the maxilla.

Now, admitting that the muscles adapt themselves to their new positions and centres, and that the external pterygoids must be considerably shorter in maxillary dislocations than in their normal state, it follows that a degree of spasm of these muscles must be present to account for their abnormal decurtation.

Assuming, therefore, that the internal pterygoids become elongated from the elevation and recession of the angles of the inferior maxilla, they, from an opposite cause, must be the subjects of spasmodic action also.

Here, then, we have two causes, both conducing to the same end, yet either of them quite sufficient by itself to produce all the phenomena of muscular spasm.

If, then, we admit that the centre of motion of any articulation being accidentally changed, the muscles of that articulation will, in consequence of the irritation assumed by all muscles, when they are forcibly thrown out of their line of action, take on or exert abnormal action of extraordinary power. We have, then, another cause sufficiently powerful not only to account for all the phenomena of maxillary dislocations, but also capable of opposing an almost insurmountable obstacle to their reduction. A moment's reflection on the appearances presented by such a dislocation must at once convince us that such an alteration has taken place. Continuing our examination, it will become evident that the external pterygoids, acting under the influence of the pathological law already enunciated, draw the condyles forward with almost irresistible force towards pterygoid processes of the sphenoid, and hitch them against the transverse roots of the zygomas, which thus become their *points d'appui*, and produce a sort of dove-tailing between the depressions on their necks and the ridges constituting those roots. That the remaining elevators of the articulation are not in a state of quiescence, may be inferred when we take into consideration the direction those muscles assume in the new arrangement of the centre of motion, the internal pterygoids drawing the maxilla upwards and forwards, whilst the masseters and temporals draw it upwards and backwards. Here, then, we have, in like manner, another combination of forces, almost diametrically opposed to each other, yet all tending to the same end, and requiring, I think, no great stretch of the imagination to presume that the increase of muscular action developed in consequence must be the chief obstacle to the reduction of such a dislocation.

To M. Nelaton, however, is the merit due of being the first to act on the inference derived from what he supposed the chief obstacle to reduction, and of practically applying means based on that inference, by directing the patient to open his mouth as wide as possible when about to reduce a maxillary dislocation; at the same time, that he completely overlooked the equally palpable inference to be drawn from his first step in the process of reduction, namely, the relaxation of all the muscles engaged in the dislocation by the action of opening the mouth.

Mr. Vincent, in his work on associated muscular action already referred to, observes:—

"That the overpowering strength which a muscle is brought to exert when its usual direction of action, about a centre, is forcibly changed, is another law of conditions of the utmost consequence to the surgeon.

"The dislocation of the patella on its edge is an example; the bone sets *the extensors of the* leg into action of the most violent kind, these muscles

act most powerfully when the limb is to be straightened, and, in the condition in question, they act with a power that defies all the force that human aid can call to its service.

"Their force is also partly the effect of that irritation which all muscles get when they are thrown out of their ordinary line of action, particularly when they are disturbed in moving round their ordinary centre of motion. We have only to fix the leg a little, and this powerful opposition to restoring the patella ceases on the slightest rotatory motion.

"The disturbed arrangement here, was the elevation of the centre of action of the extensors above the ordinary position, and as these muscles, in the straight position of the whole limb, are called upon to support a great proportion of the weight of the body, so when in that position, they are naturally impelled to exert vast force.

"But, in obedience to the associated action of combined muscles, when the leg is bent, and another order of motions in this complicated joint, brought into play—then, these extensor muscles immediately relax, as they would otherwise, by their action, prevent the rotary motion of the leg upon its axes; thus, the moment the leg was bent, the extensors returned into a comparative state of repose and left the patella quietly to resume its appointed position."

Acting on that principle, in May, 1839, I reduced, with the greatest ease, a maxillary dislocation produced by the retching of gestation in a stout young woman, the wife of a respectable trader in this town, to whom the same accident occurred repeatedly afterwards.

In March, 1847, Mary Thompson, aged 55, a beggar-woman, of stout make, dislocated her lower jaw by falling against the end of her bed at night.

Her neighbours, in the morning, seeing she could not shut her mouth, unanimously were of opinion that she was "fairy-struck." Firmly believing in the truth of this superstition, she sought no surgical aid, and remained in a state of suffering for four days; yet at the same time making every effort to procure funds to bring her to a fairy practitioner at some distance.

Whilst thus engaged, she accidently called on Mrs. F., for some relief, who at once recognized the nature of the accident, and sent her to me.

In this case also, notwithstanding the length of time that intervened, and the muscularity of the patient, I reduced the dislocation without the slightest difficulty.

Lastly, in August, 1850, J. M'Donnell, aged about 30, tall and rather muscular, having gone to the funeral of a friend, and drank too freely, the violent retching that succeeded dislocated his lower jaw. Being up to that period a teetotaller, and not knowing what had occurred, or the nature of the accident, he and his family imagined he had suffered an infliction for breaking his pledge, from which he was destined never to recover; and I found him, in consequence, although still labouring under the effect of intoxication, in a state of the greatest distraction.

In this case I was equally and instantaneously successful.

In accordance with the theory, therefore, of associated muscular action it is necessary, for the reduction of this dislocation, that the pterygoids, masseters, and temporals should be relaxed, as being the chief or opposing forces to its reduction. And to relax them, it is necessary that the digastricus, mylo-hyoid, stylo-hyoid, and genio-hyoid, or group forming the depressors of the lower jaw, should be called into action by opening the mouth.

To reduce it, therefore, place the balls of the thumbs, covered with a handkerchief, on the crowns of the posterior molars, grasping the angles of the jaw externally with the fingers; then desire the patient to open the mouth, when the slightest pressure downwards, during the act of opening it, will be sufficient to disengage the necks of the condyles from the transverse roots of the zygomas, and will permit the temporals and massefers to at once reduce the dislocation by the reinduction of their normal action.

In conclusion, so easily is such a dislocation reduced in some subjects by that mode of treatment, that I instructed Mrs. F. in the manipulation, who was quite familar with the accident, and who also became so familiar with the principle, that she was accustomed to reduce her own dislocations by pres-

sure with her indices placed on the crowns of the posterior molars.—*Dublin Quarterly Journal, Aug.*, 1867, *p.* 36.

ORGANS OF CIRCULATION.

59.—ON THE RAPID PRESSURE TREATMENT OF ANEURISM.

By Dr. W. Murray, Newcastle-on-Tyne.

The object of this paper is to prove, first, that aneurisms of the largest size can be treated successfully by a process which is so rapid in its operation as to occupy less than an hour; and, second, that the cure takes place in these cases by coagulation of blood in the sac of the aneurism, and not, as has been hitherto believed, by the deposition of fibrine.

What is the nature of this treatment? The patient is put fully under the influence of chloroform, that we may be enabled to apply a very powerful pressing instrument on tender or sensitive parts, such as the site of the abdominal aorta. The full administration of chloroform is further necessary to relax the muscular system, which is an important condition of success, as the slightest movement of the pressing instrument by the muscular action of the parts pressed on, is fatal to this process of cure. I would draw special attention to the next part of the treatment, as success is dependent upon the care with which this is carried out. *It is the complete arrest of all movement of the blood in the aneurismal sac.* By the old method of cure, fibrine was supposed to coagulate and be deposited from the very slow, scanty, and feeble current of blood which found its way through the sac of the aneurism while incomplete pressure was maintained. Such a current, however feeble and slow, would be absolutely fatal to the rapid treatment. Complete arrest, therefore, of a current of blood, which shall be retained in a motionless state, is the secret of success. In fact, you must do by pressure exactly what is done by the ligature when it is applied to the artery above the aneurism; and you may do even more than is done by the ligature; for Dr. O'Ferrall, of Dublin, has insisted upon the application of distal as well as proximal pressure; and Dr. Mapother has carried out his suggestion with complete success. Aneurisms requiring pressure on the abdominal aorta are perhaps least dependent on distal pressure, as the collateral circulation to the lower parts of the body is here so limited as to render a current into the distal orifice of the aneurism improbable. In order to obtain this complete arrest of blood in the sac of the aneurism, the most careful and energetic watching of the pressing instrument is necessary. You must have so deep an interest in the treatment that you will sit for a whole hour or more, enduring the most trying strain on your muscular and nervous systems, before you can hope to obtain a a cure. Your eye and hand must be continually testing the condition of the aneurismal swelling; and the faintest indication of pulsation there must be considered fatal to the process of cure, and at once remedied; in fact the tourniquet must be so placed as not to permit a single rush of blood into the aneurism. Nothing short of this will bring about a cure in a short space of time.

We must now consider the duration of the treatment. At Newcastle, a case of aneurism of the abdominal aorta underwent the process of cure in three-quarters of an hour; and, in another case at Sunderland, under Dr. Heath's care, consolidation was distinctly observed to occur within twenty minutes. In the Newcastle case, unsuccessful efforts had been made for four hours; and, at the end of that time, the aneurism remained unchanged. As neither increase of solidity, diminished pulsation, nor decrease in size, could be detected, I determined to make a final effort. The patient being fully under the influence of chloroform, I re-applied the tourniquet, and held it firmly and securely over the aorta so as to obliterate every trace of pulsation. By a prolonged effort, three-fourths of an hour passed without a single slip of the instrument. It was then removed, and the aneurism had ceased to pulsate. A slight movement was perceptible for some time afterwards; this being an *impulse communicated* from the pulsation of the aorta above it.

In Dr. Heath's case, the pressure had been kept up irregularly for about ten hours, when the patient fainted under the chloroform. The pressure was then removed, and the pulsation and other characters of the aneurism were found to be as bad as ever. The patient was then urged to bear a final attempt without chloroform. This he did; and, to our amazement, when, at the end of twenty minutes, he declared he could bear the pressure no longer, we found the aneurism had become solid and had ceased to beat.

Here, then, are two cases in which the actual process of cure was brought about in less than an hour. How is this effected? We affirm by coagulation of blood in the sac of the aneurism. We believe this, because of the short time occupied in bringing about a sudden and decided change in the disease. It seems to me impossible that fibrine can be so rapidly deposited from the blood in such quantities as to fill up the cavity of a large aneurism, and to change a pulsating, thrilling, and expanding sac into a motionless solid mass. This opinion is supported by another important fact—the large soft mass which is produced by the filling of the aneurism with solid matter disappears in a few hours. In each of the above cases, all trace of the aneurismal tumour was gone before we could obtain a plaster cast of the spot. Rapidity of disappearance, then, as well as rapidity of production, is in favour of the existence of coagulation of the blood. In it we have contraction of a clot of blood inside the aneurism, by which the watery parts of the blood are squeezed out and absorbed, such as occurs in a clot of blood outside the body.

In conclusion, we may observe that the solution of fibrine in liquor sanguinis is, like all complicated natural phenomena, to be regarded as resulting from the adjustment of certain conditions to the forces by which it is maintained. When we disturb the conditions, the phenomenon ceases, to be replaced by another equally remarkable—coagulation. In fact, by substituting the conditions of coagulation for those of the solution of fibrine we cure aneurism. The day may come when even a simpler plan than pressure will effect this change; but, at present, it must be admitted that a great advance has been made in limiting the time and altering the process by which this disease may be cured.—*British Medical Journal*, *Oct.* 5, 1867, *p.* 287.

60.—ON THE TREATMENT OF ANEURISM BY COMPLETELY ARRESTING THE CURRENT THROUGH THE SAC.

By Dr. E. D. Mapother, Surgeon to St. Vincent's Hospital, Dublin.

The treatment of external aneurisms by compression, one of the greatest of modern surgical improvements, was established by Dublin surgeons. They advocated such a degree of compression as would send the blood gently flowing through the sac: layer after layer of coagula being expected to form, as happens, it was asserted, when aneurisms cure spontaneously. Arrest of current, and clotting of blood in the sac, were deprecated; but it seems to me that the following cases show that these events are, on the contrary, most desirable.

Case 1.—J. D., aged 25, healthy, was admitted into St. Vincent's Hospital, January 14th, 1865, for right ilio-femoral aneurism, which had begun without injury five months before. Digital and partial instrumental pressure having failed, I tried to stop the common iliac with an elas ic compressor, the patient being kept under chloroform for twelve hours. No clot formed. An anthracoid slough formed at the point of pressure.

Five days afterwards, another attempt was made, after the following preparatory steps. The abdomen was made lank by emptying the bowels and bladder; the limb was raised, bandaged and fixed to aid venous return, and to render increased flow of blood for muscular action unnecessary; and the sac was compressed by an elastic roller, so as to contract the space to be filled by the clot as much as possible. At Dr. O'Ferrall's suggestion, the superficial femoral was stopped, so as to keep the sac full. Signoroni's clamp

was then fixed over the common iliac artery for four and a half hours, when the tumour was found solid and pulseless, the common and external iliacs being still pervious. Absorption and complete cure followed.

Case 2.—J. B., aged 35, healthy, was admitted into St. Vincent's Hospital, May, 1866, for left popliteal aneurism, which followed a strain a fortnight before, while getting down from his cab. The sac was as large as a turkey-egg, and towards the biceps appeared diffuse. Digital and elastic instrumental pressure failing, stoppage as complete as could be achieved in a very restless patient was kept up for five hours, when the sac was found pulseless. Thirty-six hours afterwards, pulsation recommenced. After three other attempts, unsuccessful because chloroform was refused, the femoral at Scarpa's triangle was compressed, and the flow of blood out of the sac was impeded by tight bandaging and elevation of the leg, distal pressure on the popliteal not being possible. The patient was kept apathetic, not insensible, with chloroform, for nine and a half hours, when the sac was found hard and pulseless. At the compressed point, a superficial slough formed; but, with this exception, the recovery was rapid and perfect.

In both cases, bromide of potassium internally, and ice locally, gave aid by lowering the circulatory force. A good meat diet was given, to increase the plasticity of the blood; but drink was not restricted, for it is unphysiological to suppose that thus we can thicken that fluid. Fresh vegetable food augments the plasticity of the blood in scurvy, and may be reasonably allowed in the diet of a patient with aneurism.

Pain has been the great obstructive to partial compression; and complete pressure would be unendurable without chloroform, that inestimable boon, which we owe to the genius of Simpson. By acupressure also, that philosopher has enabled us to treat cases such as innominate aneurism now under Mr. Porter's care, in which compression was inapplicable, and ligature would be mortal. Chloroform darkens blood, but does not lessen its coagulability.

Distal pressure should precede and accompany proximal pressure; for thus the sac is kept full of blood at rest, and the resulting clot will equal its cavity. During the many hours which elapse before the separation of clot and serum is complete, the sac will probably contract on the clot, while the serum oozes away or is absorbed. In popliteal or antecubital cases, distal pressure being impracticable, Mr. Hart's flexion plan may achieve the same object. I doubt that aneurism has ever been cured by such pressure as would only lessen the calibre of the artery leading to the sac; for then the flow of blood being quickened, and the efferent vessel being larger, the blood would not remain long enough to clot. The blood flowing in would carry the agent to which the fluidity of fibrine is due; but, if the blood in the sac be isolated, this volatile agent may permeate its coats. In cases said to be cured by partial compression, it is probable that, the pressure having been increased to the amount of complete arrest for a short time, clotting in the sac, or plugging of the artery below it, produced the cure; for the pulsation is usually reported to have suddenly ceased. Digital pressure has of late been most successful, because it is usually employed to the degree of complete arrest. The evidence to prove that, in cases cured by partial compression, the sac is filled by laminated fibrine, is most insufficient; and, in fusiform aneurisms or sacculated aneurisms freely opening into the ruptured artery, it is impossible to conceive such an occurrence. In the fifty-three cases cured by compression, and detailed in the *Nouveau Dictionnaire de Médecine et de Chirurgie*, nine solidified under twelve hours, and four under four hours, in which time this much talked of stratification could not have been accomplished.

A few hours (perhaps less than an hour, as in Dr. Murray's aortic case) suffice to clot the blood isolated in the sac; while cure by partial compression takes, on the average, twenty-five days. One believer in the method persevered in its use for nine months. The anxiety and confinement probably lessen the coagulability of the blood; and it has only succeeded in 62 per cent. of the cases in which it was tried. Complete pressure has succeeded in at least four cases; and I am not aware that it has ever failed. With two

pressing points, and as soon as experience reduces the time of pressure to a minimum, sloughing need not be feared. In conclusion, I claim for this method greater rapidity and greater certainty than partial compression, and far greater safety than deligation.—*British Medical Journal, Oct.* 5, 1867, *p.* 286.

61.—ON A RARE CASE OF SIMULATED ANEURISM.

By Thomas Nunneley, Esq., Surgeon to the General Infirmary at Leeds.

[The following is, if not an unique case in surgery, at least one of the most curious and rare ever seen. There was a comminuted fracture of the neck of the thigh-bone, of the occurrence of which no history was obtained, either from the patient, his wife, or his sons. It must have occurred without violence and almost without symptoms, so that it was never even suspected by anyone. The abnormal distribution of vessels was most unusual, and, in connexion with the unknown fracture, simulated in a very exact manner, a disease which had no real existence.]

You will therefore perceive that, in the outset, I openly confess to a mistake in the diagnosis of this case. Now, in so doing, I am not about to excuse myself by saying that if I erred I did so in good company, nor to shield myself behind the fact that several others agreed with me in the opinion which I had formed; for no surgeon should shrink from the responsibility which attaches to himself individually. But I am about to say, now that a review of all the facts may be made, I do not consider that either I or they were to blame; as I do not think any other opinion could *then* have been formed, though perhaps *now*, and for the future, what we have learned by this case may be of use. At any rate it affords another illustration of how carefully we ought to investigate every symptom and explanation which by possibility may illustrate any case, and not to rely too implicitly upon those appearances which are even stated to be pathognomonic. It may even, perhaps, dispose some of you to incline to the conclusion that, with all the care you can exercise, you may occasionally be unable to avoid error; though, let me tell you, my object would much rather be, when you have committed a mistake, to stimulate you by increased study to render a repetition of the mistake impossible, than to induce a comfortable feeling of an unavoidableness and non-responsibility which would make you sit down contented under it. No; if such a feeling were to take possession of your minds, errors would soon cease to be mistakes—they would become crimes.

I will relate the case, and then add such comments upon it as I think may be useful to you, when you will see how these remarks are borne out by the case itself.

Albert H., aged 44, coal-miner, working at the Oaks Colliery, near Barnsley, presented himself on Friday, Nov. 16th, 1866, for admission into the Leeds General Infirmary. He had walked from the railway station to the Infirmary. He happened to be the last patient examined, and, as it had been a heavy day, he had waited in the room at least three hours. He walked up to the consultation table without any difficulty, into the private room where he stripped himself for examination, back to the table, and then up to his ward on the upper floor, without assistance. He had the look of a suffering man. Though somewhat lame of the left leg, and using a stick, there was neither eversion nor inversion, nor shortening of the limb. The body was bent somewhat forward, but the whole foot was placed on the ground in walking. The pain and stiffness of which he complained he referred to the groin, and not to the hip. On this side (the left) there was a swelling just under Poupart's ligament, in the course of the femoral artery, which was perceptible to both sight and touch. There was a decided pulsation. The tumour was not large, but was well defined, and certainly under the fascia lata. He stated that he had never had any accident or injury whatever, nor did he recollect having at any time had a fall; but that twenty-two weeks before, while in perfect health, as he was walking on level ground to his work,

as usual, he suddenly felt something give way in his groin. He immediately felt sick, and was unable to walk. The pain was so intense that he became helpless, and had to be assisted home by two fellow-workmen, between whom he walked a considerable distance. Since then he has not been able to do any kind of work, or move the leg much. The suffering has principally been in the groin. There is neither deformity nor tenderness about the buttock.

This account of the commencement of the attack he repeated many times in my hearing, and also gave it in precisely the same terms to all the surgeons, all the officials, the dresser, and to other students,—for, as it was at once pointed out as an interesting and important case, many of them took notes of it. I dwell upon this because the statement formed an important element in the diagnosis. I may here also mention that, during the few days he remained in the house before the operation, he got out of and into bed by himself, walked about the ward, and on the day of the operation walked by himself into the room from his ward. The swelling might be either a small aneurism or a tumour immediately behind the artery. There was a very distinct pulsation to be both felt and seen. There was a decided lateral pulsation, and there was an evident distensile pulsation. It was not a mere elevation of the swelling, but an enlargement from within. With a stethoscope on the part a distinct bruit was perceived—very different from that in the right thigh. Pressure on the femoral below the swelling increased the size of it, and also the distinctness of the pulsation. Pressure on the iliac artery at once lessened the swelling and the pulsation, both of which were absent so long as this was kept up, and returned on its being removed. In the popliteal space the artery was less perceptible than that of the right side. The limb was colder and weaker than the right leg; it was not swollen. These symptons, with the sudden accession of the complaint, induced me to consider it to be a small aneurism, probably springing from an opening on the posterior side of the femoral artery, very high up, and not containing much, if any, coagulum. Mr. Samuel Hey then saw the man with me; and, in order not to influence his judgment, I left him to investigate the case, not telling him the opinion I had formed of it. Without any hesitation he arrived at the same conclusion as I had done. Subsequently my other two colleagues did the same. You were present at the consultation, and heard the remarks made. It was therefore determined to tie the external iliac artery. This I did on the 22nd of November. The operation was easily done. The peritoneum was reached and pushed aside, when the finger came upon the artery. It was felt to be of more than ordinary size. Neither it nor the vein was seen. I carried the ligature from the outer side, and brought the aneurism-needle up between the artery and the vein. This is a method which I commonly adopt, though you will see in books it is recommended that the needle should be, in such operations, passed between the vein and the artery, so as to bring its point up where the vein does not lie, that this may not be wounded by it. Now, as I think there is more danger of the flaccid thin vein being caught by forcing the point of the needle in between the vein and the artery than there is by bringing the needle up between them, when the needle is kept well close to the artery by passing it from the outer side, I prefer to so carry the thread. By so doing, if the needle point be blunt, I do not know how the vein can be wounded. If there is to be a large separation made between the artery and vein by dissecting them from each other and from their sheath, you may easily pass the ligature in the way commonly directed; but, as I think it of primary importance not to separate them from each other, nor either of them from the containing sheath, I believe you may more easily if you do not separate them, or even see them (particularly the vein), safely pass the needle as I recommend you to do, for the firm arterial coat will safely guide you. By so doing, you will, I think, have very few cases of the so-called secondary hemorrhage from ulceration or sloughing of the vessel, which, in my judgment, is more frequently caused by the connexions of the vessel having been destroyed during the operation than by all other causes put together (except, perhaps, the near existence of a branch). Of course, if a surgeon thinks it *necessary actually* to see an inch in extent of a bare artery before he feels

sure of his position, and cannot see with his finger-ends (as it was long ago not inaptly called), but must have a full revelation to the eye itself, then to proceed to place a ligature round an artery in the manner I recommend would not be prudent in him; though to another, whose fingers are *habile* to the work, it is easy enough, and far better for the patient.

Flexible iron sutures and plaster closed the wound. On tying the ligature the tumour instantly lessened and the pulsation ceased, so that everyone present was confirmed in the correctness of the diagnosis. A woollen stocking was kept on the limb, and it was enclosed in two flannel bandages with a layer of cotton wool between them. It was a little flexed upon a pillow. At first the temperature of the limb fell eight degrees (Fahr.) below that of the other, then rose two degrees higher. As the heat increased, the wrapping was gradually removed until the temperature of the two limbs was equal.

The patient expressed himself as much relieved. The chloroform excited great nausea and retching, for which a subcutaneous injection of one-third of a grain of morphia was used, with considerable benefit. He had a dry mouth, with great thirst; no appetite; and a very quick pulse. There was no attempt at effusion of plastic lymph about the wound; but as there wa an escape of thin dirty matter from it, it was dressed on the fourth day. On the sixth day, as this discharge continued, the plaster and sutures were removed, and a poultice was applied. There was no attempt at union. Though the frequency of the pulse abated, the sickness disappeared, he could take some food, and was not so thirsty; though there was no peritonitis, and the temperature of the two limbs was equal,—still I was not satisfied with his condition. There was no delirium or confusion of mind, and he had some sleep; yet there was an anxious countenance, a hesitating way of answering questions, and the toes became dark. He showed no power. On the ninth day he died, it being not very obvious why he did so, unless from exhaustion.—*Lancet, Sept.* 28, 1867, *p.* 390.

62.—ON THE TREATMENT OF ANEURISM BY ELECTROLYSIS.

By Dr. John Duncan, M.A.

[The patient, the account of whose case commences the following article, was 52 years of age, and stated that he had discovered pulsation in the upper part of his chest a month before his admission into the Barnhill Hospital, Glasgow. When first seen, the tumour projected about two inches from the surface of the chest, was nearly round, and had a diameter of more than four inches. It was situated to the right of the sternum on the level of the second and third ribs. The pulsation in it was strong throughout, and the aneurismal bruit loud and distinct.]

On account of the rapid increase of the tumour, the apparently small amount of coagulum in it, and the comparatively slight disturbance of neighbouring organs, I agreed with Dr. Macgregor that the tendency to death was by external hemorrhage, and we determined to attempt its delay by means of electricity.

The operation was therefore performed on the 21st of June, 1866. The patient was placed on his back, and two needles were introduced from the outer side, parallel to each other, and about one inch apart. The positive needle, which was coated with gutta-percha, traversed the centre of the tumour; the negative, coated with glass, was placed a little lower. The electric circle was closed at 11.58 a. m. by means of a key interposed in the wires.

Six minutes after closure he began to complain of pain, the skin seemed to grow more tense, and a slight tympanitic note could be elicited by gentle percussion on the tumour. All these signs of galvanic action became more marked as we continued the application. He at first described the pain as a "burrowing," a "boring," a "weight on the chest;" but three minutes before the end of the operation he said it had become acute and burning.

The electric current was stopped at 12.15, seventeen minutes after its commencement, a slight lividity began to show itself round the negative needle, the glass coating of which had cracked in introduction, and slightly separated from subsequent movements. On withdrawing the needles, a portion of the gutta-percha which covered the positive was left in the wound. It had become softened during the operation. Then also a quantity of gas escaped into the cellular tissue, and crepitated on pressure.

The patient passed a restless night—partly on account of another sufferer in the ward. Next morning his pulse was 100, and he had little appetite for breakfast. From the escaped gas and consequent œdema, the tumour measured nine inches from right to left, but was not much, if at all, changed from above downwards. The skin was tense and tender to the touch, so that he could not bear even slight pressure without a sickening sensation. The pulsation was not very notably diminished. The same evening the swelling began to decrease, and the pulse to fall; and by the 24th, the tumour was much as before the operation, retaining only an oval shape. The patient stated that he felt relieved, and especially that the shooting pains in arm and head were very greatly better. He complained, however, of uneasiness at the sternal edge of the tumour.

On the 27th, a small abscess formed at the point where the positive needle had penetrated, and an insignificant dry slough at the other puncture began to separate. Under a light poultice the slough came away and the abscess discharged itself; and, by the 29th, the aneurism had completely resumed its original shape and size.

From the 7th to the 24th of July it slowly increased again, becoming more uneven on the surface, while the pains returned to their former severity. The increase was mainly upwards, and the lower part felt moderately hard, and had a less distinct pulsation than the upper. On the 24th, I extracted, by a minute incision, a little ball of gutta-percha, which had remained under the skin since the operation, and had become encysted. I also repeated the galvanism, using needles coated with vulcanite, and inserting them in the upper or enlarging portion of the tumour. A battery of six Bunsen's cells was used (four had been the former number), and the action was continued for twenty minutes. It was stopped because the patient began to complain of feeling faintness, though the pulse did not diminish in force. The train of occurrences was nearly as has been already described,—rapid swelling with rapid subsidence of the same, pain on pressure and tension, relief, but not so marked as before, from the shooting pains, and for a short time a seeming arrest of growth. Dr. Macgregor notes on the 25th, the day after the operation:—"Tumour of an oval shape; pulsation very slight, and more like the general lifting of the whole bulk of a solid body than the expansive feel of an aneurism."

Soon after this, however, the nodulated character of the tumour became more marked, and three eminences, each about one inch in diameter, were especially prominent by the 10th of August, when also they began to assume a dusky red hue. One of these was on the right side of the tumour, another near the centre, and a third, the smallest but most elevated, lay close to the sternum. The pains now became more severe, and were accompanied by numbness in the course and distribution of the ulnar nerve; and, in short, all his symptoms were rapidly getting worse.

On the 11th, while getting up, he felt something give way in the tumour, fell back in his bed, and for some time had the appearance of one dying. He was still with difficulty restored by stimulants.

On the 12th, a portion of the central nodule assumed the appearance of a blister filled with dark serum.

On the 15th, the prominence to the right began also to point, and next day a small opening formed there, from which a drop or two of dark fluid escaped. This was followed, on the 17th, by a more copious though easily arrested hemorrhage. For some days there was no recurrence of the bleeding, but new nodules and blisters formed close to the central one, and, by *the 20th, a large superficial* ulcer covered the centre and left of the tumour—

At this time Dr. Macgregor wrote to me, and I suggested that the operation should be repeated. He did not, however, feel himself warranted in doing so alone, and I was unable to go to Glasgow.

The rest of the case is a story of repeated hemorrhages. On the 22d, there was bleeding from the little orifice to the right, which is described as being occupied by a coagulum that advanced and retired with each pulse in the aneurism. The 24th witnessed yet another from the same source, and, on the 25th, a stream of blood spouted from it to a distance of fully two yards. Two hemorrhages from the central ulcer took place on the 28th, and the last was so copious that, after soaking through the bed, it formed a pool on the floor. From this the patient died.

The post-mortem examination was made by Dr. Macgregor six hours after death. The large central ulcer measured three inches across, and in it were three orifices, one was patent, the two others were plugged by coagula. The orifice in the right of the aneurism admitted a large goose-quill and was also occupied by a clot. The tumour was sliced like a brain. In the centre was a firm mass having the appearance and consistence of liver, but with a circularly striated arrangement. This was surrounded by liquid blood, which filled the space between it and the wall of the tumour. The sac itself could contain a closed fist, and the central mass was about the size of a hen's egg. The aorta was largely dilated and lined by a perfect sheet of calcareous matter, which shone like porcelain and rang to the stroke of the knife. It communicated with the external sac by a smooth opening the size of a two-shilling piece situated close to the origin of the innominate artery. The second, third, and fourth ribs were eaten through, and the right edge of the sternum and lower border of the clavicle were in a state of carionecrosis. The heart was enlarged and the semi-lunar valves were calcareous.

In sending me the notes from which I have compiled the history of the case, Dr. Macgregor says:—"The post-mortem examination clearly proved that a considerable quantity of blood had undergone coagulation by the galvanism; and had the orifice in the artery been not quite so large, and the galvanism been more frequently or longer applied, success might have been the result."

It is evident that the patient's death was but little if at all delayed by the operation. The history seems to me to indicate the following as the course of events:—A coagulum, adherent to the wall was formed in the lower part of the tumour by the first operation, and the aneurism thereafter extended upwards. This clot was increased in size by the second, and the pulsation was thereby notably diminished. Still, not being entirely occluded, the aneurism continued to grow in various directions, and ultimately, in consequence of this growth and on the occasion of a slight exertion, the clot became separated from the sac. The "giving-way" sensation very probably marked the occurrence of this separation, which was followed by a speedy thinning of the aneurismal coverings. I have little doubt that even then, had I been able to operate a third time, death by hemorrhage would have been obviated; but from personal causes I could not leave town so often as I could have wished.

Although there is nothing particularly novel in the circumstances of this case, it corroborates the conclusions to be drawn from the one I have recorded in my former paper. It is evident that the action of galvanism when carefully controlled is not productive of danger to the patient. The care taken to prevent cauterisation of the sac, by avoiding contact of the exposed portions of the needles with it, obviated in great measure that tendency to inflammation which had proved a source of danger in other hands. Moreover, the insulation, pretty well attained by the glass and gutta percha,—perfectly so by the vulcanite coating of the needles,—prevented that sloughing at the points of entrance which has on more than one occasion given rise to unpleasant results by hemorrhage and otherwise.

While, then, little danger to the patient is to be apprehended from the operation, this case seems to show the possiblility of occluding the external portion of an aortic aneurism. Although it cannot be absolutely demon-

strated that the central clot was produced by galvanic action, its nature and position, taken in connexion with the experiments made on blood external to the body, with other like cases, and especially with the one I shall presently relate, make it extremely probable that it was so produced. How much was due to direct galvanic action, how much to subsequent deposition, it is impossible to say, though doubtless the greater part was immediately coagulated. But being there, it follows as a matter of course that, if a clot of the size of a hen's egg could be produced by two operations of twenty minutes each, more frequent and longer continued operations will have a yet greater effect, even to the filling by coagulum of any size of sac. In this respect I speak only of the sac external to the thoracic wall, for the hope of curing an aortic aneurism by galvanism cannot be seriously entertained. Not to speak of the impossibility of placing electrodes in accurate position in the thorax, or of the probability that whatever might be formed would be at once carried away by the rush of blood, it is certain that any clot formed under such circumstances would become free on withdrawal of the needles and would pass on to plug up the aorta or one of its branches. Galvanism in aortic aneurism is to be regarded in the same light as tracheotomy in that disease, as a means of prolonging life in exceptional circumstances, to be used only when the obvious tendency to death is by the way which it is calculated to prevent.

But while we must in this and other directions set limits to the capabilities of electrolysis, we have also examples of its perfect applicability to certain forms of aneurismal disease; and I have great pleasure in relating a case of cirsoid aneurism in which it afforded most complete relief.

About a year ago, James Gordon, a labourer, 21 years of age, consulted Dr. Hardy, of Leith, for an eczema of the scalp. In examining the head, Dr. Hardy observed a cirsoid aneurism, and recommended the man to go to the Royal Infirmary. He was there cured of the eczema; but it was thought injudicious to interfere with the tumour, and on leaving the hospital he put himself under my care.

The tumour was situated on the left side of the head, and reached from the vertex to the zygomatic arch. It resembled very closely in shape and size the half of a large Jersey pear. Its broad end lay upwards, and the enlarged temporal artery entered it below. The external carotid also was distended, and even the common carotid seemed large, and beat with unusual force. In front, the left anterior temporal artery curved round to enter the tumour; behind, the left occipital could be traced from it to the muscles of the neck; while the right temporal also contributed by means of its middle branch. The whole tumour pulsated strongly, and contained no clot. By firm pressure it could be so emptied of blood that the bone felt hard and firm, but it rose again at once on the hand being removed. A loud and prolonged bruit accompanied each pulse; and the constant sounding in the ear was the great cause of the patient's distress. By compression of the carotid or left temporal artery, the pulsation was greatly diminished, and the tumour rendered very flaccid; but pressure even on all its distinguishable affluents did not prevent a slight pulsatory movement.

It was evidently a case in which other means would be so dangerous and uncertain that galvanism deserved a trial.

On the 20th of July, assisted by Drs. Fraser, Hardy, and Joseph Bell, I operated for the first time. The needles, having one inch of exposed surface, were entered at the vertex and placed parallel to each other, about an inch apart. Four cells were used, and the action was continued for twenty minutes. Pressure was maintained up the affluents throughout the operation. The closure of the circle produced a smart shock, causing the patient to quiver; and another, not so severe, accompanied the opening. During the action a painful burning sensation was experienced; and in this as in subsequent operations, Gordon stated that he distinctly heard the "blood boiling" in the tumour. When the current had been continued for about ten minutes, the aneurism began to swell; and soon afterwards the tympanitic note on *percussion* became audible. A slight flow of blood followed the withdrawal *of the needles, but it* was at once checked by lint and a bandage.

The same evening there was slight œdema all over the swelling. On its subsidence next day, there was felt in the centre of the tumour a hard nodule, divided into two parts by a tortuous pulsating passage.

At a second operation, on the 4th of August, the needles were introduced one on each side of the central nodule, and six cells were used. The shocks on closing and opening caused the patient to start violently, and tremble for about a minute; but the burning pain was not greater than with four cells, and was easily endurable. The action was stopped after twenty minutes, on account of an accident to one of the cells. The effects were precisely as already detailed. The hard core was increased in size, and the rest of the tumour continued to pulsate as before. From the negative needle not having been deeply enough buried, a piece of skin, not larger than a pea was cauterised and separated in a few days as a thin scale.

On the 8th of August one needle was placed in the middle pulsating passage, the other towards the back of the tumour, and further additions were made to the consolidated mass. On this occasion the patient became a little sick from the pain, and the circle was opened after eighteen minutes' action, to relieve his uneasiness.

I was now for some time prevented from seeing the patient, but by the 28th of August no change had occurred; and on that day, with the help of Dr. Andrew Inglis alone, I put the patient under chloroform, to avoid the shocks, which he greatly dreaded, and proceeded to repeat the operation. The initial shock, however, caused him to throw up his hand and draw out one of the needles; and as we found ourselves unable to control him, and at the same time manage the battery and the chloroform, I contented myself with binding up his head to stop the bleeding.

On the 31st, another failure occurred; for the patient had, contrary to orders, eaten largely before the operation; and after about five minutes' action it was found impossible to keep the needles in position, on account of his motions in vomiting.

On the 5th of September, however, a most successful repetition was made. The positive needle was introduced about an inch above the zygoma, and placed with its point directed upwards; the negative had its point directed downwards in the front of the main tumour. They overlapped about half an inch, and were more than that distance apart. A current from four cells was passed through them for twenty-five minutes. The immediate effects on the aneurism were much as before; but when I saw the patient in the evening, he stated that the bruit had entirely disappeared; and on applying the stethescope I found that it was so. Two days afterwards, when the œdema had quite subsided, I made a careful examination. The neck of the tumour above the entrance point of the positive pole was consolidated; below that point it had collapsed. The temporal and carotid arteries had resumed their normal size. The centre of the tumour was entirely occupied by a hard mass, but between this and the surface was a fluid pulsating layer supplied by four tortuous and enlarged vessels, viz., the right temporal, the occipital, and two branches of the left temporal, which ran one before and the other behind the obliterated neck, and entered at opposite points half-way up the tumour.

On the 12th of September, I proceeded to obliterate this superficial layer, by introducing the negative needle, pretty deeply behind, passing it even through the coagulum, while the positive was placed in the fluid portion slightly anterior to the middle line of the aneurism. After twenty minutes' action, I observed a slight lividity of the skin over the positive needle, and at once stopped the current. Unfortunately it was too late, for next day it became evident that the skin had been cauterized over a space about the size of a shilling. The whole mass was now, however, consolidated, except where the anterior and right temporal arteries entered, and neither portion was larger than a threepenny-piece. I obliterated the anterior a week afterwards, by passing under it a needle and compressing it for twenty-four hours by means of a piece of cork, secured by a figure-of-eight silk thread. The same process *was not successful with* the right temporal, and the patient

would not allow me to repeat the attempt, as he had returned to his work, which he did not wish to be interrupted. The rest of the history is entirely favourable. The slough, much smaller than had been anticipated, was very dry and tough, and took a month to separate. But, six weeks after the last galvanic operation, the cicatrix was the only trace that remained of the tumour, with the exception of a slightly enlarged right temporal, and a barely distinguishable occipital artery. The man has continued to work since that time, and there is no tendency to a development of the disease from these sources. If there were, a needle passed under or through these small vessels would serve effectually to obliterate them.

The very satisfactory termination of this case, should, I think, be sufficient to establish electrolysis as a valuable therapeutic agent, well suited to the treatment of cirsoid aneurism. The only real difficulty which attended the operation was to get a maximum of effect without endangering the skin. I had previously met with the same difficulty in applying electrolysis to a nævus in the eyelid of a child. Very small needles were then used, and the current was continued for a few minutes only. Only a small central induration was, in fact, produced; and, as some impatience was expressed, I resorted to subcutaneous ligature instead of trying a more efficient application of the battery. Till the last operation on Gordon's head, I succeeded in erring on the safe side; but, notwithstanding, nay even in consequence of the mistakes in that one instance, some facts may be elicited for guidance in future. The first operation affords, perhaps, the best data for estimating the coagulating power of electricity under given conditions. As I have already said, four cells of a Bunsen's battery were used; the period of action was twenty minutes; the active portion of each needle was one inch in length, and their thickness No. 16 of the wire gauge. A hard coagulum was formed, of which the breadth was an inch and a third; but a narrow uncoagulated passage remained rather nearer the negative than the positive electrode. Now this passage was, in all probability, due to a vessel lying just within the surface of the coagulum, which would thus be perfectly continuous behind it, and may in any case be very fairly estimated at one and a quarter inches in breadth and rather less in thickness. The length was one-fourth greater than that of the exposed steel, to which, of course, it will always correspond.

The intermediate operations afford no reliable data for such an estimate, on account of the relation which the needles bore to each other and to the already-formed clot; but the last is again of some importance. All other conditions being the same, the negative needle was placed perhaps not quite three quarters of an inch from the surface, the positive certainly not half an inch. The former brought the coagulum, in which it lay, up to the surface; the latter cauterized the skin.

From these facts it may be safely concluded—

1st, That both needles are efficient.

2d, That, under the circumstances named, they are together capable of producing more than one cubic inch of coagulum, the greater part of which is due to the positive pole.

The great object, and I may add, difficulty, in an ordinary aneurism, is to prevent that amount of irritation which is liable to excite a dangerous inflammation. On this ground, as on every other, there seems no cause of apprehension in a cirsoid aneurism; and for that form of disease galvano-puncture may, I think, be regarded as the safest, simplest, and most certain method of treatment. Ligature of large arteries, like the carotid, is extremely dangerous, and has been singularly unsuccessful. Out of eighteen cases, seven died, and only two were permanently cured. The perchloride of iron, though it has been successfully injected, is also liable to failure, and has, moreover, certain well-known risks attending it. Extirpation, ligature, and incision have been tried, but hold out little inducement to repeat them. Galvanism, however, has been successful in the only two cases in which it has been used; *and though several* operations were required in each, not the slightest accident *happened in either.*—*Edinburgh Medical Journal, Aug., 1867, p.* 101.

63.—CIRSOID ANEURISM CURED BY INJECTIONS.

Prof. PITHA showed, at the Medical Society of Vienna, a young man whom he had cured of a cirsoid aneurism of the side of the head. It was of large size. The occipital, posterior, auricular, and temporal arteries were much dilated, being as large as quill-pens, the temporals and occipitals showed sacs as large as nuts. The auricular was enormous, violet in colour, and showed distinct pulsations. Extirpation was not to be thought of, and ligature of the external carotid would have been useless, as the collateral branches would have carried on the circulation. Pressure, which was borne with much patience, caused œdema without diminishing the size of the tumour. Injection of the sesquichloride of iron was then tried. It formed a clot; the part became hard, suppurated at points, and the tumour disappeared. After ten injections only a little capillary dilatation remained; the occipital, which resisted longest, still pulsated.—*Edinburgh Medical Journal*, *Aug.*, 1867, *p.* 191.

64.—ON ACUPRESSURE.*

By Dr. WILLIAM PIRRIE, Professor of Surgery in the University of Aberdeen.

[The following paper was read by Dr. Pirrie, at the annual meeting of the British Medical Association, at Dublin:]

The First Method may be quickly and easily performed by placing the left forefinger or thumb against the mouth of the bleeding artery, sending the needle from the cutaneous surface direct through the whole thickness of the flap, and causing it to emerge a little to the right side of the tube of the vessel. The projecting end is then pressed firmly against the site of the artery, made to re-enter the flap close to the left side of the vessel, and pressed on till it emerges on the surface of the skin. In this method, the artery is compressed against the component parts of the flap, to use the words of Sir James Y. Simpson, "just in the same way as, in fastening a flower in the lapelle of our coat, we cross over and compress the stalk of it with the pin which fixes it, and with this view push the pin twice through the lapelle." In some cases, the vessel can be conveniently pressed against a neighbouring bone, instead of against the soft parts of the flap. The portion of the needle within the wound is very small; but, in many cases, it may be passed higher up, so as to compress the artery without emerging on the wound-surface. In many of my most gratifying cases, I have adopted this modification; and by so doing the wounds have not had, even for the shortest time, any foreign body within them.

The Second Method is performed with a needle threaded with twisted iron-wire, and—unlike as in the first method—the skin is not interfered with, and the needle is passed above instead of below, the artery. The needle is pushed twice into the soft tissue of the wound. The first point of entrance is at a little distance from the artery to be acupressed, and the first point of exit close to it. The second point of entrance is close to the vessel on its opposite side; and the second point of exit at a little distance. Between the first point of exit and the second point of entrance, the needle is made to bridge over the track of the artery; and care must be taken, before making the needle re-enter the wound, to press it down sufficiently to close the artery. For this purpose, the soft parts may be pressed with the tip of the left forefinger, to aid the pressure of the needle in closing the artery before the needle is fixed by being pushed a second time into the soft parts. I trust this description will make the performance of this method perfectly clear. The needle can be removed at pleasure by pulling the twisted wire. In minor operations, and in acupressing vessels of moderate size, I have often used this method; but I have never adopted it in acupressing an important

* Although we have described the different methods of acupressure in a former volume, from the writings of Sir J. Y. Simpson himself, the subject is so important that we will again describe them in the words of Dr. Pirrie.

artery in a major operation; because, although I have been greatly surprised how little pressure, when direct, is sufficient to stop the circulation through an artery, there are other methods whose reliability is unquestionable, and I have, therefore, thought it judicious to give them the preference. Occasionally I have used a pin instead of a threaded needle; and I invariably do so when the form of the wound and the situation of the artery admit of the head of the pin being conveniently kept without the wound.

The Third Method requires for its performance a threaded needle and a loop of inelastic iron wire; and consists in effecting compression between the needle *below* and the loop *above* the vessel. The needle is entered a few lines to one side of the vessel, and, as I now demonstrate with this needle and flap, pushed behind it; caused to emerge a few lines beyond the vessel; the loop of wire is thrown over the point of the needle; brought over the track of the artery and behind the stem of the eye-end of the needle; drawn sufficiently to shut the vessel; and fixed by half a twist around the needle. It is important, in the performance of this method, to avoid including an unnecessary amount of tissue; not to draw the wire tighter than is absolutely necessary to close the artery, lest its coats should be lacerated or the tissues molecularly injured; to arrange the wires so as to prevent wriggling or entanglement, and to relieve the vessels from acupressure at the earliest moment deemed advisable, which is done without the slightest difficulty. By pulling the twisted wire, the needle is removed; and the loop being liberated is easily withdrawn.

The Fourth Method differs from the third, inasmuch as a long pin is substituted for the threaded needle. Little as the difference is, the pin should be preferred in all cases where the form of the wound and the position of the artery admit of the head of the pin being conveniently and without straining of tissues kept without the wound. Kinking of the wire at the head of the needle has sometimes caused pain in withdrawing the needle. In the method with the pin this kinking is avoided; and, besides, there are the advantages—of greater facility of insertion; being more easily withdrawn; admitting of being twirled, which greatly facilitates removal; and avoiding all wriggling and entanglement of different kinds of wires. The pin, however, cannot always be used; as certain forms of wounds and some positions of bleeding vessels render it impossible to insert the pin in the proper position and keep its head out of the wound without straining of tissues, in circumstances where the threaded needle—owing to less length of unbending material—can be used with facility. The third and fourth methods are as efficient means of arresting surgical hemorrhage as could possibly be desired; and the principle of both is precisely the same.

The Fifth Method, or Aberdeen Method, or that by the Twist, is a favourite practice with my colleagues and myself, and may be performed with a long pin or a threaded needle; but the former, when admissible, is greatly to be preferred. For making clear the description of this admirable method—which may be said to consist of three different steps—I hope I may be allowed to repeat what I have already published in explanation of this proceeding. "In the first step, the pin is inserted on one side of the bleeding artery, then pressed onwards a few lines in the same direction as the length of the vessel, and its point caused to emerge on the surface of the wound. In the second step, a quarter rotation is given to the instrument so as to place it above the artery, and well pressed down against the small portion of tissues between the instrument and the vessel. In the third step, the pin is secured and the twist retained, by sending the point into the tissues beyond the artery. The operator has, on the cessation of the bleeding, a reliable proof that a sufficient degree of rotation has been given to the needle. The first time I tried the method by the twist, a half-rotation was given to the needle; but as so little pressure, when direct and steady, is sufficient to arrest hemorrhage, I have invariably preferred, in other cases, a quarter rotation. Of all methods of acupressure of which I have experience, this is the simplest, the easiest, the quickest, and, so far as experience in the Aberdeen Hospital warrants an *opinion, perfectly* efficient. Simplicity, efficiency, quickness, and ease of

performance, are unquestionably great recommendations of the method by the twist, when performed with a quarter rotation and a long pin; but this variety possesses two other advantages, in my opinion, of the greatest importance, for obtaining either immediate union or union by primary adhesion. The one, that there can be but little molecular injury or straining of tissues; the other, that, by the gentlest twirl and traction, the pin can be easily withdrawn, with extremely little, if any, discomfort to the patient. Surgeons who know how little produces pain in an amputation wound; how slight a degree of pain is apt to cause involuntary contraction of muscles in the stump; how certain such contractions are to separate parts of the internal surfaces of the wound from each other, although, by retentive means, its edges are kept in apposition; and who, after the greatest anxiety to obtain the best results, have so often in a few days witnessed the disheartening effects of such contractions, will fully appreciate the value of a means of arresting hemorrhage that, at the moment deemed judicious, can be removed, not only without pain, but almost without discomfort, to the patient. In this method the artery is, to a certain extent, twisted as well as steadily compressed; and in that way, no doubt, the occlusion of the vessel and the suppression of the hemorrhage are produced."

The Sixth Method, or that by the Ring, was suggested by my able colleague, Dr. Keith—who has done much to promote the adoption of acupressure—and requires for its performance a long pin and a loop of passive iron wire. The pin is inserted a few lines on one side of the bleeding mouth of the artery, made to pass close to but not to transfix it, and to emerge about two lines, or so, beyond the mouth of the vessel. Another important precaution during this step is, to have the point of exit as near as possible to the point of entrance, so that too much tissue may not be included in the ring in the next step of the operation. The surgeon then takes an end of the loop of wire in each hand; throws the duplicature over the point of the pin; brings back an end on each side of the vessel; crosses them behind the body of the pin; draws them in opposite directions to the extent necessary to arrest the hemorrhage, *but not more on any account;* brings an end up on each side; and fixes the wire by a semi-twist in immediate contact with the front of the pin. In making the semi-twist, it is important, for a reason which will afterwards be stated, to cross the end in the surgeon's left hand in front of that in his right. In every form of acupressure where wire is used, it is important not to draw it with more tightness than is absolutely indispensable to arrest the hemorrhage; and it is surprising how little pressure is necessary when continued and direct; but in this method, more than in any other, this precaution should be observed, owing to the wire being applied in a manner more likely, with undue force, to lacerate vessels and to cause molecular injury. But, besides, if the wire should be drawn with injudicious tightness, undue constriction of the tissues it embraces might cause difficulty in withdrawing the wire after the removal of the pin. Should such difficulty occur, through forgetfulness of the necessary precaution, twisting the wire which was held in the left hand from right to left will undo the twist, and facilitate removal. This method has been found extremely serviceable in perpendicular wounds where the artery has been cut short, and also in wounds containing a bleeding vessel not sufficiently accessible to be acupressed by the third or fourth methods.

The Seventh Method requires for its performance a long pin, which is inserted twice on the surface of the body, precisely in the same way as the needle is inserted on the surface of the wound in the second method. It consists in passing a long pin through the cutaneous surface pretty deep into the soft parts, at a little distance from the vessel, pushing it on, causing its point to rise up as near to the artery as possible, bridging over and compressing it, dipping the point of the needle into the cutaneous surface on the other side of the vessel, forcing it on, and causing the pin to emerge a second time on the skin. The artery is compressed between the bone and the middle portion of the pin in front of the integument. I have published an interesting case, in which I acupressed the humeral artery by this method, with

most gratifying results, on account of a wound in the upper part of the forearm attended with great hemorrhage.

[Professor Pirrie illustrated the various methods with large diagrams, and demonstrated with a needle on a flap.]

Synopsis of my Experience of Acupressure.—My experience of acupressure in important cases, of which records have been kept, comprehends its use in eleven cases of amputation of thigh; four of amputation of leg; two of amputation of arm at the upper part of the surgical neck of the humerus; one of amputation at ankle-joint; one of Chopart's amputation; two of amputation of the whole of great toe; twelve of excision of mamma; six of excision of elbow-joint; one of excision of knee-joint; one of excision of an erectile tumour; one of excision of tumour on chest; one of excision of tumour on thigh—wound eight inches long; one of excision of head of fibula; three of excision of testicle; one of hemorrhage from sloughing of hand; one of wound of hand; one of wound of upper part of forearm, with great hemorrhage; one of wound of the radial artery; and one of wound of hand, attended with great hemorrhage.

These fifty-one cases—in which alone I have acupressed one hundred and eighty-five vessels—are of a highly diversified character, and many of them are among the most important in the practice of surgery. They are admirably fitted to test the efficiency of acupressure, and to enable a surgeon to form a just appreciation of its merits. I have also employed this means of arresting hemorrhage in a large number of minor operations, as well as in many cases of wounds which occurred both in hospital and in private practice, but which I did not deem sufficiently important to put on record. My able colleague, Dr. Keith, acupressed the first case in the Aberdeen Hospital, and I acupressed the second. I have had occasion to assist Dr. Keith in all his hospital operations, and thus had ample opportunity of forming a correct appreciation of the value of acupressure in his cases as well as in my own. I have also assisted Dr. Fiddes in his operations; who, like Dr. Keith and myself, invariably practices acupressure. Such have been my opportunities of arriving at a just estimation of the merits of acupressure.

Merits of Acupressure.—Is acupressure a perfectly reliable means of arresting surgical hemorrhage? In the list I have just read of fifty-one important cases, in numerous minor operations, and in all accidents to which I have been called since my adoption of this new proceeding, there has not occurred one single example in which it has failed to arrest the hemorrhage, or where I found it necessary to substitute the ligature for acupressure; and such also has been the experience of my colleagues, Drs. Keith and Fiddes. What stronger evidence could be wished of the efficiency of acupressure for the arrest of surgical hemorrhage? And, as to the risk of bleeding on relieving a vessel from acupressure, in only one case did I meet with return of hemorrhage in doing so. It was a case in which I removed the leg in its upper third for medullary cancer. There was not a drop of oozing after the operation. Twenty-four hours after operation, earlier than usual, I relieved the anterior tibial artery from acupressure, and it instantly bled as energetically as if it had that moment been cut across. Acupressure pins and loops were in the ward; the house-surgeon and some of the surgical pupils of the hospital were with me at the time; and I immediately got the femoral artery compressed, opened the wound, and acupressed the artery again by the fourth method. The blood did not exceed a dessert-spoonful, and the whole proceeding did not occupy more than three or four minutes. This is the only instance in which I have seen any hemorrhage from an artery on its being freed from acupressure; and I attributed its occurrence in this case to the extreme exhaustion of the patient being unfavourable for adhesion. In only one case did Dr. Keith meet with hemorrhage on relieving a vessel from acupressure. It was after amputation of the forearm. The radial artery bled on removing the compression; but it was immediately acupressed again, and there was not afterwards a single drop of oozing. These are the only instances in Aberdeen. In upwards of eight hundred vessels, in two only has hemorrhage occurred on the discontinuance *of acupressure; and* I have no doubt as many would be found in the same

number of examples of deligation of arteries. Even without the valuable experience of surgeons elsewhere, the evidence furnished by the surgeons of Aberdeen appears to me to prove that acupressure is a means which can be entirely relied upon for the arrest of surgical hemorrhage, and that this question may be considered as set at rest.

Does acupressure accelerate the healing of wounds? This question may be put in other words; namely, Are perfect examples of healing by the first intention, or by primary union, equally attainable under deligation and acupressure? By a perfect example I wish to be understood one where not a single drop of pus has been seen. This appears to me the great question to be determined; because, although acupressure, like deligation, is a perfectly reliable hæmostatic; and although, in my judgment, acupressure has other advantages which make me greatly to prefer it, yet, although most desirable, they are comparatively of inferior importance; and the final general adoption or rejection of acupressure will no doubt depend, mainly at least, on the opinion arrived at by surgeons on this important question. As I have stated elsewhere, the use of the ligature is attended with an insuperable obstacle to obtain perfect examples of union by the first intention, or by primary adhesion without the formation of some pus. The well-known effects of the ligature, the laceration of two of the coats of the artery, the strangulation of the external coat, the molecular destruction of that coat, and the presence of the ligature acting as a seton in the wound, render more or less suppuration at the points and tracks of the ligatures inevitable. Healing by the first intention cannot, therefore, be perfect throughout in any case where the ligature has been used. Accordingly, I never saw in my own experience, or in that of any other surgeon, where the ligature was used, a perfect example of healing by the first intention, or by primary union, without any suppuration. I believe such a case never was, and never will be, seen. On the other hand, acupressure, if properly performed, and not too long continued, does not, so far as my observation has enabled me to judge, cause laceration, strangulation, nor destruction and suppuration at the site of acupressure. No doubt, the precise local conditions resulting from the use of the needle are not, as yet, so well known as those succeeding the ligature. Opportunities of making such examination in Aberdeen have fortunately been few; but in those which have occurred the coats of the vessels were perfectly entire. But, whatever the difference may be in the pathology of deligation and acupressure, I am perfectly convinced, from personal observations and experience, that acupressure accelerates the healing of wounds; and that under its use, aided by metallic sutures, the avoidance of all dressings, and perfect rest of the wounded part, surgeons may have the gratification of seeing, in many instances, the largest surgical wounds after major operations heal up directly without a drop of pus. Out of the foregoing list of fifty-one cases, in many of which "the major operations of surgery" were performed, twenty-one, comprising three cases of amputation of the thigh, eight of excision of erectile tumour, &c., healed entirely by the first intention, and without any pus. I regret that, on account of a recent family bereavement, my colleague Dr. Keith—whose experience of acupressure has been quite equal to my own—could not come to Dublin; as his report of cases of union by first intention without any suppuration, which I witnessed in his practice, would have added greatly to the testimony I bear to the advantages of acupressure. It should be remembered that these results have been obtained in the early days of acupressure, but with a very limited amount of recorded experience, and on many points none whatever for guidance; and that as surgeons become more experienced in the practice of this new proceeding, it is to be hoped that much more favourable results will be obtained. The practical results, however, already obtained have convinced me that acupressure is an invaluable means of accelerating the healing of wounds; that it does so by removing one of the hindrances to complete union throughout by the first intention; and that the anxiety, suffering, and risk, that in future may thereby be saved, is incalculable.

Has acupressure any other advantages over the ligature? In my opinion it possesses the following:—1. A briefer sojourn of a foreign body in the wound.

2. The foreign body is of a less irritating character, owing to its being of a metallic, and not of a textile, nature. 3. Acupressure is the easiest of performance, and the quickest of all methods of arresting hemorrhage. 4. The needle is removable at pleasure, instead of after a tedious process of ulcerative destruction and more or less of accompanying suppuration. 5. The comfort which the patient early enjoys from knowing that, as far as vessels are concerned, all further interference and suffering are at an end. 6. The diminished risk of the occurrence of pyæmia, owing to suppuration not being an inevitable consequence of this mode of arresting hemorrhage. It is a remarkable fact, that as yet there has not been a single instance of pyæmia in any case where acupressure has been employed by my hospital colleagues or by myself.

In conclusion, I have only to add that, having given acupressure a fair and unprejudiced trial, I have no hesitation in stating that I am a decided believer in its advantages; that my appreciation of its merits is such, that I regard it as the greatest improvement of modern times in the treatment of incised wounds; and that—with chloroform for preventing pain during operations and with acupressure, aided by metallic sutures, perfect rest of the wounded part, and the avoidance of all dressings—much suffering to patients may in future be averted, and the success of an important department of practical surgery greatly increased.

Discussion on Dr. Pirrie's Paper.—Dr. Buchanan: I have used the method of acupressure very extensively in the hospital of Glasgow; and my experience—perhaps owing to my want of care at first—has not been quite so favourable as that attained by Dr. Pirrie. Until I used the method of acupressure, I never saw a case of union by the first intention in all the amputations of any size in my own practice, nor in the practice of any other surgeon. There was one case of amputation of the shoulder-joint, in which I acupressed the axillary artery and two other vessels; and that case was the first case in the history of the Glasgow Infirmary of any amputation uniting by the first intention; so much so, that many surgeons in Glasgow did not believe in the possibility of such a thing, and came to visit the case. I think the thing is still in its infancy; because I am perfectly certain that union by the first intention will yet take place in all instances. I suppose it is in consequence of my want of exactness in manipulating that I have not succeeded in all instances. If you succeed in acupressing correctly, if you succeed in making an apposition of the flaps, then you may expect union by the first intention. The last case which I have related was accompanied by amputation, in the same case, of the leg below the knee. The young man on whom the operation was performed was rather restless in his bed, and, when I subsequently came to see him in bed, I found that he had kicked out two of the pins. To my astonishment, there was not a drop of blood lost. Inflammation occurred afterwards, which did not favour complete union; but it is a curious fact, that in acupressure, sometimes the pin may be removed in half an hour without hemorrhage. I think the method is one that deserves the attention of all practical surgeons.

Dr. Rawdon Macnamara: It is well known to my colleagues how earnestly I have taken up this subject of arresting surgical hemorrhage, and my experience of it is each day more and more satisfactory. In any operation I have lately performed, I have used no other means of arresting hemorrhage. In amputation of the breast, I have used no other means; and I have been pressing the matter on some of my professional friends in this city, who have been kind enough to follow out my suggestions and to employ acupressure. The way in which I was in the habit of employing it was what was called Simpson's third method. Now, the result of my more mature experience is, that Porter's twist is more satisfactory, as it is more rapid. Latterly I have been using Porter's twist. Mr. Smyly, in the Meath Hospital, used it a few mornings ago in arresting hemorrhage. This (exhibiting a needle) is what I have been in the habit of using, instead of the long needle to which Professor Pirrie has alluded. I was perfectly astonished by *the way in which,* some days ago, a large vessel was at once secured, and there

was not a drop of blood lost. What I usually do is, to leave open the wound on which we are operating. (Some of our brethern may not be aware that we operate here at a much earlier hour than they do in London—we operate at 10 a.m.) Having secured all the needles, we go back about six o'clock in the evening to remove the needles and bring the lips of the wound together into perfect apposition. Union of the most satisfactory character takes place. In one instance, there was not the slightest appearance of a drop of pus; and in others it was very slight indeed. Speaking for myself, I may say that, in using this method, I see nothing left to be desired in the arresting of hemorrhage. The more I am acquainted with it, the more satisfied I am concerning it. In one case, a very interesting one, in which I employed acupressure in amputation of the thigh, on removing the needle, a large dash of arterial blood came, and we were all under the impression for a short time that it had not sealed the femoral artery.

Dr. GEOGHEGAN: After how many hours?

Dr. MACNAMARA: The needle had been in for forty-eight hours. But a little pressure on the femoral artery stopped the blood. I think that there must have been some twist in the wire, and I tore across another vessel. But it is perfectly idle to imagine that, if the blood came from the femoral artery, a little manual compression would have resulted in sealing it up. Two months have now elapsed since the operation. I cannot too strongly express my opinion as to the value of acupressure in arresting hemorrhage. If I were in Edinburgh, I should hold my tongue; but I should be sorry that the distinguished suggester of acupressure should be in this city, and that I, so thoroughly acquainted with its value, should sit silent while the subject was being discussed.

Sir JAMES SIMPSON, who was received with applause, said: Perhaps I have no right to speak on a section of surgery as I am not a surgeon, but I shall feel obliged if you will allow me to say two or three words. Let me first remark that, after I had worked out some problems with regard to acupressure, I happened to show the result to a lady who was a little skilled in anatomy as well as everything else; and she said, "Now, Doctor, do you want me to believe that nobody ever thought of that simple thing before?" I said that I had searched all the olden and modern authors, and, to my own wonder, in none of them had I found any allusion to it. Let me add that I have searched them honestly. My friend Professor Smith stated in his address to-day, as I am told, that he has found a passage in John de Vigo describing the process. I know that passage very well; and I think that my friend, Sir Dominic Corrigan, will have to change our knowledge of the Latin language before Dr. Smith will be able to prove that it relates to acupressure. Dr. Smith has committed merely an error in reading the Latin of de Vigo. It has been translated more than once. It was translated from John de Vigo's book into English three centuries ago, by Trahernon, and into French by Portal. Both of these translations make Vigo say that he put a needle below the artery in order to pass and place with it the thread which was to tie and compress the vessels; and you must remember that, in the days of Paré and before him, the common way was by putting a needle around the artery altogether in order to place the thread around the vessel. De Vigo speaks of the process as one of *ligatio* (deligation); and distinctly, I think, states the thread to be left and used for tying—not the needle. But it is, after all, not a matter of any practical moment, whether the needle was ever used or not by any old authority. Let me say one thing more: that it is not seven years since I first offered the suggestion of acupressure publicly; and that I think I have some little right to rejoice that, in the course of those seven short years, instead of seeing the practice expire, as so many prophesied that it would, it has already extended very widely; and I have no doubt that in the hands of such very able surgeons as Drs. Pirrie, Buchanan, Porter, Macnamara and others, it will rapidly enough extend. There is no use in its spreading too quickly; let it take good hold. I quite agree in the belief that eventually we shall probably come to have acupressure done solely or almost solely with the pin, and without the wire loop. There are various

other modes of acupressure besides those described by Dr. Pirrie, in common use in Edinburgh. One, done in a moment, consists of making over or vertical twist with the single pin instead of a circular twist. I say that acupressure will probably be yet effected perfectly and entirely by a single pin; and then, as now, the rapidity with which the process can be employed will be found infinitely greater than that with which a ligature can be applied. Acupressure can be done by one man; whereas a ligature requires two. My friend, Dr. Davidson, practising surgery extensively in Madagascar, with no educated man near him to give him assistance in his operations, told me that he there got on very well with acupressure, whilst he could not have got on with the ligature in this isolated position. I may now speak of the time for removing the pin. I have seen it removed repeatedly from a breast, after amputation, within a few hours. I think there can be nothing more curious than that case of Dr. Keith's, which has been instanced, where the boy, whose thigh was amputated, removed the pin himself from his own femoral artery after four hours. Why, that boy was already one of the greatest surgeons alive, practising thus philosphically as he did on his own thigh and femoral artery. Dr. Pirrie referred to the results which may ensue from the pin being left in too long. My friend, Dr. Humphrey of Cambridge, one of the most accomplished surgeons living, in the last number of our journal spoke of its having been left for five days over a femoral artery. My wonder is that, when left so long there, it should not have had irritating results. It is against every rule that obtains in relation to the process to leave the pin inserted such a length of time. I published, at the suggestion of Mr. Compton, the rule that Dr. Pirrie speaks of, of not removing a needle while its knob throbs and moves; just as Homer describes the wounds of some of the Greeks, where the spear had been buried in the heart, that the weapon pulsated until the heart ceased to beat; so I believe that, in the case of the acupressure pin, while pulsation continues in the head of the pin, it, as a general but not absolute rule, perhaps should not be removed. With regard to the so-called methods of acupressure, termed numerically the first, the second, and the third, &c., methods, I believe we must classify them in some other way, as confusing the subject. Some time ago I saw a surgeon compress by two pins the humeral artery in a case of aneurism of the fore-arm. He inclosed, without any previous cutting, the humeral artery by a pin on each side of it, fixed the points in a cork, and then brought the heads together till all pulsation in the aneurism ceased. It may become yet a question whether all arteries in limbs could not be thus secured by needles alone, and without any previous use of the knife. In the case alluded to, the needles compressed the humeral nerve as well as the artery, and with what result? The woman before the operation was lying moaning with pain, apparently from pulsatory distention. The moment the pressure was effected, she said that the pain was gone; so that, instead of the compression of the nerve having given increased pain, it seemed to relieve the pain at once. A gentleman from India who, I was told, was to be at this meeting of the Association, has, I believe, done in various instances what I proposed to do long ago in the matter. Having no assistants with him, he has repeatedly placed the pin over the femoral artery when he was going to amputate the thigh, using it thus as a tourniquet. He always took it out afterwards. There I think he was wrong. I do not think surgeons have yet to learn to put a large needle over the femoral or other artery, and then by leaving it, the needle would become both a tourniquet and a ligature to the principal artery, and thus make surgery comparatively bloodless if it succeed. Remarks have been made as to the effects of acupressure in promoting the entire primary union of the wound. We are collected in a section of surgery, and I would like to ask any surgeon here, whether, in any hospital with which he is connected, or in his private practice, he has ever seen a single case of entire primary union taking place in any surgical wound where a ligature was used? Dr. Pirrie says he does not believe any man ever did see such a case; and I scarcely think it possible in the case where ligatures are employed. I have once or twice nearly got into a scrape on the subject with some gentlemen from North America (who often try to

take their fun out of you); for, when they came to speak about it, I have said that I knew, of a perfect certainty, that when the Northern surgeons cut off the limbs of poor Southern soldiers, they always put five or six little morsels of dead flesh into the sides of every amputation wound. But what was the explanation? That all of them used ligatures, and each ligature necessarily strangles a small morsel of flesh; and with five or six bits, however small, of dead septic decomposing animal tissue in the wound, no surgeon in his senses would expect it to heal by complete primary union. Dr. Pirrie mentions that he has, since using acupressure, amputated twelve mammary tumours, some of them of large size. Each of these twelve cases of amputation of the breast recovered by entire primary union. Now mark what Dr. Pirrie speaks of as "entire primary union." In some of his first amputations of the thigh, he says with respect to one, that he had only half a tea-spoonful of pus; but the presence of pus at all prevented such cases from being looked on as being cases of *entire* primary union. The surgeons of Aberdeen are all agreed, and the surgeons of all hospitals should agree, that no case can be called a case of complete primary union where a single drop of pus appears; and, in eight out of Professor Pirrie's twelve cases of amputation of the breast, there was not a single drop of pus. The primary union was entire, and effected at once. Surely this is a great march forward in surgery. I believe that, up to the time when acupressure has been used, no man in any hospital, either in Dublin, London, Edinburgh, or elsewhere, has observed similar results with the ligature. If Dr. Hingston from Montreal is here, he will tell you that I showed him in Edinburgh, a month ago, two or three cases of removal of mammary tumors in which acupressure was employed by me, and in which there was not a single drop of pus. If surgeons are strangely apathetic as to the desirability of attaining such attainable results, patients are not equally so. In one of these cases, in which it was deemed necessary to remove the breast, more than one leading surgeon saw the case; and a most skilful and excellent surgeon was selected to perform the operation. After it, six or seven weeks of daily dressing of the wound ensued. The patient told me that at last this daily dressing and irritation became so frightful a process to her, that she used to shake when she heard the surgeon's carriage stop at her door. In three months, the tumour grew again; and no persuasions of Dr. Wood or of mine could induce her to submit to the same process again. Eventually, however, I undertook to remove the renewed growth again without ligature or dressing; for there is no dressing when acupressure is used. Except some stitches with iron wire, no kind of dressing was used. Not a drop of pus followed; and the disease has not returned, though eighteen months have elapsed since this second operation. The very irritation of the dressing the patient argued, speedily brought it back the first time; and I wish this section could hear her dilate upon the professional iniquity and cruelty of dressings, when none such are necessarily required. I was lately told by a medical friend of a case of a gentleman who had a tumour some time ago removed in Edinburgh, and who, after being operated on, was weeks in getting well. After returning home, he happened to get hold of Dr. Pirrie's book; and, after reading it, angrily argued with his ordinary attendant, my informant, "Why was I tortured for six weeks to please old surgical prejudices? Dr. Pirrie might have cured me in a day or two." Now, speaking of complete union, let me add that, in the hospitals both of Paris, London, Edinburgh, and Glasgow, there have been tried of late years various materials as local applications to raw surfaces, for the purpose of producing primary union. The material has varied from turpentine and alcohol to iodine and chloride of zinc. The material latterly used has been carbolic acid. One of the chief reasons for recommending it and others has been, that there may be sporules, germs, or fungi floating in the atmosphere, which, if allowed access to the wound, would cause irritation and suppuration. No one has yet, I believe, seen these mythical fungi with the eye or the microscope; but carbolic acid and other chemicals are believed to have the power of destroying them, and of preventing the decomposition of blood, &c., upon the surfaces of wounds. Some years ago, Dr. Küchenmeister (see *Sydenham Society's Year Book* for

1860) wrote on carbolic acid as a disinfectant useful in surgical practice, and fitted for arresting putrefaction and preventing the development of such fungi. My colleague, Professor Spence, tells me that, three or four years ago, he washed the surfaces of some stumps with a solution of it, but soon abandoned it as useless. Within the last few months, the practice has been revived by Professor Lister of Glasgow, as quite novel; for there are more strange errors in the neglect of antecedents in modern than even in ancient surgery. I got a portion of the skin of the back of my hand scorched off by a diluted application of it, and the sore suppurated more or less for four weeks. It may or may not turn out, when more diluted, an useful application, and an aid to acupressure; but Professor Pirrie has shown that acupressure by itself, and without any dressings or medicated applications, can secure complete primary union for the surgeon. Surgery cannot gainsay such evidence as his. Why should it longer resist it?

Dr. PIRRIE afterwards observed: It is only since the adoption of acupressure that union without any suppuration has been seen where means were required to arrest the hemorrhage. With regard to the kind of needles best suited for acupressure, I, in common with others, have found the common sewing-needle with a round point a powerless instrument and with difficulty inserted into the tissues. Pins, where admissible, are greatly to be preferred. The pins I have been in the habit of using are those I now show you. They are made by Messrs. Weiss, of London, and are of four sizes, as you here see. My colleague, Dr. Keith, frequently employs the pins I now hold in my fingers. They are made by Messrs. Turner, of Birmingham. Spear-pointed pins are objectionable, as they are apt to cut the tissues; those with glover's points are greatly to be preferred. In every case where union by the first intention is possible, Dr. Keith, Dr. Fiddes, and myself, practice acupressure, both in hospital and in private practice; and the appliances you now see, are those which we employ. I am sorry I cannot give reliable guidance as to the shortest possible time at which it is safe to relieve vessels from acupressure. The practice at the Aberdeen Hospital for some time has been to remove the compression from the larger arteries in forty-eight hours, and from the smaller in twenty-four hours or in much shorter time. It is unquestionably safe at these periods; but I have no doubt that future experience will show that the early friends of acupressure have been over-cautious in removing the compression, and that they thereby lost some of the advantages of its use. In one case I relieved the axillary artery from acupressure in fifteen hours; the spermatic arteries in two hours and a half in another case; and in an excision of the mamma, three weeks ago, the whole of the vessels were relieved from acupressure in three hours after operation. The two first-mentioned cases have already been published. The most remarkable case I know of the early discontinuance of acupressure is that of a boy, whose thigh was amputated by Dr. Keith. Four hours after operation the boy removed three of the pins, and one of them was that by which the femoral artery was acupressed, yet no hemorrhage followed. I was much gratified by the remarks one gentleman made, expressive of his surprise at the amazingly short time in which he had seen an artery acupressed by means of the method by the twist. It is not only the simplest, the easiest of performance, but the quickest of all methods for arresting hemorrhage, and it has been proved to be perfectly efficient. I cannot forget the impression produced in the theatre of the Aberdeen hospital the first time I performed acupressure by the twist. This method was suggested to me by the late Dr. Knowles, and the first time I tried it was to acupress the femoral artery in one of my amputations of the thigh. The students had not heard of this method, and they were perfectly surprised, and could not understand by what means the bleeding from the femoral artery was so quickly arrested. In situations, where admissable, no better method need be desired. In illustration of the happy results of acupressure, aided by perfect immunity from motion and the avoidance of all dressings, I hope I may be pardoned for stating briefly the outline of one of my cases, although already published elsewhere. At the Aberdeen hospital I performed amputation of thigh in a delicate looking boy, six years of age, on account of exten-

sive disease of the knee-joint and the condition of the leg. The femoral and other two arteries were acupressed by the Aberdeen method, by the twist, and another vessel was compressed by the fourth method. The pins were withdrawn in forty-four hours, without being followed by a drop of blood, and, the patient said, without causing any pain. After operation I thought it necessary to caution the little patient not to touch the acupressure pins, which he called the pins with the beautiful heads, and promised to give them to him after their removal. He took care not to touch them, or to allow any person except myself to do so, and, to his great delight, sent them home by his father for preservation until his return. Since the operation, the little boy has slept as well as ever he did in his life; has been anxious for his food; has had no uneasy sensation of any kind, and has been in the highest of spirits. The stump healed perfectly and throughout, either by immediate union or by primary adhesion. The healing must have been by one or other of these processes; and I am inclined to think it must be by the former, as no medium of union is discoverable at the edges of the wound. No dressings have been employed, or retentive appliances of any kind, except the few sutures and the three strips of isinglass plaster which were applied before the patient was taken from the operation table. The sutures were removed on the fourth day. This little patient has a fine musical ear; and a few days after the operation, when I entered the ward with the surgical pupils of the hospital, he was whistling most beautifully "The Braes o' Mar," and amusing himself by passing a little model carriage over an inclined plane which he had got constructed on his bed. He continued to whistle the above and other airs until it was his turn to be visited. This is one of the many instances we have lately seen in the Aberdeen hospital of a capital operation, and after treatment having been made delightful to the patient as well as to the surgeon. By the aids of chloroform, acupressure, no dressings, and the most perfect immunity attainable from every movement of the stump, many of the hindrances to these gratifying surgical scenes are removed. Since the use of dressings was discontinued in the Aberdeen hospital, patients have looked forward to the visit of their surgeon as the happiest event of the day, instead of, as formerly, dreading the visit, and being thankful when it was over.—*British Medical Journal, Aug.* 31, 1867, *p.* 171.

65.—ON A NEW METHOD OF CLOSING BLEEDING VESSELS BY MOVEABLE FORCEPS.

By Thomas Nunneley, Esq., Surgeon to the Leeds General Infirmary.

[Mr. Nunneley has not been satisfied with the results of acupressure. The difficulties of effectually closing the blood-vessel are in his judgment so great as to counterbalance the undoubtedly great advantage of having the wound free from the long continued presence of a foreign substance.]

Though Drs. Pirrie and Keith assure me that in their hands no case of hemorrhage, either primary or secondary, has happened; and that all, even large amputations, have not only recovered, but have healed by the first intention, without sloughing, pyæmia, erysipelas, or other misadventure; the results have been far different in other hospitals and in other hands, where some of the stumps have not only had to be opened out to more effectually secure bleeding vessels (which, it might perhaps be asserted, happened to be so by the want of skill or care on the part of the operator), but where the withdrawal of the pins has been followed by hemorrhage so copious as to prove that no permanent closure of the vessel had been effected; while union by the first intention, which is the great desideratum, has not only not been obtained, but as much suppuration and delay in healing by granulation has taken place, as in the ordinary method; gangrene of the stump has occurred, and pyæmia and erysipelas have been set up. I must confess to my experience more nearly corresponding with the latter results, than with those of Drs. Pirrie, Fiddes, and Keith. I, therefore, infer that the great success which, I am assured, has been experi-

enced in Aberdeen, is dependent upon some other influence than acupressure alone; indeed, I cannot but feel too much is proved by the reported enormous success, which is too great to arise solely from the employment of acupressure. However advantageous it may be, it alone could not exempt large operations from danger, and ensure uniform recovery. Some other accidental, overlooked, and unknown, but most potent influences, must have been at work. As there are seasons of great want of success, in which, from one cause or other, often so subtle as to be unknown, almost all wounds, however unimportant, do badly, so there are other times when all, however serious, do well. I cannot but suppose that at least part of the Aberdeen success must be attributed to such a condition rather than to the use of acupressure alone.

Immediately after Sir James Simpson's introduction of acupressure, I had correspondence with him on the subject; and he has favoured me with letters from time to time since then; so that my attention has been constantly directed to the subject. My first idea was that, if the fibrous material were the cause of pyæmia and other calamities, as Sir James Simpson has emphatically declared, the use of metallic ligatures would at once obviate the mischief; and as animal bodies are more tolerant perhaps of iron than of any other of the metals, I got iron wire drawn of various degrees of fineness, even up to what was presumed to be No. 45 (for no gauge is made nearly so fine), and, by careful annealing, managed to obtain it as flexible as common thread or silk, without rendering it brittle, so that it will draw into a double knot as easily as they will. With this, in amputations, I tied many blood-vessels. In two cases of amputation of the thigh, the femoral arteries were secured by the wire; I found it to cut well through the outer arterial coats, and to hold firmly; but, while it thus proved effective as a ligature, I did not perceive any material advantage in the healing of the wounds, and it was accompanied by a very important disadvantage, for the very property of being so tolerated by the system without exciting irritation, which gives the metal so great a superiority over fibrous material for sutures, is attended by a corresponding disadvantage where it is used as a ligature on vessels; for instead of separating within the usual period required for the coming away of thread-ligatures, the iron-ligatures would remain on for an indefinite period. Some of them I was obliged to cut off as close as I could and leave on the vessel, so that a complete cure was unduly delayed; and I have almost given up the plan in consequence.

More recently it has suggested itself to me that, if I could contrive self-acting forceps sufficiently fine in the stem not to keep open the wounded parts, sufficiently strong to keep the divided vessel perfectly closed, and yet not so powerful as, by their pressure, to cause sloughing or ulceration of the included vessel, but merely adhesion of its coats and closure of its mouth, firm enough to remain securely on the part to which they are applied, and yet capable of being withdrawn at any desired period without in any way disturbing the wound, or to interrupt the desired union by the first intention, the hemostatic power wanted would be obtained.

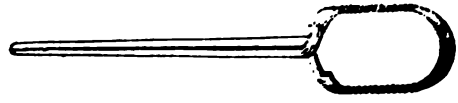
After a trial of various forms, I think that I have succeeded in devising a form which will supply all the required essentials. I need not detail the different forms of compressing power which I have tried, nor the defects of one kind or other that I have found in them; but simply give a sketch of that one which, after sufficient trial, I have found to answer. As the vessels lie at different depths from the surface and are of different sizes, the forceps must be of different lengths and strengths. Obviously, to reach and hold the femoral artery, a larger and stronger instrument must be employed than that which is required to compress a cutaneous and subcutaneous branch, and so for vessels of intermediate size and position; but, I think, four, or at most five, sizes, will be found sufficient for all purposes. They should range from three inches to three-quarters or half an inch long. While the latter will be adapted for small superficial vessels, the larger will be large enough for the femoral or iliac arteries, even in stout men. The points of the forceps should *just embrace the* vessel, while the bowed shoulder should just project out of

the wound; a greater length would be useless and inconvenient by being in the way, while it is necessary that the bow should project from the skin, or it will keep the lips of the wound apart, and not allow of the forceps being readily removed from the vessels, and so, in the attempt to do this, possibly, by disturbance of the vessel, cause hemorrhage, or arrest the healing process in the wound. The forceps are self-holding; to remove them, it is only necessary to press upon the bow. This may be made broad, rather flattened at the sides, and, if desired, somewhat roughed, so as to afford a firmer hold; while the blades should be made as fine, and lie as close together, as is compatible with the necessary strength. This, however, need not be great, for very little power is necessary to effectually close the mouths of even large arteries, provided it is only uniform and regular in application; a certain strength is more requisite for giving firmness and stability to the forceps than for making great pressure on the blood-vessels. The points of the blades should be roughened, as in the ordinary dissecting forceps, so that they may hold better. I had at first small teeth made so as to seize the vessel, but this I find to be unnecessary in ordinary cases; but perhaps, where a large number of vessels have to be secured, and much handling and disturbance of a stump is required before all is made safe, the small teeth would cause the forceps to be less liable to be displaced while this is going on. So soon as the surfaces are brought together, there can be no danger of disturbance. Hitherto I have had them made of steel by a cutler, which, of course, is somewhat costly at the first; but as they can, after well washing, be used any number of times, this result is not a consideration, and if they had a thin deposit of gold upon them all fear of rust, &c., would be removed. I am by no means sure that, for the short time the forceps remain on the vessel, steel is necessary; I think it probable that manufacturers of the safety-pin, or like handicraft workmen, may be able to supply a forceps sufficient for all purposes at so small a price, as to allow it to be used without any consideration of economy. This I am taking steps to ascertain. At one time I thought of employing forceps whose blades are brought together by a screw motion or by a sliding tube; the latter idea had also occurred to Mr. Birkett, as he told me the other day when we accidentally met on a steamboat on Loch Lomond; but, like him, I abandoned it, for I found it had disadvantages, and, moreover, would be very costly. Doubtless other forms will suggest themselves to other persons, and modifications may be made; whatever may most effectually secure the requisites I have named will be so much gained.

From small arteries I have removed the forceps in from twelve to twenty-four hours, but, in the larger, twenty-four to forty-eight hours should be allowed to elapse before they are taken away, by which time, I apprehend, the largest artery may be regarded as effectually sealed, though, if thought requisite, I see no reason why they may not be left *in situ* for a longer period. Hitherto all the cases in which the forceps have been employed have done remarkably well, but a longer time and more experience will be required before we can dogmatise upon how much is due to the employment of the forceps, and how much to other circumstances.

I think it by no means improbable that, in some cases of aneurism, compression, by similar forceps, of the artery, on some point of its course above the sac, for a greater or less length of time may be successful. The sheath of the vessels should be merely punctured to an extent sufficient to allow of the

FIG. 1.



Forceps with roughened points.

forceps being glided upon the artery alone, no other part being included or disturbed. If, after a sufficient trial of the forceps on divided vessels, I find the result as satisfactory as I anticipate, I shall certainly feel disposed to use them on an aneurismal artery.

I will just add another word. After the wound is properly brought together, the forceps are very much less likely to be disturbed in the necessary or accidental movements of a patient than are the needles in acupressure; that no other tissue than the vessel itself is compressed, as it necessarily is in that process; and that, if venous hemorrhage be troublesome, there is no reason why the forceps should not be applied to the open vein as well as to a bleeding artery, for the effects would not be like those to be feared from a ligature, and the forceps may be removed on the completion of the operation or very shortly afterwards.

I append a sketch of the forceps, which sufficiently explains their construction, only pointing out that the compressing power is obtained by the cross joint, as in the little bull-dog forceps of Liston; and, if more power be desired than the simple bow gives, any amount of pressure may be obtained by giving it a second or third turn, making the turns larger, but this I apprehend will not often be required. To make security doubly sure: if any one feels any doubt as to all fear of hemorrhage being removed by giving the forceps one or two turns after seizing the vessels and bringing them out of the wound, he will continue torsion with pressure.

As I have heard fears expressed lest the long legs of the forceps should give way, and allow the open vessel to escape and hemorrhage to occur—a fear, however, in which, if the forceps are properly constructed and applied, I do not participate—I have had another form of forceps made. These are shown in fig. 2. They are fine forceps, with long straight blades, on the upper part

Fig. 2.

Forceps with compressing clip: 1, a moveable clip, having a slot which slides upon 2, a projection of the rivet, which unites the two blades; 3, a roughened button upon the top of the clip; and 4, a fixed one upon the opposite side of the forceps, by which these can be more easily held, and the clip pushed down towards the points of the forceps.

of which is a sliding clip, which is to be pressed down as soon as the vessel is seized by the points. By this arrangement, any amount of pressure may be applied, and slipping is impossible. The clip in those shown in the figure is kept steady by a slot, which slides upon a projecting part of the rivet which unites the two blades; but I think this slot may be done away with, and the clip made simply to slide upon the two blades, by which arrangement there would be less chance of the forceps becoming fixed in the wound by the coagulated blood.—*British Medical Journal, Oct.* 12, 1867, *p.* 310.

66.—ON LIGATURE OF THE FEMORAL ARTERY ACCORDING TO THE PLAN OF THE LATE PROFESSOR PORTER.

By Dr. Rawdon Macnamara, Surgeon to the Meath Hospital, and Professor of Materia Medica in the Royal College of Surgeons of Ireland.

In Mr. Erichsen's work on *Surgery*, when speaking of the propriety of deligating the common femoral artery, after stating the objections to the procedure, he winds up with this remarkable passage: "This operation, I think, therefore, should be banished from surgery." Whether this sentence is as just as it is sweeping, is the object of the present communication. It is true, in the present position of surgery with respect to aneurism occurring in the popliteal space, that ligature of the femoral artery has been deprived of a great portion of its importance in consequence of the success attending its treatment by compression; and it is equally true, that the surgeons of this city have borne no small share in bringing about this most important improvement in practice. Still, cases will occasionally occur which are either *unsuited for treatment* by compression, or in which, no matter how dili-

gently or scientifically employed, it will fail in effecting a cure; and then our only resource is occlusion of the main vessel, either by the old-fashioned, time-honoured ligature, or by the more modern system, acupressure. When such cases present themselves, therefore, it is all-important that that form of operation should be selected which, beyond all other considerations, is attended with the minimum of risk to the patient's life, and which (a minor consideration, to be sure,) is easiest of execution by the surgeon. Should ever the operation, the merits of which I am about to bring under notice on the present occasion, be recognised as the proper one to be selected under circumstances calling for deligation of the femoral artery, it will be a subject of interest to look back and observe how, as the science of operative surgery progressed, the tendency has been to ascend higher in selecting the site for its deligation—from Hunter's canal to Scarpa's space, and now from Scarpa's space to what, I doubt not, will yet be known as Porter's space.

The manner of securing the vessel in this situation will require but a few words to bring it in all its details before your mind's eye:—a transverse incision, about two inches in length, parallel with, but half an inch below, Poupart's ligament; the incision being so effected as to leave the artery in its centre. The method of performing this incision is simple in the extreme. A fold of skin is pinched up—the operator himself holding one side of the fold, his assistant the other; and it is transfixed with a sharp-pointed bistoury; and, by cutting outwards, the first incision will be completed. The most ordinary care will ensure the artery's being in some portion of the wound. A very few touches of the scalpel, with the assistance of a director, will now expose the vessel; and it can be secured in any way that the operator may select. The great advantage of this operation is that, in this situation, the artery is widely separated from its vein; and consequently there is no danger of wounding it. When I say "widely" separated, of course I speak in an anatomical sense; the space lying between them in reality being about two to four lines. All operating surgeons know the danger that exists of wounding the vein when either Hunter's or Scarpa's space is selected as the site of operation; and the records of surgery are not wanting in numerous cases where, in the most skilful hands, this accident has, I may almost say, unavoidably occurred. The lamentable results of such an accident as including the vein in the ligature require not to be dwelt upon here by me. All this is gotten rid of by selecting Porter's space. The vein cannot be wounded, much less included in the ligature; and thus deligation of the femoral artery is happily freed from one of its most serious risks. The facility with which the artery is reached also is a recommendation. After the first incision, a very few and very trifling touches of the scalpel bring us down upon it; in fact, the surgeon who performs this operation for the first time will be surprised at the facility with which he will in this situation reach the artery. As to the vein, he need have no apprehension about it: it is far out of his way; and, unless by the most stupid bungling, he cannot in any way injure it. As to hemorrhage, in all the cases which I have seen, the operation was all but a bloodless one; no vessel being wounded; no ligature but that for the femoral itself being called into requisition. As to the close proximity of the profunda interfering with the formation of a coagulum, theory is one thing, facts another; and as long, but no longer, as theory harmonises with facts, I will accept the theory; but when theory is unsupported by facts, I most respectfully decline to accept the theory, no matter from what quarter it may emanate; and, upon this point, I shall presently direct attention to the facts of the operation, as observed in this city. It has been objected also to this operation, that tying the artery above the profunda cuts off the supply of blood from the limb—a circumstance that would militate against a successful result; but deligation of the same artery a little higher up (for really the external iliac may be so considered) is obnoxious to the same charge, and yet this operation is one of the most successful amongst our greater surgical procedures. But, fortunately, theory in this instance harmonises with facts; for anatomy teaches us that, if the femoral artery be tied above the profunda, the current of blood would find its way through the anastomoses existing

between the obturator and the internal circumflex arteries; the ilio-lumbar, with the circumflex ilii; the gluteal, sciatic, and pudic, with the internal and external circumflex, and with the perforating branch of the profunda; and, as I trust to prove to your satisfaction, in practice these anastomoses are found to be quite sufficient to carry on the circulation efficiently.

I shall now proceed to bring this operation, in its relation to surgical practice, under your consideration. It has already been performed eight times in this city—three times by the late Professor Porter, once by the late Mr. Smyly, once by Mr. Butcher, once by Mr. Porter, once by Mr. Collis, and once by myself. Mr. Butcher's case was fatal, as was also Mr. Collis's; all the rest made good recoveries, and, so far as I know, are still alive. So that out of eight cases operated upon in Porter's space we have six recoveries to two deaths—a sufficiently respectable result for an operation that should be banished from surgery! But, if we analyse the two unsuccessful cases, the results will be still more favourable. In Mr. Collis's case, as you will perceive from this morbid specimen, a most unfortunate anomaly exists in respect to the giving off of the profunda—in detaching itself from the femoral at the very point where the ligature was applied. In Mr. Butcher's case the operation was performed for a wound of the profunda artery, in which, after most persevering exertions, he was unable to reach the wounded vessel itself and consequently he ligatured the femoral in Porter's space; and, although the patient died in eleven hours after the operation, it, so far as operative interference goes, has a just right to be looked upon as a successful case, inasmuch as the patient lost no more blood after Mr. Butcher tied the femoral, the poor man having evidently sunk from the effects of all the blood he had lost previously to Mr. Butcher's seeing him. At all events, it has no right to be considered one way or other in calculating the statistics of this mode of procedure.—*British Medical Journal, Oct.* 5, 1867, *p.* 258.

67.—ON THE PATHOLOGY AND TREATMENT OF NÆVUS.

By Thos. Smith, Esq., Assistant Surgeon to St. Bartholomew's and the Children's Hospitals.

A nævus is either a separate growth formed of blood-vessels; or an area where blood-vessels so predominate over the natural tissues of the part as to communicate to it an abnormal and permanent vascular stain.

Nævi may be composed chiefly or entirely of arteries, chiefly or entirely of veins, or of capillaries; being on this account termed respectively "arterial," "venous," or "capillary." The microscopic structure of a venous nævus—the ordinary kind that belongs to childhood—may be seen in the specimens under the microscope on the table. The growth is formed of a congeries of veins and venous capillaries running a most tortuous and irregular course in loops and tufts; the whole growth being generally surrounded by a capsule of areolar tissue. The blood to a growth of this kind is supplied by one or more large veins which may be called afferent, and is carried off by one or more efferent veins; so that the vascular system of such a nævus is just after the plan of the portal circulation, being carried on by two sets of veins with intermediate venous capillaries. This is the structure of a subcutaneous venous nævus—the only kind of child's nævus that I have had an opportunity of examining after its removal from the body.

Nævi may occur in almost every part and in almost every tissue of the body; having been found in the liver, lungs, bones, and tongue, as well as in their more common seats, the integument and subcutaneous cellular tissue. M. Guersant relates a case where, in one child, he found the disease in the lungs and liver, besides over a considerable portion of the head and face.

For practical purposes, and since nævi are generally situated either on or in the common integument, these growths are often classified as cutaneous, subcutaneous, and mixed; the last being partly in the skin and partly *under it.*

There is no part of the external surface that is exempt from a liability to

nævus, though certain localities show a greater tendency to the disease than others. Thus, the hairy scalp and face are, perhaps, the chosen seats of the affection, next the trunk, and lastly, the extremities. They may, however, be met with in the vagina, in the rectum, or in the interior of the mouth; on the gums, the tongue, and on the conjunctiva. But in these cases they are, for the most part, in connexion with, and are prolongations from, nervous growths affecting the skin in the immediate neighbourhood.

In looking over the cases that I have noted as occurring in the out-patient room, I find a singular predominance of females over males in those suffering from nævi. Since the month of July, 1861, I have seen 140 cases of nævus, of which I have kept some record: 95 were females, 42 were males, and in 3 the sex is not stated. This difference is too considerable to be accidental, especially when the aggregate number is taken into consideration; we may, therefore, safely conclude that female infants are far more liable to the disease than males, though I confess I was quite unaware of this circumstance until I thus reckoned up some of my own cases.

In referring to the *symptoms* of nævus, one may shortly say of the *arterial*, that the only symptom distinctive of this variety is pulsation; if any nævus does not pulsate in a living child it is not arterial. These nævi are but rarely seen in adults, and are scarcely ever met with in children; it has never fallen to my lot to see one in a child.

Subcutaneous venous nævus is generally a lobular, puffy swelling, about the consistence of a fatty tumour, soft and very slowly elastic; that is, if compressed firmly, it collapses considerably, and but slowly regains its shape and consistence on pressure being relaxed. It is situated at various depths in the subcutaneous tissue; if very near the skin, it generally communicates a pale bluish tinge to the surface.

Here is a little girl, about two years old, with a subcutaneous nævus; on the side of the trunk it extends from the navel to the spine in one direction, and from the ribs to the crest of the ilium and Poupart's ligament in another. It is seemingly two or three inches thick, and is as large as a good-sized plate. Until lately it was unconnected with the skin, which, as you see, is tinged of a bluish purple colour. This growth is now undergoing spontaneous cure, and the skin is dimpling down over it and forming connexions to it as the vascular tissue beneath shrivels up.

These subcutaneous nævi may be confounded with cystic, glandular, and fatty tumours, and lastly, and most fatally, with meningocele. The diagnosis between subcutaneous nævi and congenital cystic tumours, in doubtful cases, may be facilitated, and the ignominious, though sometimes necessary, resort to a grooved needle may be avoided, by observing the effect of firmly grasping the tumour, a proceeding that much diminishes the size of a nævus, while it produces no impression on a cystic tumour; and this manœuvre is often absolutely necessary in order to distinguish between these tumours, since the blueness of deep vascularity over a subcutaneous nævus almost exactly resembles the blueness of translucency seen over congenital cystic growths.

Large subcutaneous nævi, when deeply situated in the sub-maxillary region, bear some resemblance to glandular tumours, and mistakes of this kind have been committed. Moreau relates the case of a surgeon, an author on nævi, who made this mistake. In this case, also, the employment of pressure would be a valuable means of settling the question; while the existence of any vascular tinting of the skin over the part, or the presence of nævi elsewhere on the body, would be almost decisive in favour of the growth being nævus in structure, in a doubtful case. The same may be said of the diagnosis between nævi and fatty tumours, though the latter are comparatively rare in infants.

The following case, given by M. Guersant, illustrates the fatal resemblance the subcutaneous nævus may bear to meningocele:

A child had on the internal angle of the right orbit a small swelling, which bore all the characters of a subcutaneous erectile tumour. Many Fellows of the Surgical Society of Paris saw this case, and agreed with M. Guersant that the tumour should be treated by thread setons. Shortly after this had been

carried into effect, the child was seized with cerebral symptoms, and quickly died.

On post-mortem examination, the growth was found to be an encephalocele; the sac of which, the size of a pea, passed through the fronto-ethmoidal suture, and protruded at the inner angle of the orbit. The protrusion was formed of a small portion of cerebral substance, covered by the membranes of the brain.

Such a tumour I myself have seen on the bridge of the nose in an infant. The real nature of this swelling was only decided by a puncture with a grooved needle—a proceeding which proved it to be a meningocele, that must have protruded through the fronto-ethmoidal suture. This conjecture has not, I am happy to say, been either confirmed or refuted by post-mortem examination, since the child is still alive. In dealing with nævi in connexion with the bone at the inner angle of the orbit, it is always prudent to bear such facts as these in mind. And though I confess that in some cases it is exceedingly difficult to arrive at a clear and certain diagnosis, yet I believe that a searching investigation of the surface of the skull beneath the tumour by firm pressure will generally detect some chink in the bones if the tumour be of cerebral origin. In any case of doubtful identity, the existence of nævous growths on other parts of the body will be a strong ground for suspecting the disease to be of a similar character; while a small puncture with a fine grooved needle will always settle the question without endangering the life of the patient.

Cutaneous nævi are those that involve the skin to a greater or less extent without extending into the subcutaneous tissue. The vessels composing the disease are mostly very small veins or capillaries, and they derive their vascular supply almost entirely from the vessels of the cutis—i. e., a lateral and superficial source; they may simply stain the integument in varying degrees of vascular intensity, or the proper texture of the skin may be destroyed, being replaced by nævous tissue.

It is necessary for practical purposes to observe other distinctions—namely, that cutaneous nævi may be flat, on a level with the surface of the skin, or may protrude beyond it; that they may be definitely circumscribed, or may be diffuse, or spotted about the surface of the skin; again, they may be pendulous. Here are examples of each of the different forms of cutaneous nævus that it is necessary to recognise for therapeutical purposes.

There can be no difficulty in the diagnosis of this species of nævus; the appearance of the staining of the skin is characteristic, though care is well bestowed in ascertaining the exact limits of the disease, and the degree to which the structure of the skin is implicated. The colour of these nævi may vary from the slightest blush, through tints of lividity and crimson up to the brightest scarlet.

Mixed nævi, as they are called, are by far the most common variety of the disease. The bulk of the growth, in this case, lies in the subcutaneous tissue, while more or less of it involves the skin, either replacing the latter or staining it in various degrees of intensity. They derive their supply of blood both from the vessels of the skin and those of the subcutaneous tissue. These nævi, like the cutaneous, may be circumscribed; or if diffuse, they may either gradually and imperceptibly fade away and blend with the natural textures, or they may at their margins pass into spots and patches of nævous tissue, cropping up here and there on the surface of the skin, from out of the subcutaneous tissue.

Venous nævi, of whatever kind, are generally congenital, being usually noticed at birth as small specks, spots, or lumps, beginning to enlarge rapidly a week or two after birth. Their tendency is to continued increase for an uncertain time, and before the age of puberty to disappear. This is especially true of nævous *tumours* (that is, the subcutaneous and mixed nævi), as distinguished from the strictly cutaneous variety. Cutaneous nævi are too apt to remain throughout life as permanent disfigurements, though they may fade somewhat in the intensity of their colour. In support of my statement, I refer to the opinion of some who have had most experience in this disease, such

as Guersant and Moreau. Mr. Syme also entertains and has expressed decided opinions in favour of this view. The rarity of any other form of venous nævus than the cutaneous in adults is testimony in favour of it; while one's personal experience and published records furnish many instances of spontaneous disappearance of nævi when the period of childhood has been passed. Moreau mentions three cases where nævi condemned to operation by Dupuytren recovered spontaneously. Guersant relates an instance where several nævi on an infant disappeared in the same way. Bouchet gives another such case.

Here is the child I showed to you before with this large subcutaneous nævus on the abdomen undergoing a spontaneous cure. The manner in which a spontaneous cure may come about is either by simple shrivelling, by inflammation, by ulceration of the surface, or by degeneration of the nævus into cysts. Deep subcutaneous nævi are, of all others, the most likely to undergo shrivelling and cystic degeneration—a process that has been well described by Mr. Coote.

The florid cutaneous nævi that entirely destroy and replace the natural texture of the skin, not rarely ulcerate, either spontaneously or as the result of blows, or from the friction of the child's clothing. Thus the natural history, as we may term it, of nævous formations is to begin life with the individual, to grow actively for a time, and, having lived its term, to degenerate or wither, and disappear.

The period of infancy seems the most favourable to the development of these growths. There are, however, two other epochs when already existing nævi may start into active growth, or fresh nævi may form: these are the commencement of puberty in either sex, and the period of pregnancy in women. The nævi that appear at these times are generally small and cutaneous, and are likely to disappear with the peculiar constitutional condition that favoured their formation.

[The natural tendency of all nævi when left quite alone is to degenerate and disappear. Why then interfere by treatment? For many reasons, because it is an unsightly disfigurement, especially upon the face; it is liable to accidents; by the time it has disappeared much of the skin may be spoiled by puckering in of the cutaneous surface, and lastly, because nævi in infants are generally small, and easily and safely cured.]

Before considering the subject of treatment, let me say a few words on the kind of nævi most likely to undergo a spontaneous cure, and that within a reasonable limit of time. These are the large, unwieldy, and turgid nævi that are usually found about the axilla, or in the fat of the breast and the surface of the abdomen, and especially the enormous mixed nævi that sometimes grow about the parotid and submaxillary regions. Here are drawings of two very large growths of this kind, involving the parotid and submaxillary regions. Both of these disappeared spontaneously. Here is a drawing of a little girl who has been under my care with a nævus, the size of a cricket-ball, over the left breast. This sloughed and separated during an attack of scarlatina without causing hemorrhage or any untoward symptom.

The early signs of the probable retrocession of the disease are, a cessation of growth, an increase in solidity if it be subcutaneous, and a loss of brilliancy of colour. When the process has fairly commenced, light-coloured and dead-white spots appear on the surface, looking like the cicatrices of some caustic application; while the skin at these points is often slightly dimpled down and puckered in. Again, the occurrence of spontaneous ulceration over a cutaneous nævus may be regarded as warranting the expectation of a natural cure.

I will not weary you with a categorical description of the different means of treatment at our disposal, but will mention them in connexion with the varieties of the disease to which they are severally applicable.

Arterial, that is, a pulsating nævus, I have never seen in a child, and I much doubt if you will ever meet with the disease. Having no personal experience to guide me, I will not venture to attempt to teach you from the scanty materials afforded by others.

Treatment of cutaneous nævi.—For the more exact description of the

treatment suitable to these, I will divide them into (*a*) the prominent, (*b*) the flat, (either of these may be circumscribed or diffuse), and (*c*) the pedunculated.

(*a*) *Prominent cutaneous nævi* are generally very florid, and are entirely composed of vascular tissue, the proper texture of the skin being destroyed; there is often a considerable thickness of the diseased tissue, though it does not extend subcutaneously. They may be destroyed by caustic potash, Vienna paste, the strongest nitric acid, or by ligature, since there is no object in saving the deeply stained and morbid integument. It is advisable to select some plan of treatment that will in one application completely destroy the growth. If the disease, therefore, be circumscribed, circular or oval in shape, and situated on any part of the surface, except some parts of the face, you will find the ligature a speedy and certain method of cure. If the growth be of moderate size, say not larger than a penny, two harelip needles may be passed beneath it at right angles to one another, their orifices of entrance and exit being just where the nævus tissue joins the true skin. Beneath these a well-waxed and strong ligature of twine may be tied, being drawn so tight that the surface of the nævus assumes a ghastly pallid hue. The ends of the needles being cut off, the whole may be covered with a piece of cotton wool, and allowed to remain until the needles and nævus fall off; this generally occurs at the end of a week. The nævus should not be pricked to let out the blood while the ligature is being tightened, since this to a certain extent relieves the strangulation and diminishes the efficacy of the treatment. Should the nævus be too large to be included in one ligature, a needle may be passed beneath the base carrying a double thread, and thus the growth may be strangulated by two knots, each surrounding half the tumour. If the growth is still larger it may be tied in quarters, or in as many parts as may effectually arrest the circulation therein. The use of one or more harelip pins passed beneath the nævus is, of course, to ensure that the thread, in cutting its way through the tissues, may pass beneath the growth. There is a method of treating this variety of nævus, devised, I believe, by Mr. Gay: it consists in passing harelip pins through the sound skin beneath the growth, cutting the growth entirely out, and then applying the twisted suture over the ends of the pins, thus at once closing the wound and arresting hemorrhage. It is a plan which may occasionally be suitable, but when I have tried it I have generally found that the shape of the wound left after excising a nævus is such as to prevent the edges being closely and evenly approximated.

Prominent cutaneous nævi, when irregular in shape or ill defined in their boundaries, are best attacked by nitric acid, as this can be applied with a glass brush to the very smallest spots of diseased tissue without injuring the surrounding skin. The acid used should be the strongest that is made: the kind that is called "anhydrous" is best. It should be painted on with a glass brush, in a thin layer, until all appearance of vascular tissue has disappeared. The brush should never be so much saturated with acid that a drop could fall from it on to the neighbouring parts. Some carbonate of soda should be at hand in case of accident, and to apply to the nævus when the cauterisation is complete. Vienna paste or caustic potash is used by many for this kind of nævus, but I find it difficult to adapt them to the irregular edges and outlying portions of these growths.

(*b*) *Flat cutaneous nævi*—that is, such as do not project beyond the surface and have no depth of tissue—may, if circumscribed, be treated by vaccination, by nitric acid, Vienna paste, or the actual cautery. I should, if the child were unvaccinated, either inoculate the nævus in several parts with vaccine matter, or surround it with a circle of punctures. Though a necessary proceeding for the child, vaccination is a very uncertain remedy for nævus. If vaccination is inapplicable, nitric acid may be used. I prefer it to the other potential cauteries, as well as to the actual cautery. The latter I never use on account of the smell it occasions, and the horror it excites in a mother's mind; while its powers of destruction are in no way superior to those of the potential cauteries, and the inflammation it occasions is much greater.

The flat cutaneous nævi vary much in depth of structure and in intensity *of colour; they are sometimes* extremely superficial, and only affect the very

surface of the cutis, and do not extend into its substance. They might be called erythematous nævi, since they stain the surface like a blush. They are often spread over large surfaces. Here is a painting of one that affected the whole of one upper extremity: it was drawn from a little girl aged seven, under Mr. Holmes's care. I have only seen the erythematous nævi on the back of the hand, on the arm, the neck, and the face; but I know of no reason why they should not occur on any part of the external surface. Fortunately, this kind of nævus is peculiarly liable to disappear with advancing growth. Should it remain, however, and by its position cause any serious blemish, the surface may be frequently painted with collodion, or a small portion of the discoloured skin may be painted with blistering fluid to try its effect. It is to such a nævus as this, but more vivid in colour, that tattooing may be employed—a plan introduced and recommended by M. Paul de Landau. The skin should be stretched tightly between the fingers and thumb to control the hemorrhage, and, a multitude of small punctures being made with a common sewing needle, finely powdered oxide of zinc, or calcined magnesia, may be rubbed in; the process being repeated until the skin has regained its natural colour. I have never myself put this in practice, though it seems to be a promising plan of treatment in suitable cases.

The diffuse and spotted cutaneous nævi are best treated by nitric acid dotted on to the diseased points of skin.

Very minute cutaneous nævi may be cured by tightly stretching the skin, incising the spot with a small crucial cut, and touching it with nitrate of silver. I have usually washed the part afterwards with iodide of potassium in solution lest any oxide of silver should remain in the substance of the cutis. I dare say this is a needless precaution.

Subcutaneous nævus may be either (*a*) circumscribed, or (*b*) diffuse. (*a*) In this form of the disease, when the entire growth is situated beneath the skin, it is important to preserve the latter uninjured, and for this reason the following remedies are suitable:—The injection of coagulating fluids, such as solutions of perchloride of iron or tannic acid; the introduction of setons steeped in the perchloride solution; breaking up and introducing probes armed with nitrate of silver; or complete excision, may any of them be employed according to the size and situation of the nævus and the predilections of the surgeon. The most elegant method of cure is by the injection of solution of perchloride of iron, since it inflicts but a single puncture. It is difficult, however—first, to be certain that the fluid will flow into the nævus; and secondly, if it flows, to regulate the quantity. It may be used where it is of great importance to avoid all trace of treatment. It should not be employed to nævi about the head, face, orbit, and neck, except under conditions to which I will hereafter allude.

Perchloride of iron may more certainly, though less elegantly, be introduced into a nævus by drawing through the growth setons steeped in the solution. The thread employed should be darning cotton, on account of its absorbent properties; and the needle should be large in proportion to the thread, in order that the needle-hole in the cutis may allow the thread to pass through, without squeezing out the perchloride solution. In a circular nævus three, four, or more of these threads may be passed through the centre of the growth so as to form the radii of a circle. In an oblong or irregular-shaped nævus they may be drawn through side by side half an inch apart. While passing the threads, one finger may be kept lightly over the aperture of exit, and over the track of the thread, so as to squeeze out as much of the solution as may be thought fit. These setons may remain in for six or seven days. I usually remove them the day week after their introduction, and find that by that time a little suppuration in their tracks has occurred. In this way I am in the habit of treating a large number of nævi on all parts of the body, the head and face included.

Subcutaneous incision, so as to break up the nævous tissue is a good method of treatment for subcutaneous nævi: breaking up, together with the introduction of nitrate of silver, is a better; it turns the nævus into an abscess, and is an excellent *remedy for the* large subcutaneous nævi that occur about

the trunk. In this method the nævus is thoroughly stirred and cut up with a long tenotomy knife introduced through the sound skin at the side. If nitrate of silver be not employed, pressure should be applied by means of a pad and a bandage. If nitrate of silver be used, it should be introduced on probes at the point where the knife entered, and be passed into all parts of the growth until the latter has acquired a feeling of considerable solidity; a poultice may then be applied, and the growth may be left to suppurate.

Complete removal of a subcutaneous nævus is quite possible, and not more dangerous than the removal of any other tumour, provided always that the growth is circumscribed, and that the incisions are not carried into the diseased tissue. A growth thoroughly suitable for removal by the knife is rarely met with; and should such a nævus present itself it would be easily curable by injection, by nitrate of silver, or perchloride seton.

A diffuse subcutaneous nævus—i. e., one where a certain, or rather an uncertain, area of subcutaneous tissue is spotted about here and there with vascular growths—is unsatisfactory in the highest degree to treat. I would advise you in these cases to wait for a cure by nature, since it is far more likely to occur through lapse of time than by your efforts.

Mixed nævi, as well as being the most common forms of the disease, are rather more difficult to cure than the former varieties. This is apparently because they receive their blood from so many sources, the deeper as well as the cutaneous vessels being channels of supply. These nævi rarely have any definite capsule like the completely subcutaneous variety. The disease is situated in the subcutaneous tissue, and projects through the skin. Like the cutaneous and subcutaneous, they may be either diffuse or circumscribed, and just as in these, the circumscribed are more easy of cure than the diffuse. Since they combine the principal features of the cutaneous and subcutaneous, they may require a somewhat complicated treatment.

When *circumscribed*, and but a small portion of their substance lies in the subcutaneous tissue, they can be treated conveniently by ligature, either in one mass, in halves, or in quarters, just as the prominent cutaneous nævi. Should the disease extend deeply beneath the skin, the subcutaneous part must be treated just as a true subcutaneous nævus. It may be broken up with a narrow knife, and pressure may be applied, or nitrate of silver introduced; it may be injected with solution of tannic acid or perchloride of iron, or the perchloride may be introduced on setons. After the cure of the subcutaneous portion of the growth, should the cutaneous part not disappear, it may be cured by painting with the strongest nitric acid.

This kind of nævus may grow to an enormous size, especially in the parotid and submaxillary region.

A little boy, aged 13 months, came here three years ago with a mixed nævus, situated chiefly in the parotid region, involving the pinna of the ear and meatus, and extending widely over the face towards the occiput, far down the neck, and along the submaxillary region towards the chin. I treated the most prominent and disfiguring portions of the cutaneous part of the growth with nitric acid, and did not meddle with the massive subcutaneous part. The disease has now disappeared.

There is a drawing of a similar growth in the same position in the museum of St. Bartholomew's. This disappeared spontaneously.

In 1864 a child was brought to me with a large mixed nævus, as big as a man's fist, in the parotid and left submaxillary regions. I passed two setons steeped in perchloride of iron through its deepest parts. No other treatment was employed. I believe that in this case also a cure has resulted, more, however, from the lapse of time than as the effect of the treatment employed.

Mixed nævi, if diffuse, are unsatisfactory to treat. The cutaneous part may be destroyed by acid or other caustics; the subcutaneous portion is best left alone.

Pendulous nævi are rarely met with. They are very vascular, easily bleed, and generally hang by a slender stalk. I have had but one under my care. It was just the size and colour of a large raspberry. It was ligatured at its *base, and the root being* touched with nitric acid, it completely disappeared

Ignorant people usually connect the most extraordinary tales with these growths, which are generally supposed to increase and bleed about the time of the ripening of the fruit they most resemble.

Peculiarities of treatment necessitated by certain localities.—There are certain parts of the body where the general rules applicable to the treatment of the different kinds of nævus must be modified so as to suit the peculiar requirements of the part. I shall now refer to those parts where special treatment is either advisable or necessary.

The scalp and forehead.—Here nævi may be sometimes advantageously treated by pressure constantly maintained by means of a piece of lead, or a small coin, stitched into an elastic silk band, or by means of any other device for exercising pressure that may suggest itself to the surgeon. It is a somewhat wearisome means of cure, and can only be successfully carried out with the assistance of an intelligent mother or nurse, who must be able to readjust the apparatus when displaced.

Nævi over the fontanelle should be more cautiously treated than elsewhere —*i. e.*, injection or perchloride setons, if used, should not penetrate very deeply. I have treated many in this situation successfully, but should always prefer to wait until the fontanelle be either quite closed, or at all events until ossification had taken place beneath the nævus.

The only case of nævus that has died under my hands, in consequence of an operation, was one situated in this region, where death occurred from sloughing after the introduction of perchloride setons.

Face.—It is well that you should be reminded that the treatment of nævi in this region by the injection of perchloride of iron has more than once been followed by instant death.

In the *London Medical Gazette* of 1837 the following case is reported by Mr. Paget, of Leicester:—A child, aged 2 years, suffered from a mixed nævus, as big as half an orange, over the right upper jaw. The growth was principally subcutaneous. On two occasions the nævus was pierced with a lancet, and a mixture of one drop of nitric ether with fifteen of nitric acid was injected. As this produced no effect, on the next occasion a small quantity of liquor ammoniæ was injected, the solution being so diluted that it could be smelt without its pungency being disagreeable. The instant that this fluid was injected, the child had one slight convulsion and was dead.

Mr. R. B. Carter, in 1863, reported the following case:—

An infant seven weeks old had on his nose a mixed nævus that was rapidly growing. The hot needle was employed without effect; contractile collodion was painted on without avail. When the child was eleven weeks old a small quantity of perchloride of iron was injected, and a few days afterwards this proceeding was repeated. On this latter occasion ten minims of the solution were put into the syringe, and five minims were injected with a jerk, as the piston of the syringe did not move freely. A small discoloured spot appeared on the nævus over the point of the syringe, the child shrieked out, had a single convulsion, and was dead.

Again, I quote another, and the only other, instance, so far as I know, of this catastrophe. The case is reported by Mr. R. B. Carter, being communicated to him by Mr. Nathaniel Crisp, who was good enough to correspond with me on the subject Mr. James, of the Melbourne Hospital, where the accident occurred, also gave me an account of it. It is by far the most instructive case, as it gives one the clue to the cause of the sudden death in these cases. The circumstances were these:—

A child suffered from nævus of the cheek, into which perchloride of iron was injected, and death occurred in fifteen seconds. On examination, it was found that the point of the syringe had passed into a small branch of the transverse facial vein; the blood was found coagulated in this vein and in the venous system down to the right side of the heart, which was filled with a firm clot.

This case, scanty though it be in its details, is sufficient to confirm the suspicion that would arise in one's mind as to the cause of death in the two previous cases.

As bearing on this point, I ought to mention that an infant under my own care was seized with difficulty of breathing a day or two after injecting a subcutaneous nævus on the face with perchloride of iron. It died, and after death exhibited symptoms of what is termed lobular pneumonia—*i.e.*, the lung-tissue was consolidated and apparently inflamed in patches. It is not impossible, in this case, that a small quantity of the perchloride had got into the circulation—too little to cause general coagulation of the blood in the right side of the heart, but enough to produce embolism in the pulmonary capillaries.

In any case, sufficient is known of the effect of the possible admixture of perchloride of iron with the general circulation, from injecting nævi on the face, to justify us in rejecting it as a remedy for nævi in these parts, unless, by pressure or by the employment of some instrument, the circulation in the growth is controlled, at least for a time.

Eyelid.—There is an admirable instrument (the ring forceps) for grasping the eyelid and commanding the circulation of a nævus situated on this part. The growth, when thus surrounded, may be injected, treated by introduction of nitrate of silver, or in any other way thought desirable, though ligature is not well suited for nævus in this locality.

Conjunctiva.—The only advice I have to give concerning the treatment of nævus in this locality is to leave the growth alone if it affects the ocular surface of the membrane.

The *lip* is often affected with nævus on its mucous edge. During the application of any remedy, the circulation can easily be controlled here with a hare lip instrument or the ring forceps. The growth should, if possible, be attacked from the mucous surface of the lip.

The *ear* and the *nose* are very unfavourable localities for the cure of nævus. When the disease extends through the entire thickness of the cartilages, as it frequently does, painting with strong nitric acid has given me the best results. Should the nævus be in the form of a tumour, it can be ligatured advantageously.

It is a question with some, when a child is largely blotched over the face with a highly-coloured cutaneous nævus, whether the crimson stain of the disease should be exchanged for the dead-white mark of a caustic. I am in the habit of treating these nævi at their most highly-coloured parts with nitric acid, as I am certain that, in an adult, no mere disfigurement, apart from real deformity, is so great as a plum-coloured or purple irregular stain over one side of the face. The acid should be painted on the most deeply-stained parts, and especially about the circumference of the nævus. I can strongly recommend this plan from repeated experience of its efficacy, and the satisfactory results it produces.

Nævus on the mucous surfaces—such as the gums, the vagina, and the rectum—is generally of the cutaneous variety. It may be cured by the application of nitric acid. Those in the rectum often give much trouble, from their great tendency to bleed; and unless this sympton was present, I should be loth to interfere, on account of the contraction that might follow the application of caustic. However, when hemorrhage occurs, nitric acid may be applied freely, so that if possible one application may be sufficient.

You may have observed that, in describing the remedies for nævus, I have omitted any mention of the treatment by croton oil, tartar-emetic ointment or plaster, simple setons, circular incisions, and the introduction of needles. It is not that I have not tried these means; but that they have either proved inefficient in my hands, or that the severity of the remedy has not been proportionate to the promise of cure.—*Lancet, July* 20, *Aug.* 3 *and* 17, 1867, *pp.* 65, 124, 191.

ALIMENTARY CANAL.

68.—ON THE TREATMENT OF OBSTRUCTION OF THE BOWELS, AND UPON CONSTIPATION.

By Dr. Thomas Head, Carlisle.

[In the first case, which we give as illustrative of Dr. Head's treatment, the patient had eaten a large quantity of unripe fruit, great abdominal pain followed, with obstinate constipation. The bowels had not been relieved by any treatment adopted. The enemata used were of gruel, and had never exceeded a pint in quantity.]

On consultation I suggested the use of an enema of three pints of warmed oil, which was relunctantly acceded to, in consequence of the previous enema being still retained. Dr. Todd made it a condition that I should superintend its administration, to which I readily consented; the whole of the oil was slowly and carefully thrown up into the bowels; the abdomen became enormously distended, and the stomach rejected everything taken—even the smallest quantity of fluid of any kind was rejected. The oil was retained in the bowels by means of pressure over the anus by a soft napkin, continued for more than an hour. In less than three hours, part of the oil returned with slight appearances of fœculent matter; but, within ten hours after the warmed oil had been injected, four or five copious evacuations were passed containing enormous quantities of scybalous fæces. During the operations upon the bowels, anodynes and champagne were given, and some refreshing sleep ensued. This young gentleman's health remained for some time in a precarious state, but was eventually completely restored. Some weeks after this attack a thickened condition of the ileo-cæcal valve, which, however, disappeared, in course of time, could be distinctly felt by a manual examination. The bowels remained rather torpid, but in other respects his health was gradually and eventually completely reinstated.

In cases of obstruction resulting from the lodgment of undigested articles of food, observation has led me to regard the lower portion of the ileum or the ileo-cæcal valve as the locality of the disease, and having seen such frequent instances of disappointment in the use of enemata of the ordinary quantity, I have been led to place my chief reliance in those of large volume. For constipation limited to a loaded state of the colon, injections possessing stimulating properties will generally prove successful; but in cases similar to that described, they will most frequently be found inefficacious. A pint enema with a few drops of laudanum may indeed with advantage be first injected, and upon it the oil in the quantity described; the oil ascends through the watery fluid and is thereby more certain to reach the seat of the disease.

Obstruction of the Bowels.—Failure of small Injections.—Successful Effects of copious Injections of Oil.—Recovery.—A poor woman, a patient in the Carlisle Dispensary, married, aged 33 years, became the subject of obstruction of the bowels. The physician under whose care she came had not seen her for two days, but had ordered a small treacle and water enema to be administered with the old pipe and bladder apparatus. In consequence of his absence I was asked to see the case. All the symptoms of obstruction of the bowels in a severe form were present; the internal medicines which had been prescribed had proved unavailing, and had caused much vomiting. I ordered hot fomentations with turpentine to be applied to the abdomen, and a large oleaginous enema to be administered with the syringe and O'Beirne tube; in the course of a few hours the bowels acted freely with great relief to her sufferings; the symptoms of peritonitis, the result of intestinal distention, rapidly abated, and she made a speedy recovery.

The next case presents points of much professional interest.

Obstruction of the Bowels of ten days' duration.—Recovery.—A gentleman, aged 56, extensively engaged in the corn trade, who had been in the habit of biting wheat and other grain to ascertain their quality, and to swallow the particles thus introduced into his mouth, was attacked with great pain in the

bowels (especially on the right side, in the situation of the ileo-cæcal valve) accompanied with vomiting; the pulse was rapid and feeble. The treatment consisted at first of hot fomentations and turpentine to the abdomen, an enema of oil saponified with liquor potassæ and turpentine; moderate doses of calomel, opium, and creosote were given at short intervals. From these remedies no relief was procured. The pain having increased in the right side of the abdomen, ten or twelve leeches were applied; no improvement in the symptoms took place; large enemata of oxgall and warm water were repeatedly administered; no satisfactory results followed. Three pints of warmed oil were now administered as an enema, and retained by firm external pressure to the anus. The sickness became most distressing, followed by severe vomiting, and a large quantity of the clear oil that had been injected into the bowels was thrown off the stomach; this took place repeatedly for the next few days, with, however, an apparent diminution of the pain and other distressing symptoms. No evacuation from the bowels took place. Pills of concentrated oxgall with creosote were now repeatedly given, and a free evacuation of fæcal matter speedily followed, mixed with immense quantities of comminuted grain; the motions appeared devoid of bile. The stomach became less irritable, and the vomiting gradually subsided. A tablespoonful of recent oxgall was given every morning with a few drops of chloric æther, and was happily retained, and the bowels by these means were brought into free daily action. The obstruction of the bowels was of ten days' duration. The patient gradually recovered his health, and, with the occasional use of artificial and natural Harrowgate water, the bowels have been restored to a much more healthy state than they have been in for a long time previously.

Obstruction of the Bowels of twenty-one days' duration.—Recovery.—It must not be supposed that all cases of intestinal obstruction are characterised with the urgent symptoms detailed in the preceding cases; on the contrary, it may be safely affirmed that in persons of advanced age the system acquires under every form of obstruction a power of endurance much greater than in younger persons suffering from the disease. The following is a good example of this condition:—

A lady living in the vicinity of Edinburgh, aged 74, became the subject of intestinal obstruction, and, notwithstanding that all the ordinary remedies were unremittingly used for a period of three weeks, no fæcal evacuation was procured. At this date the late Dr. Allison saw the case, in consultation with Mr. Sanderson of Musselburgh, and as a last resource small and repeated doses of calomel and opium were prescribed; in three days, ptyalism ensued, and on the day following free fæcal evacuations took place; the bowels now became so very much relaxed as to threaten fatal exhaustion; by the use of stimulants and mild nourishment a slow recovery was made, and the life of the old lady was prolonged for several years.

The practical conclusions to which these cases would lead are—that in the intussusception of infancy inflammatory action which occurs in the middle periods of life is not excited in the serous membrane; that the straining and the evacuations of pure mucus streaked with blood are points of the highest diagnostic value. In such cases the peristaltic action of the small intestines ought to be allayed, rather than excited by medicines given by the stomach; and oleaginous enemata and quicksilver are means of the most probable efficacy for restoring the displaced intestines to their healthy relations. Where obstructions depend on the presence of undigested aliment, the lodgment most frequently occurs at the commencement of the large, or near the termination of the small intestines. After the lower portion of the colon has been cleared out by the use of a strong enema of oil, saponified with a little liquor potassæ, combined with about an ounce of turpentine, without the general peristaltic functions of the bowels being restored, it becomes desirable to use large enemata of warmed oil, the lower portion of the colon having been previously filled with a small ordinary enema of warm water, to which a few drops of laudanum may be added to secure its retention. Experience has appeared to justify me in believing that this mode of treatment is calcu-

lated to accomplish relief more speedily and certainly than the kind of treatment ordinarily adopted.

In the obstruction of aged persons the tendency to inflammatory complications becomes lessened, and the treatment may, on that account, be both of a less energetic character, and be longer persevered in before either relief to the obstruction or a fatal issue occur. It may here be also noticed as a point of diagnostic value, that in obstruction of the bowels, the seat of the disease being near the small intestine, the symptoms more nearly resemble those present in strangulated hernia; while, when the larger bowels are obstructed by scybalous accumulations, there is much less irritability of the stomach, although in both great abdominal pain may be produced.—*St. Bartholomew's Hospital Reports, Vol. III.*, 1867, *p.* 91.

69.—OBSERVATIONS ON CERTAIN SUBSTANCES LIABLE TO BE MISTAKEN FOR INTESTINAL WORMS.

By Dr. Wm. Frazer, Honorary Member of the Medico-Chirurgical Society of Montreal, &c.

Cells of Orange Pulp mistaken for Entozoa and for Hydatids.—Three examples of this curious mistake have occurred to me. The first was referred for investigation some years ago from the north of Ireland by a physician who wished to ascertain whether the strange substances his patient had passed from his bowels were hydatids, as their appearance had given rise to much uneasiness. When informed of their nature his reply was, "I beg to thank you for your kindness in examining the supposed hydatids, the patient has been using oranges, and you are perfectly correct.

The second case was submitted to me by Dr. Austen. A delicate child had got an active vermifuge under the idea of worms being the cause of her ill health, and undigested cells of orange pulp were discovered in the alvine discharges, these were alleged to be genuine intestinal worms, but doubts having arisen about the supposed ascarides, he wished to have them examined. Dr. Austen's own impressions were opposed to their being worms, and his note in reply to my letter was, "You are perfectly correct as to the supposed worms, the child had eaten an orange or two for several days consecutively, the matter is really rather ludicrous."

In the third instance, a young gentleman of delicate appearance was brought to me by Dr. Kirwan; he displayed some substances floating in a phial of water, which he had picked out of the evacuations, and believed he saw them *distinctly moving*, they were cell walls of orange pulp, the contents being quite digested. When told what they were, he admitted that he had eaten the fruit. He was suffering from impaired digestion, and complained of irregular action of the bowels.

Undigested Celery mistaken for Ascarides.—Some years since a gentleman brought me several long undigested fragments of celery stalk, chiefly consisting of the stringy vascular tissue, which he fancied were worms. He had seen some ascarides lumbricoides passed by a child, and was convinced he was infested by them. The microscope showed their vegetable nature and rendered their recognition easy. It is not unusual for persons who have feeble digestion to excrete unaltered vegetable substances consisting of indurated vascular tissues or of sclerogen, and even the cellular portion of vegetables may pass off undigested, and it must be admitted in excuse for this patient's error, that the fragments of celery he mistook for entozoa were not unlike semi-digested ascarides.

Plastic lymphy intestinal exudation mistaken for Tape-worm.—In this distressing case a physician of great promise was attacked with plastic lymphy exudation of the intestines; he had suffered from griping abdominal pains and a train of nervous symptoms, when some fragments of lymphy matter being expelled, he was supposed to have tape-worm and advised to use drastic purgatives and anthelmintics. These had the immediate effect of aggravating his symptoms, severely injuring him and increasing the number and

size of the expelled masses. The specimens that I examined were of a pure white colour, seldom exceeding half an inch in length and of irregular shape, consisting of granular substance which passed into fibrillation on the surface that appeared to have been attached to the intestine. The lymphy fragments continued to form at intervals for several weeks until he left Ireland for a protracted sea-voyage, and soon after his departure he ceased to pass these plastic masses.

Larvæ of Diptera supposed to be Worms.—In two instances, of which I have preserved no notes, dead larvæ of some fly were brought to me under the idea of their being intestinal worms; they were stated to have passed from the bowels, which was possible, though their presence might admit of other explanations.

Elastic-ligamentous Tissue supposed to be Tape-worm.—Some years ago a woman in the wards of the Hardwicke Hospital, under the charge of Sir D. J. Corrigan, fancied she was attacked with tape-worm. The alleged worms were submitted to me for microscopic examination, they proved to be a quantity of fragments of ligamentum nuchæ which she had eaten weeks previously; they lodged in the cœcum, causing distinct fulness in that region, and required repeated purging for their expulsion. This case was recorded by Sir D. J. Corrigan in one of the early numbers of the *Dublin Hospital Gazette*, and affords a good illustration of the value of microscopic examination in determining the nature of doubtful substances.—*Medical Press and Circular, April* 17, 1867, *p.* 361.

70.—OPERATIONS FOR HEMORRHOIDS IN TWO FEMALES.

(From a report of some operations at King's College Hospital.)

In these cases Mr. Smith adopted the method usually followed by him—an old Dublin operation, but considerably modified by Mr. Smith. The part to be removed is seized with a clamp, the under surface of which, *i. e.*, that next the anus, is covered with ivory, and the instrument is prevented from slipping by a screw. The hemorrhoid, grasped by the clamp, is then cut away, and the actual cautery energetically applied to the wound, the ivory preventing the conduction of the heat, and thus saving injury to the parts behind. The screw is then gradually loosened in case of hemorrhage, which, should it occur, can be easily stopped by retightening the screw. Mr. Smith remarked that this operation was much less painful and dangerous than that of ligaturing and allowing the part to mortify away; for, by that means, sometimes fatal results followed, or, at least, the recovery was more tedious, whereas, in the case just operated upon, the patients would be well and moving about in three or four days. He mentioned that he had never seen hemorrhage with the clamp operation in internal, but once or twice in external piles.—*British Medical Journal, Oct.* 12, 1867, *p.* 313.

ORGANS OF URINE AND GENERATION.

71.—CONSIDERATIONS SUGGESTED BY THE STUDY OF 100 CASES OF STONE IN THE BLADDER OF THE ADULT, RECENTLY OPERATED ON.

By Sir Henry Thompson, Surgeon-Extraordinary to H. M. the King of the Belgians.

The following cases of stone in the bladder of adult patients are not selected in any way, but are those which my own practice has furnished during the last three or four years. No exception has been made, every case being recorded as it occurred: so that the last 100 adult patients on whom I have operated up to the time of my writing form the material for the remarks which I shall have the honour to submit to my professional brethren. I have *taken these cases* because all have been treated with the same instruments and

on the same principle, and because the number is a simple one for per-centage calculations, while it is sufficiently large to warrant our dealing with it in relation to some important particulars in connexion with this malady.

First, let me call your attention to the fact that these are all *adult* cases, and for the most part adults of an advanced age. It is necessary to bear this in mind, because, as we all know, the mortality following the operation of lithotomy before puberty is very inconsiderable as compared with that which occurs in after-life. Hence, when we learn the rate of mortality in any given series of cases which contains patients of all ages, we do not really obtain any information on the subject unless we are at least supplied also with the number of patients of an age below puberty included in the total.

Reasoning from some extended researches respecting the death-rate in lithotomy which I very carefully made a few years since, I may safely assume that the mortality following that operation in children is 1 in 14 or 15 cases; that in middle-life it is 1 in 6 or 7; and after sixty years of age, about 1 in 3 or 4 cases. These broad numerical statements will serve our purpose as standards of comparison in dealing with the series of facts to which I have now to request your attention.

Secondly, in regard to the 100 cases under consideration, I have this, also, to remark: not only are they adults and the majority of them old men, but, in point of eligibility and fitness for operation, they fall short, in my opinion, of the average of stone patients afforded by any single district. These cases have been furnished from all parts of Great Britain, and some few have come from distant parts of the world—for example, from India, the Cape, and Canada. For I may, perhaps, be permitted to say, that it has been observed that known experience on the part of an operator attracts a certain number of patients, in unusually bad condition and with large stones, who seek his aid as their last chance of recovery. This fact, complained of by operators at an early time, was never so influential as at present from the numerous facilities of locomotion now existing; and the effect certainly tends, in a very appreciable degree, to injure the results of practice, if these are to be judged solely by a numerical statement of deaths and recoveries, without reference to the severity of the cases.

With these preliminary observations, I will now proceed to state that the following 100 cases have been operated on in about three years and a half; that I have not refused to operate in a single case which has applied to me in that period; that I have employed lithotrity in every instance in which I felt it was safe and prudent to apply it; and have performed lithotomy in the worst possible cases, however bad, rather than leave the patient to the certainty of a painful and lingering death. Further, that I shall submit a table of the cases, as an appendix, which I will not occupy your time by reading; and associated with each case is the name of some gentleman who witnessed it with me, either in consultation or otherwise. Lastly, that in almost every case the stone itself is placed on the table for your inspection.

The following facts respecting these cases may first be noticed. Of the 100 cases, 84, or about four-fifths of the total number, were operated on by lithotrity, and 16 by lithotomy. The mean age of the 84 lithotrity cases was 62½ years; among them were no less than 21 cases of 70 years and upwards, 2 being upwards of 80 years of age. Among these 84 cases there were only 4 fatal cases. The lithotomy cases, 16 in number, had a mean age of 63 years. The youngest was 42 and the oldest 80 years. 6 were above 70 years of age, including one each of 77, 78, and 80 years. 6 cases were fatal. Of the entire 100 cases of operation upon unselected patients, having a mean age of 62½ years, and submitted to either lithotrity or lithotomy, there were 90 recoveries and 10 deaths.

I think I may now venture to assume that these results are successful—perhaps as much so as any similar series of cases which have been authenticated as carefully and satisfactorily as this has been. And I am anxious to promote the habit of thus authenticating cases; not that I have any doubt whatever that any circumstance stated by me to be a fact would be for an instant called in question; but I feel very certain that any operator would

prefer that statements of so much interest and importance as those must have which influence surgical practice generally should rest on authority which can be tested, especially as this is quite easy to accomplish, without in the least degree infringing the confidential relations which ought to exist between us and our private patients, and which should ever be strictly respected.

I shall now endeavour to embody the most important of the considerations which are suggested by the study of these cases in four propositions, and shall remark upon each as briefly as possible.

The first proposition is,—That lithotrity is the most successful operation for at least four-fifths of all cases of stone in the adult which come under the surgeon's notice at the present time; a statement which is more definitely expressed in the fact, that the rate of mortality for such cases in this series is barely 5 per cent.

The second proposition is,—That these successful results from lithotrity can only be obtained by performing the operation in accordance with certain rules which can be laid down.

The third proposition is,—That cases of calculus, in which one of the two operations, lithotrity or lithotomy, ought not to be employed, are exceedingly rare.

The fourth proposition is,—That by exercising an ordinary degree of vigilance for adult patients suffering from symptoms of urinary disorder, every case of calculus may be discovered in an early stage; may be successfully treated by lithotrity; and, consequently, that the operation of cutting for stone in the adult may be rendered obsolete, or applicable only for some very exceptional example which has been developed as the result of extreme neglect or ignorance.

The first proposition—namely, that lithotrity is the most successful operation for at least four-fifths of all the adult cases of stone which come under the surgeon's notice at the present day—is supported, I may say proved, by the 100 cases before us. A rather larger proportion, indeed, than four-fifths was dealt with in that series. This must be regarded as an immense advance on what has been in past time the practice in Great Britain. I doubt whether, if we were to take for any given year all the calculus cases occurring in adult patients in Great Britain, more than one-half would be found to have been submitted to the crushing operation. I think I may safely say that very nearly one-half of the adult cases are still operated on by the knife. At the same time it must not be denied that in the hands of surgeons who have not practically studied lithotrity with care, the cutting operation would probably still afford the best results. Yet how immense is the superiority of the crushing operation in practised hands. Four deaths in 84 cases of men averaging 62½ years. I am satisfied that this result is attainable only with considerable experience and very careful attention to all the minutiæ of such cases. Yet the difference is so great that if even a much smaller success only is achieved, it is still superior to any which the knife can accomplish.

The next proposition is that lithotrity can be thus successful only when it is performed on a definite system, in accordance with certain practical rules which experience has determined, and which can be laid down.

But as a necessary preliminary to a very brief consideration of this topic, I would remark that, in every case of stone in the bladder, it is a matter of the highest importance first to make a correct diagnosis of the size and nature of the body which is to be removed, as well as of the constitution and condition of the patient; so that a judicious choice of the appropriate operation may be insured. I have often had occasion to remark, that more harm may be done, and has been done, by the employment of the two proceedings—lithotrity and lithotomy—without due discrimination, than if lithotomy only had been resorted to in every case.

In making the diagnosis, the first fact necessarily ascertained is the capacity of the urethra. It is not common to find it deficient for the purposes of *modern* instruments, although it was frequently so regarded when larger and *clumsier lithotrites* were employed; at most a slight congenital narrowing at

or near the external meatus may require a trifling incision to allow the lithotrite to pass with facility.

The size and form of the stone are next to be determined. The kind of sound to be used is by no means a matter of indifference. I should have thought it unnecessary to say this did I not know from experience how many surgeons still use a sound having the form of an ordinary catheter. Such an instrument does not admit of much lateral movement in the cavity of the bladder; while rotation is impossible. Hence it often fails to detect a calculus. An exploring sound should have a short beak, which can be moved with facility in every direction, and which can, therefore, be easily reduced or made to dip behind the prostate into the bottom of the bladder. I have made an addition to this sound, which I have found extremely useful in enabling me to ascertain the size of a calculus without using a sliding blade, or having recourse to a lithotrite for the purpose of measuring. Provided a suitable manipulation is employed, this can be accomplished with a very near approach to accuracy. The mode of using it may be thus described.

The surgeon, having struck the stone moves the beak of the sound to the further end of it, and slightly taps its extremity. The sliding ferrule is then slipped down the stem till it touches the external meatus. He then slowly draws out the sound, making a series of little taps on the stone as he does so, until he reaches the near end of the stone, and the instrument ceases to elicit a tap, having passed over it. Meantime, keeping his eye on the sliding ferrule, he observes how much of the stem has issued from the urethral meatus during the progress of the taps. This sounding will furnish the length of the stone to one-eighth of an inch, as I have often demonstrated at the bedside in University College Hospital.

If a still more precise demonstration is required, a flat-bladed lithotrite may be introduced; the stone caught at least twice—if possible in two directions—and the measurements noted, and while grasped in the lithotrite, it should be moved about in the bladder gently, when, if another stone exists, it will be struck. This proceeding usually gives rise to more irritation than the method with the sound, but it produces results which are perhaps a little more precise. The note from the stone struck, and the condition of the urine, as well as the condition of the patient himself, will indicate with great accuracy whether the stone is an acid or an alkaline one—a compound of the urates or of the earthly phosphates. The oxalate of lime calculus is much more rare; unless its volume is small, the resistance it gives to the lithotrite will indicate its quality.

In discussing fully the question arising out of the constitution of the patient much time might be occupied, but it will be held to be undesirable to do so here. In few words, a quiet, unexcitable condition of the nervous system, with no extreme susceptibility to feverish attacks, are points in favour of his submitting safely to numerous sittings if the stone be large, and *vice versâ*. The fact of renal disease existing in my experience, by no means contraindicates lithotrity. Several of the patients in my list had advanced kidney disease, and were passing much constitutional albumen.

Supposing that a uric-acid stone of about an inch and a half in the long diameter is met with, and all the conditions are tolerably favourable, there is no doubt that lithotrity may be performed with a good chance of success. I by no means affirm that such a stone of two inches in the long diameter should not be crushed, but, as a rule, I think lithotomy is the safer operation. The weight of the former will probably be about six to seven drachms. After the weight of an ounce is reached, we arrive at the category of large stones. If the stone is phosphatic, one of much larger size may be treated by lithotrity.

[Sir H. Thompson next expounds those maxims which are chiefly valuable in this practice.]

1st. If the patient has never had instruments passed into the urethra before, it is as well to use a large flexible bougie or two, on two or three occasions, to observe the result both on the bladder and on the general con-

dition. If fever is easily aroused by so simple a procedure, the question of performing lithotrity must be carefully considered.

2nd. The first sitting should be an extremely short one, and should consist in seizing the stone not more than twice (with the fenestrated instrument, only when large), and merely breaking it into several portions. The patient should remain in bed, and be admonished to pass water for the next twenty-four or thirty-six hours only while lying on his back. During the first two or three sittings I always enjoin this rule, esteeming it to be one of considerable importance. I endeavour to prevent the exit of débris during this period, in order that the fragments may become a little water-worn and their edges less sharp, and that the urethra may recover from any slight degree of irritation which the transit of the lithotrite may have produced. The fragments and the urethra are mutually better adapted to each other on the second or third day after the operation than on the day itself. I regard this practice as one which conduces in no small degree to prevent a painful, if not a dangerous, degree of irritation.

3rd. I am satisfied that the lithotrite should rarely remain at any sitting more than two minutes in the bladder. One minute usually suffices for three or four efficient crushings. Of course this is only possible with an instrument the construction of which admits of instantaneous change from screw to sliding movements, and *vice versâ*; and, for successful lithotrity, I do not hesitate to say that this is absolutely essential. I believe no lithotrite fulfils the various conditions required for rapid and easy working so well as that now made for me by Weiss, of London, and in great measure from suggestions which have occurred to me in constant practice to meet varied exigencies as they have arisen. Besides the quality just referred to, this lithotrite can be turned in every direction with perfect ease, while it can be held more firmly, when so required, than any other instrument. Moreover, a small quantity of débris may be removed by it with facility and safety every time the instrument is withdrawn from the bladder. The flat-bladed lithotrite is unquestionably that which should be employed whenever it is possible to effect our object by its means, leaving the fenestrated instrument for exceptional use only.

4th. Not only by making the sitting short, but also by diminishing as much as possible the use of all instruments not absolutely necessary, we should avoid producing mechanical irritation of the bladder. For by far the greater number of these cases a simple flat-bladed lithotrite has sufficed for every operation, and no other instrument whatever has been necessary, excepting only an exploring sound; and I do not hesitate to affirm that very rarely indeed is any other required. Most unusual, even among these elderly patients, is it to observe any indication for the removal of débris by washing out—a process formerly regarded as an essential part of the proceedings which constitute lithotrity. The now well-known apparatus of Clover is occasionally valuable when the bladder, from exceptional circumstances, does not sufficiently expel its contents, and requires to be emptied of retained débris. In regard to the practice of drawing through the urethra rough fragments of large size, I can only speak with the strongest reprobation. Not only must I regard it as injurious and dangerous, but as always wholly unnecessary. In thus acting, the injury inflicted is, in all cases, a purely gratuitous one. For whenever a fragment in the bladder is seized, a single turn of the screw suffices to pulverise it, and the product is at once in a condition in which it is harmless as regards the urethra. The perfection of lithotrity consists in easily reducing the stone to powder, and in allowing Nature to act by those forces which she possesses, and in the great majority of cases successfully applies to the removal of gravel and small calculi. In a word, all the dangers and difficulties occurring to the patient in lithotrity may be broadly referred to one cause—viz., mechanical injury to the bladder and urethra. The sources of this injury are two. The stone itself, and the instrument employed to remove it. To avoid these dangers and difficulties *it is simply* necessary—1st. To use the utmost gentleness and tact in the *use of the lithotrite*, which must be of the most perfect kind, and to be as

sparing in the employment of all mechanical appliances as possible. 2nd. So to use such appliances, and so to manage the patient, as to reduce to a minimum those conditions of the stone itself which render it irritating. When these indications are fulfilled to their utmost, lithotrity reaches its highest degree of perfection. Thus, we shall aim at keeping the patient almost entirely at rest while the fragments are large and sharp, and we should reduce them to the form of powder as soon as possible subsequently. Sometimes it happens that cystitis is set up as the result of crushing a stone into large fragments, which does not readily yield to ordinary treatment. I have often found that, contrary to the usual rule, the patient is greatly relieved by a fresh crushing, which reduces these large and sharp fragments to smaller pieces, so rendering them less irritating to the bladder. Sometimes, too, under these circumstances we may go further, and by means of the flat-bladed instrument, we may, as an exception to the general rule, remove a considerable quantity of the debris when it is no longer well tolerated by the bladder.

5th. Few matters are of more importance, as regards the ultimate well-doing of the patient and the credit of the operation, than the removal of the last fragment. When a case of lithotrity draws to a close, and the symptoms have greatly diminished, but are not quite absent, it is often difficult to determine whether the bladder has been absolutely emptied of stone or not. On sounding, let us suppose that nothing is discovered; nevertheless the patient feels a little pain at the close of micturition, and this act is performed with too great urgency and frequency. It is difficult to discover whether this is due to a fragment still remaining in the bladder, or to an irritated and slightly inflamed condition of that viscus or of the prostate, resulting from the presence of the stone and of the means which have been employed to remove it. If it is the latter, repeated soundings will only increase or pro long the morbid condition; if it be a fragment, one or two more soundings may be necessary to discover and reduce it. The real cause must, however, be discovered; but time will not allow me to enter on a complete practical consideration of this important point. One hint respecting a plan which I have found useful in this state of doubt is the following: Supposing that, after one or two careful explorations with the sound and with the small short-beaked lithotrite, the blades of which are easily reversed, and which can thus be made to traverse easily the floor of the bladder, nothing is found, I prefer to give up all interference, and let the patient have a week or two's freedom from instruments; but during that time he should on two or three occasions take a rough drive in an omnibus or other jolting vehicle. I know nothing which will so certainly increase the irritation if it is due to a fragment, while the effort will probably be very slight if none is present.

I now come to the third proposition—namely, that cases of calculus in which one of the two operations, lithotrity or lithotomy, ought not to be performed, are exceedingly rare. I have already said that I have not declined a single case during the period in which these 100 cases were operated upon. On reflection, I might perhaps have done wisely to decline one case—a patient for whom I performed lithotrity,—a fatal one; and, further, I might have exercised a worldy-wise and prudent policy had I declined some five or six more whom I submitted to lithotomy. But then I believe it to be a dereliction of sacred duty to deny the one single chance of life to a suffering brother, lest the statistical results of practice should be impaired. Not unfrequently have I consented with much reluctance to operate; but it has been difficult to withstand the entreaty of a poor fellow for whom death was imminent, and who, knowing fully all the risks, begged hard that he might encounter them, as his last and only hope. This recently occurred with a patient who, after seven years of suffering, came to me from the Cape—a mere skeleton worn out by disease. I declined, but offered an appeal from my decision, in the form of a consultation with my friend Mr. Paget. The refusal was confirmed, and, in despair, the patient sent for a well-known physician, and begged to learn why he should be denied the only hope of life, for which he had at so much cost of time and of pain crossed half the globe.

The result was that we consented. I cut him, and he recovered. Is it not, then, almost impossible to say there is no hope? And if there be a single chance, how difficult to deny it! It was among such cases as these—cases of advanced age and of large stones—that I met the 6 deaths among the 16 of my lithotomy cases.

The fourth proposition is—"That by exercising an ordinary degree of vigilance for adult patients suffering from symptoms of urinary disorder, every case of calculus may be discovered in an early stage; may be successfully treated by lithotrity; and, consequently, that the operation of cutting for stone in the adult may be rendered obsolete, or applicable only for some very exceptional example which has been developed as the result of extreme neglect and ignorance."

It may seem a bold assertion, that every case of stone in the adult may be treated by lithotrity. Depend upon it, this will be the case, and that its realisation is simply a question of time. I suppose no one will deny that it is an easy thing to diagnose the presence of calculus in an early stage, while it is yet small, and easily amenable to the crushing process. Very rarely does a calculus reach the size of a bean without giving marked symptoms of its presence; enough to send the patient to his surgeon; enough for his surgeon to suspect its existence. At all events, with anything like ordinary intelligence on the part of the patient and on that of his attendant, no stone should reach the weight of 100 grains, or measure an inch in its longest diameter; in other words, arrive at the size of a filbert without discovery.

Now, by way of proof, I wish to call attention to the striking fact that among the 84 cases of lithotrity, *all such stones were successful cases.* Each one of the fatal cases were stones of much larger size, occurring in patients worn out not merely by age, but by the long existence of the calculus and its fearful consequences. I am entitled, therefore, to say that every calculus being discoverable with proper vigilance while it is still small, it is removable by proper means, with an almost certain prospect of success. I regard the operation of lithotomy, then, attractive as it is to the practical surgeon by its brilliancy, and by the qualities which it demands for its successful performance, as destined ere long to be unknown, or nearly so, in the cases of the adult. For myself, I should resign it, with all its glories and excitements, with a profound regret, if I may be so permitted to speak, yet with a conviction that it will, though decreasing in frequency year by year, yet last perhaps during this century. The limits of time do not permit me to enter upon that fertile and interesting topic, the mode of its performance. Suffice it to say, that almost all the cases here recorded have been cut on the median and medio-bilateral methods; opening up a subject which I should have been glad to discuss had it come within the limits of our time. My object here has been rather to illustrate and enforce the capabilities of lithotrity, and to establish for it if possible a higher place in the estimate of British surgeons than it has hitherto attained. It is with this end that I have brought these facts before you to-day, believing that under no other circumstances should I find so good an opportunity of submitting them to the consideration of the profession at large.—*Lancet, Oct.* 26 *and Nov.* 2, 1867, *pp.* 511, 549.

72.—TWO CASES OF LITHOTOMY BY A MODIFICATION OF THE OPERATION OF CELSUS.

Under the care of Sir WILLIAM FERGUSSON, at King's College Hospital.

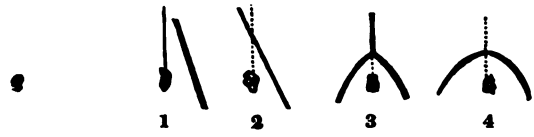
So content are surgeons ordinarily with the lateral operation for lithotomy that we do not very often have to record any departure from this well-known procedure. But the presence of a large stone in the bladder, and the want of sufficient space for its extraction, which is occasionally experienced by operators using this method, every now and then tempt the surgeon into devising some means by which the difficulty may be remedied. Of late years *the principal modifications* have consisted of two, (1) the median operation

of Allarton, and (2) the medio-bilateral of Civiale, which has been reintroduced and performed with very satisfactory results by Sir Henry Thompson (see Mirror. 1865, vol. ii., p. 673).

The procedure recently adopted by Sir William Fergusson is of peculiar interest, because in the main it is a return to an operation of great antiquity, and which remained unchanged up to the sixteenth century. There is an important addition, however, to the proceeding. In the old operation, that described by Celsus (De Medicinâ, lib. vii., cap. xxvi.), no staff was employed. A strong man, seated, took the patient on his lap, and held the bent thighs apart. The operator introduced two fingers of his left hand into the anus and endeavoured to push the stone into the neck of the bladder, the right hand aiding the pressure above the pubes. A semilunar cut was then made (its horns directed to the ischial tuberosities) upon the projection formed by the stone in the perineum. The neck of the bladder was divided by a second transverse cut, and the stone removed by the finger or a spoon. In this process, which was called "cutting on the gripe," owing to the absence of a staff, there was always the danger that the urethra might be cut through transversely.

In the plan lately adopted by Sir William Fergusson, the ordinary curved staff was introduced into the bladder, and held as if for lateral lithotomy. The operator then plunged his knife into the right side of the patient's perineum, nearly an inch below the anus, and about midway between that aperture and the right ischial tuberosity. The knife then—its cutting edge being upwards—was carried over the anus and down again on the other side to a position corresponding to that where it commenced. In this way a lunated cut was made, the concavity of which was directed vertically downwards. The forefinger and middle finger of the left hand, with their dorsal surface looking up, were then directed horizontally forwards, so as at once to push back the rectum out of harm's way, and to embrace between them the bend of the staff, furnishing thus an effective guide to its groove, which the knife then entered and completed the operation exactly as if for lateral lithotomy. The difference, indeed, between the process here employed and the usual one lay in the outer cut, which, instead of being directed downwards and outwards from the raphé of the perineum, formed a crescent which included the anus. The position of the staff, the deep incision, and the use of the forceps, were the same as those ordinarily employed.

It was on February 16th that a healthy-looking boy, aged five years, who had shown symptoms of stone for two years, was cut in this manner; and a lithic-acid calculus, the size of a large bean, extracted by means of the scoop. On that occasion, Sir William, in referring to the procedure, illustrated his remarks by some diagrams upon the board, which we reproduce, as, even in the accompanying rough form, they will save much lengthy description.



1. The ordinary incision in lateral lithotomy.
2. The incision often employed by Sir W. Fergusson, which passes from the right of the raphé to the left ischio-rectal fossa.
3. An inverted X incision, which he had more than once employed with the object of gaining space.
4. The incision adopted in the present instance.

On June 1st we witnessed another trial of this procedure. The patient was a muscular man who had suffered for four years from symptoms of stone, and in whom lithotrity had been attempted in the country, but without success. Chloroform was administered, but the man was not bound. For a length of time past Sir William has ceased to bind patients whom he cuts for stone, and has rarely seen any inconvenience arise from the omission.

But here the incision had only been half made when the man convulsively stretched out his legs, completely overpowering the assistants who endeavoured to restrain his limbs. Apparently not disturbed in the least by the novelty of the position which his patient thus occupied, Sir William, without halting for an instant, completed his incision with the utmost facility. The limbs then became sufficiently relaxed to be again brought into position, the succeeding steps of the operation were performed, and a good-sized stone was extracted.

In some interesting remarks which followed, Sir William said that he had long entertained the idea of adopting the process which his hearers had just witnessed; but it was only a few months since that he had first used it. The operation was partly the old one, "cutting on the gripe," described by Celsus, but employed long before him. It had gone out since the celebrated Frère Jacques introduced the grooved staff. At first Frère Jacques, ignorant of anatomy, had plunged a pointed knife into the perineum in the dark, as it were, and opened the bladder with varying success. Later he studied anatomy, and was understood to have been the first to use the staff, the employment of which, however, had been perfected by Cheselden, since whose time the lateral operation had been the one usually performed. Sir William remarked that the use of a staff quite altered the aspect of the old process, "cutting on the gripe." In its performance the knife was passed on and on, whilst two fingers in the rectum pushed the stone towards the operator. The uncertainty of the procedure and its danger put it out of date when the lateral operation was performed by Cheselden and his successors. The especial advantage of the lunated incision was that the cut was formed in exactly the widest part of the perineum, and it was, of course, of great moment to secure the widest space. It was an imitation of the direction and situation of Nature's outlet in the other sex—the vagina,—as the median operation might be considered an imitation of the direction of the vulva. Dupuytren, he said, had used a double bistourie caché, but his example had not been much followed. Indeed, Paris did not now take the lead in lithotomy as it had once done. Probably lithotrity was more often performed there. Sir William thought that the combination of a lunated cut and the use of the staff should be fairly tested. As regards his first case, the patient had done well. The wound, however, was rather long in healing. He could not venture to say that this was on account of its shape. He pointed out the advantage gained by being able to introduce two fingers between the rectum and the staff, pushing back the rectum out of reach of the knife, whilst at the same time this very act opened the wound. In median lithotomy, pressure backwards of the rectum tends to close the wound. No doubt the rectum was not now so often wounded as it was thirty or forty years ago; but it was still frequently cut, more often indeed, he thought, than was generally understood. In the present instance he had got into the bladder with as much facility as usual. He did not make a very free cut into the neck of the bladder, because he found that the stone was not so large as he expected. On touching it with the point of his left forefinger, he ascertained that a moderate opening for the forceps would be sufficient.

The patient made an excellent recovery, and was discharged on July 3rd.—*Lancet, July* 13, 1867, *p.* 42.

73.—CASE OF STONE IN THE BLADDER: LITHOTOMY BY A NEW OPERATION.

Case under the care of HENRY LEE, Esq., at St. George's Hospital.

Mr. Henry Lee recently performed and described a new operation, or rather a modification of the lateral operation for lithotomy. The patient was a boy three years of age, who at one time had retention of urine, and afterwards passed his urine very frequently, and always with pain. The first time he was sounded, Mr. Lee felt a stone; but subsequently he and other surgeons had *sounded without being* able to detect anything in the bladder. A week ago,

under chloroform, a calculus was again felt; and, as the symptoms continued unabated, the operation was determined upon.

After an ordinary grooved staff had been introduced, the operation performed consisted of a straight incision in the median line, extending a distance of a little more than the posterior half of the perineum between the scrotum and the anus, stopping a couple of lines in front of the opening of the bowel. From this point, the incision through the skin was continued outward and backward so as to embrace about one-fourth of the circumference of the bowel, at the angle formed by these two portions of the incision. The scalpel was then introduced in the median line, with its back towards the rectum. It was then passed forward into the groove of the staff, guided by the finger in the rectum. As soon as it had entered the membranous portion of the urethra, it was withdrawn, and a curved bistoury with a projecting probe-point introduced. The probe-point was then made to slide along the groove into the bladder, the edge of the knife being held to the operator's right side, as in Buchanan's operation. The heel of the knife was then made to describe a portion of a circle corresponding to the external incision; while the point, while being withdrawn, was moved a little from the median plane. Mr. Lee mentioned that in this way all the advantages of a free external incision were obtained, with a very small opening into the bladder. The plan had, moreover, the advantage of insuring that the point of the knife had entered the bladder. It was, he thought, an operation the simplest in conception, the easiest in execution, and the least liable to be followed by any untoward accident, of all the modifications of the lateral operations for lithotomy. The groove in the staff was reached with great certainty, being felt by the finger in the rectum. The rectum was secured from danger by the edge of the bistoury being directed laterally. But the greatest advantage, especially in operating upon children, he considered to be the certainty of the incision extending into the prostate gland. It was well known that accidents had occasionally occurred from the prostate not being incised. When this had happened, the finger introduced had sometimes pushed the prostate before it; and in this way the prostate had been detached from the membranous portion of the urethra, and pressed towards or into the bladder, so that a cavity was formed without the bladder having been opened.

In the present instance, a very small stone came away at once between the blades of the forceps, together with some very small fragments which had apparently lodged in the urethra. The stone was so small, that it was doubted at first whether it was sufficiently large to give the sensation experienced when the sound had been used. The cavity of the bladder was, however, carefully explored, and nothing further detected. The symptoms had all subsided on the fifth day after the operation.—*British Medical Journal, Oct.* 12, 1867, *p.* 313.

74.—ON A CASE OF EXTREME IRRITABILITY OF THE BLADDER.

Under the care of Sir H. Thompson, at the University College Hospital.

A patient (a case of supposed stone in the bladder) came under Mr. Thompson's care some time ago, with a bladder so irritable that he could not retain his urine even for a few minutes at a time, passing it as often as twenty-four times in the daytime, sometimes two or three times in the course of a meal. Mental influences had considerable effect upon it: if the patient became at all excited, it came oftener. Yet there was no pain in the penis, no blood in the urine, and seldom any pain in the back. When sounded no stone could be felt, and with all this the urine was perfectly natural and healthy in every respect—in fact, no cause could be assigned for the complaint. The remedy, however, was more apparent, for belladonna, exhibited as suppository and given by the mouth, speedily improved his condition amazingly, but he is not yet well. A hint *apropos* of this case may not come amiss to some of our readers, who have doubtless, been troubled with

cases of unhealthy urine containing much mucus mixed, it may be, with pus—consequently apt to decompose, become ammoniacal, deposit its phosphates, and irritate still more the already irritated bladder. They may have tried all the recognised and time-honoured remedies, including Brodie's favourite, Pareira, and yet have done no good. Under such circumstances, let them not forget a remedy sometimes ordered by Mr. Thompson—a common field plant, *Alchemilla arvensis*, or parsley-piot, an infusion of which (one ounce to one pint) will often succeed where more pretentious remedies have failed. *Medical Times and Gazette, July 27, 1867, p. 92.*

75.—ON A CASE OF SPASMODIC STRICTURE OF MANY YEARS' DURATION.

By Campbell de Morgan, Esq., F.R.S., Surgeon to the Middlesex Hospital.

The frequency and duration of spasmodic stricture are still amongst the unsettled points of surgery. Some foreign surgeons deny altogether the existence of true spasmodic stricture. In England the doctrines of Hunter and Home have taken too deep root to permit of such scepticism. But we find that the extent to which spasm may be regarded as a cause of obstruction to the stream of urine is very differently estimated by those who fully recognise its existence. The following quotation from Mr. Henry Thompson's article on disease of the urinary organs in the "System of Surgery" gives the opinion of a surgeon of great experience, and represents the views of perhaps the majority:—

"It is extremely rare that any considerable narrowing of the urethra takes place as the result of pure spasm of the muscles surrounding the passage. Granted, however, the presence of organic narrowing, or of inflammation in the canal, and an undue action of the urethral muscles may be excited, so as still further to narrow it. There is no doubt that a slight degree of this action may be excited in any part of the passage. ....But it is important to remember that the distinguishing feature which marks the phenomena thus ascribed to irregular muscular contraction, and by which they are contrasted with those of organic stricture, is their transitory character."

In the correctness of the first part of this quotation most will agree, but I doubt whether the second can be received absolutely and without some reservation. Generally, no doubt, the state of spasmodic stricture is very variable, but it is not always so. One sees cases now and then, in which all the signs of a narrow stricture are present, which yield readily to the contact of a bougie, and return rapidly when its use is discontinued, but where after death the appearances of permanent stricture are too insignificant to account for the condition during life. Admitting that a trifling organic contraction is present, the symptoms must surely be attributed to a permanent spasmodic state of the urethra, or to what comes practically to the same thing, a spasmodic contraction occurring whenever the patient endeavours to pass water.

The following case is, I think, a remarkable illustration of the possibility of the occurrence of true permanent spasmodic stricture. The patient—himself a surgeon—gives so circumstantial an account of his symptoms that no doubt can exist as to the fact that he had for years all the signs of permanent organic stricture; the result would show that he had probably never had organic stricture at all.

This gentleman, a surgeon-major in the army, is now 50 years of age. He first noticed the symptoms of stricture as long ago as 1847, after an attack of gonorrhœa, during which the spasmodic difficulty in passing water was so great, and the pain attending the effort so severe, that he always had recourse to the hot hip-bath. When the gonorrhœa had passed off he found that the stream of water was smaller and spiral, and that a few drops of urine always remained in the urethra. He tried himself to pass a bougie, but it seems always used a small one, and failed to reach the bladder. From this time the *stream very* gradually but steadily diminished, and passed irregularly, some-

times to one side, sometimes to another. He did not, however, care to have advice, as the complaint did not cause him much annoyance.

While on duty in Burmah in 1852 he noticed that there was a constantly recurring gleety discharge, and he had at this time a swelled testicle.

On returning to England in 1859 the stream was about the size of a crow-quill, and it took a considerable time to empty the bladder. He now began to suffer much inconvenience. Exposure, especially to night air, caused very frequent micturition, with pain in the lower part of the abdomen and loins. If he resisted the urgent desire to pass water great irritation of the rectum came on. From this time he was never free from these symptoms. The time taken in passing water became more and more protracted. The stream was sometimes thready, never larger than a crow-quill; sometimes the water has come in drops only. Lately he has suffered from pain in the perineum, muco-purulent discharge and irritation at the orifice of the urethra. The discomfort from pain, irritation, and frequent micturition became so great that he determined to undergo regular treatment. He placed himself under my care fully prepared to undergo any operation, by splitting, or perineal secton, so long as he could be quite cured.

The account he gave was so positive and circumstantial that I did not doubt for a moment that there must be some organic stricture. The perineum had a natural feel, but there was tenderness in the scrotal part of the urethra.

Instead of at first passing a full-sized instrument, as I always do, I introduced a No. 6 catheter. On passing it through the scrotal region he complained of soreness, but there was no obstruction. At about the beginning of the membranous part the further passage was stopped. There was not any particular pain at this part, and the resistance was not firm. Smaller and smaller instruments were tried, but none would pass this point. Satisfied that there must be some spasmodic contraction, but at the same time believing that there was organic stricture, I ordered a large dose of bromide of potassium to be taken overnight and on the morning of my next visit. I then found that he had had a quieter night, but the instrument would not pass. A third attempt some days afterwards was made, under the same circumstances and with the same result.

I found there was always tenderness in the scrotal region; and, desirous of ascertaining whether the urethra was narrowed at this part, I now for the first time tried a full-sized catheter (No. 11). There was no obstruction, and I passed it down to the seat of stricture. On making a very slight pressure the obstruction gave way, and the instrument passed into the bladder without the slightest difficulty. The next time he passed water it came away in a full stream; after that it diminished a little, but there was no further difficulty. I passed a No. 11 for him without meeting with any obstruction two or three times afterwards, and sent him home perfectly well, with instructions to use a large catheter if ever he found a tendency to diminution of the stream.

There can be no doubt, I think, that we have here an instance of long-protracted spasmodic stricture, never disappearing, indeed scarcely varying, during some years. It cannot be imagined that an organic stricture of over ten years' standing would at once yield to the gentle pressure of a No. 11 catheter and give no further trouble, when it had resisted the attempts to pass smaller instruments. It is possible that the irritable point in the scrotal part of the urethra may have had to do with the more distant spasm. It is possible that, whatever the source of irritation, the bromide of potassium may have quieted it. This is, however, all conjectural; but there can be no doubt as to the spasmodic nature of the obstruction. The case may be exceptional in point of duration, but it is not the less instructive.—*Lancet*, *April* 13, 1867, *p.* 453.

—OPERATION FOR VARICOCELE.

...m a Report of Practice at University College Hospital.

described below, as performed by Mr. MARSHALL, is in-

...ed vessels were carefully separated from the vas deferens, and a ...aded with silver wire introduced between the two, the needle ...k, and the ends of the wire cut so as to leave a loop at one side ... ...ple of projecting ends at the other. A similar process was gone thro... at the same spot, only in the second operation the loop was left at the side on which the ends were in the former case, and *vice versâ*; also, instead of passing behind the swollen vessels, it passed in front of them, the veins thus separating the parallel wires. The ends of the wires were then passed through the corresponding loops and drawn tight, so as to strangulate the veins. The free ends were then brought together over a roll of lint, for the protection of the skin. When ready for removal a little pushing causes the stout wires to separate at the point where they strangulate the vessels, so that each can be drawn out by its loop. Two, three, or four of these pairs of wires may be used, as the case may require, and the result is satisfactory in the extreme.—*Medical Times and Gazette, July 27, 1867, p. 92.*

DISEASES OF THE SKIN.

77.—ON THE USE OF IODIDE OF LEAD IN CUTANEOUS DISEASES.

By Dr. T. W. BELCHER, M. A., Physician to the Dublin Dispensary for Diseases of the Skin.

[The use of iodide of lead in the treatment of porrigo was introduced to the profession in Ireland by Dr. Neligan, in an article published in the Dublin Quarterly Journal for August, 1848.]

I have lately had under my care several private cases, in which I have used iodide of lead externally with great advantage; as neither in the last edition of *Pereira's Materia Medica*, nor in the exhaustive *Traité de Théra peutique et de Matière Médicale*, by Trousseau and Pidoux; nor even in Professor Macnamara's last edition of *Dr. Neligan's Materia Medica* is much to be found about this medicine, which has been more or less in use for thirty-six years, it may perhaps be useful to give an abstract of a few cases in which it was used externally.

Case 1.—In April last, Dr. Owens, of Kildare-street, asked me to see with him a patient of his, a gentleman advanced in life, who had long been tormented with what proved on inspection to be chronic eczema. The disease was apparent on various parts of the body, but particularly on the legs and thighs; the itching was intense, and many preparations had been tried with little benefit. At first I suggested that an ointment of subacetate of lead containing glycerine and chloroform should be applied to the disease; and I had great hopes that this would produce immediate relief, as I had known it to do in other cases which I had treated shortly before that date. However, it produced little, if any, relief in this case; and accordingly, on my suggestion, Dr. Owens agreed to try the iodide of lead, which was applied in the form of ointment, twelve grains to the ounce, with one drachm of glycerine, and forty minims of chloroform. This procured immediate relief from the intense and aggravated itching: and the patient, having the advantage of frequent supervision from Dr. Owens, gave the remedy fair play, so that shortly after, on my seeing him again in consultation, I was agreeably surprised at the result. The disease had in great part disappeared; we agreed to continue the treatment, adding to the ointment as much chloroform as the preparation would take up. A few days ago I was gratified to learn, from Dr. Owens, that the patient was quite well.

Of course he took constitutional remedies as well as using local ones. It

is doubtful whether in this case the iodide of lead, *per se*, would have cured the disease; chloroform certainly would not; but the conjoined use of the two was most beneficial; and I have no hesitation in recommending a preparation, such as I have above described, to the more general use of my professional brethren.

Case 2.—I was called in to see a young lady who was subject to erythematous eruptions on the face, amounting frequently to erysipelas. At this time the attack was of the latter nature; the face was red and swollen; not painful and itching. Besides giving a purgative, I ordered an ointment of about the same strength as that above mentioned. Its good effect was almost immediate. In two or three days I saw her without a trace of the affection for which I had visited her. This was always before that time constant to a greater or less degree; and had been more or less benefited by various local and constitutional medicines; but now it wholly disappeared, and has not since returned, so far as I know.

Case 3.—A young gentleman, an undergraduate of one of the colleges in Oxford, of excellent general health, given to open-air sports, and without any evidence of personal or hereditary syphilitic taint, consulted me a few months since for psoriasis, which he had on most parts of his body, save his face. I directed him to take Neligan's iodurettted solution of the iodide of potassium and arsenic, described on page 268 of my edition of his work on Diseases of the Skin; and to use locally Hebra's tincture, which I have described on page 114 of the same work, and also in a paper in the number of this Journal for May, 1865. He was further directed to take vapour and tepid baths, and physical exercise. I have seen him several times since his first visit; and always with the evidences of marked improvement in his case. At length I stopped Hebra's tincture, and a similar preparation of rectified spirit, soft soap, oil of cade, and oil of lavender, which for a short time previously I had substituted for the former; and prescribed for him instead of them the following ointment:—"Iodide of lead, twenty grains; simple ointment, seven drachms; glycerine, one drachm."

He was directed to continue the constitutional treatment as before. In about a fortnight I perceived the disease to be greatly improved; in fact, the psoriasis may be said to have disappeared, so far as external appearances went; and the skin had quite regained, in most places, its natural colour and texture. I know the amelioration must be largely due to the use of the constitutional means above noted; but in no case treated throughout as this was at the outset (see above) have I seen the same speedy repair of the skin and its function as was so very remarkable in this instance.

I might mention several other private cases in which I tried it with quite as much benefit as in the above, but they would inconveniently extend the length of these observations. The ointment of the iodide of lead of the present *Pharmacopœia* (1867) I conceive to be very much too strong for such cases as I have above noticed. It contains sixty-two grains to the ounce; whereas from a fifth to a fourth of that quantity is quite sufficient, and more useful than the pharmacopœial strength. In the cases of porrigo, in which Dr. Neligan first used it, he put thirty grains to the ounce; and therefore we read with little surprise that in some cases "iodide of lead ointment excites a certain degree of inflammation." No such result follows the use of the weaker preparation; but the stronger is nevertheless useful in other conditions of the skin than those I have described.—*Dublin Quarterly Journal, Aug.*, 1867, *p.* 85.

78.—OBSERVATIONS TENDING TO SHOW THE IDENTITY OF THE FUNGI OF FAVUS AND TINEA CIRCINATA.

By Dr. John M. Purser, A. B., Demonstrator of Anatomy in the Carmichael School of Medicine, Dublin.

In an interesting paper, by Dr. McCall Anderson, recently published, in which he attempts to prove the non-identity of the fungi which produce the

diseases known as favus, tinea tonsurans, and pityriasis versicolor, the following statement occurs:—"Of the numerous instances on record of the transmission of favus and tinea tonsurans *from the lower animals* by contagion or inoculation, favus has always given rise to favus, and tinea tonsurans to tinea tonsurans." This conclusion is supported by several cases both from Dr. Anderson's own practice and from that of Bazin, Gerlach, Bärensprung and Kobner, and, so far as I know, at the time of the publication of the paper in question no instance of a contrary nature was extant. The following cases, therefore, in which favus appears to have produced tinea circinata (which Dr. Anderson and most other dermatologists believe to be identical, as far as the fungus is concerned, with tinea tonsurans and sycosis), both by accidental contagion and intentional inoculation, may be of some interest as bearing on the question, still unsettled, of the identity or non-identity of the fungi which occur in the different epiphytic diseases of man.

In October last I was called on to attend a family, four members of which, all adult females, were suffering from ordinary tinea circinata. The spots affected the hands, arms, and shoulders; were of various sizes; presented a centre of sound skin and a spreading furfuraceous edge; itched a good deal; and, in short, presented a typical example of ordinary ringworm, so much so that, contrary to my usual custom, I neglected to make any microscopic examination of the epidermic scales. The disease was recent, and yielded readily to treatment. A few weeks afterwards, when the spots were all well, or nearly so, on talking over the possible manner in which the disease might have been contracted, I was asked by one of the patients if it could have been taken from a cat, mentioning at the same time that one of her cats had had a spot on its paw, which she, thinking it was dirt of some kind sticking in the hairs, had tried to pick off, but found that her attempts made the paw bleed. Shortly after this she remarked spots of ring-worm on her own hands. On inspecting the cat I found that it had on its left fore-paw a small spot, from which the hairs were almost completely absent; this was covered by a raised crust of dry powdery consistence, and sulphur yellow colour, through which passed a few stunted hairs. There was no cupped appearance, but the naked eye characters of the crust resembled much those of some varieties of favus. I learned further, that another cat living in the same house had had a similar patch on its nose, which was now well. I inquired as to the mice, but could get no reliable information. Microscopically, the crust from the cat's paw was seen to consist almost entirely of the spores and filaments of a fungus. The spores were large, many of them oval, and present in considerable quantity. The filaments were branched and abundant. These appearances were identical with those which I had seen in cases of undoubted favus; but mistrusting my own limited experience in a matter of such delicacy as the diagnosis of a fungus, I sent a portion of the crust to Dr. Tilbury Fox, whose opinion on such a subject no one will be inclined to question. He pronounced the fungus to be an admirable specimen of the *achorion schönleini*, the parasite of favus. In the meantime I had inoculated my own forearm with the fungus, by scraping off the superficial layer of epidermis, and laying on the denuded place some of the crust reduced to powder, and covering the whole with a piece of sticking plaster. The plaster was removed after a hundred and twenty hours, during the latter part of which time there was intense itching of the inoculated part. There then appeared a circular spot on the forearm, the shape and size of the plaster. The centre was covered by large, loose flakes of epidermis, separated probably in consequence of the retention of the perspiration. They adhered by only a few points to the subjacent parts, which consisted of skin, reddened and softened from absence of horny epidermis, but presenting no sign of eruption. Around this was a slightly raised and furfuraceous border, and outside this again a circle of inflamed skin covered with minute vesicles. After exposure to the air for some hours, the central scales separated; the skin beneath assumed a more healthy colour and appearance, while the spot continued to spread centrifugally by the formation of successive crops of vesicles. These vesicles were extremely small, of subglobular shape, formed with great rapidity, and

dried up quickly, leaving a furfuraceous crust. There was some difference of opinion among the physicians to whom I showed the spot as to what the disease should be called; but those who carefully observed the healthy centre and the spreading edge thought that it most resembled herpes circinatus, while all agreed that it had not the least resemblance to favus. Part of the crust was found by the microscope to consist chiefly of epidermic scales, mixed with which were a few pus or exudation corpuscles, a considerable quantity of the mycelium of a fungus, and a few spores. A specimen was sent to Dr. Tilbury Fox, who stated that the mycelium presented the characters of *trichophyton* rather than *achorion*; that it differed from the latter in being smaller, less freely branched, and containing fewer granules. The spores were small and round, none large or oval. He further says, "You clearly have a condition which presents, microscopically, the characters of tinea circinata (herpes tonsurans), but which may be only an early stage of favus; but this is exactly what, I argue, arises." Dr. Fox was also so kind as to show specimens of both fungi at the Pathological Society of London. After some days, as the spot continued to spread with unaltered characters, and as the itching had become so severe as seriously to interfere with my comfort, the crusts were removed. They were never renewed. The patch spread but little after that. It gradually died away with considerable desquamation, leaving a brown, discoloured spot, which persisted for several weeks. I had intended to attempt re-inoculation from my arm to a cat or rabbit, but was prevented at the time. Subsequently a second inoculation was made on another part of my forearm, which produced a diseased patch, presenting the same appearances, both to the naked eye and microscopically, as the former, but before I could continue the experiment, circumstances occurred which obliged me to relinquish the entire investigation.

It is unnecessary here to dwell on the mere fact of the transmission of disease from the brute to man, an event which is universally allowed at present to be not only possible but of frequent occurrence; but, as I have already stated, in all the hitherto recorded cases, favus has produced favus, and tinea tonsurans tinea tonsurans, whereas here we have favus in the animal giving rise, by contagion, to tinea tonsurans in the human subject; and the fungus, which, when growing on the hairy skin of the cat, under what we may suppose favourable conditions, appeared as *achorion*, with large spores and well-developed mycelium, on being transplanted to the human skin, where the hair follicles were few, and moisture deficient, changing its form to that of *trichophyton*, which is considered by many to be only an imperfect development of *achorion*. A change of a somewhat similar kind, produced also by transplantation of the fungus from a hairy to an uncovered part, is mentioned by Dr. Fox; but in this instance both forms of disease occurred on the same patient, favus of the scalp giving rise to tinea circinata of the shoulders and neck.

It is not my intention to go over all the evidence which has been adduced by writers to show that the apparently different fungi met with on man are all varieties, or different stages of growth, of the same plant, the diversity in form being due to differences in the soil on and the conditions under which they grow. It is, indeed, difficult to understand the opposition which, in the face of such evidence, this view still receives, at a time when the limits of variation in form of animals and vegetables, produced by external circumstances, is acknowledged by naturalists to be so wide. When we consider the slight differences in conditions of growth, which, in experiments on the germination of fungi, have sufficed to produce very different forms, we should rather be surprised that the variations occurring in the transmission of parasitic fungi from one person to another are so rare. Thus we read that "Dutrochet took a solution of albumen, and examined it from time to time. At the expiration of a year nothing had occurred; he added some acid, and monilia was produced; conversely he added an alkali, and botrytis resulted. Thinking that the agency of the reagent might have had some special influence, instead of albumen he used fibrin, and, curiously enough, he obtained just the very opposite result. Fibrin and alkali grew monilia, and fibrin and

acid botrytis." There is some reason to believe that if the history of the source of contagion in each case of parasitic skin disease could be made out, and if more care were given to the determination of the microscopic characters of the fungi, instances of variation in the latter would be found to be more frequent than is at present supposed.

In conclusion, I have to notice a paper, lately published by Dr. Salisbury, in which he describes a disease very common in cats, produced by the growth of a fungus, and very readily spreading by contagion to human beings, giving rise in them to a disease very like ordinary ringworm, but differing from the latter so much as to lead Dr. Salisbury to call it "a newly discovered disease," and to give it a new name, derived from its origin, *trichosis felinis*. It affects principally young cats, rarely spreading to their elders. The milk with which the paws and face of kittens are often smeared is believed to furnish a suitable soil for the germination of the fungus. Of human beings children are chiefly attacked. In them the disease spreads in isolated patches to all parts of the surface, often in a few days' time. The patches are of various size, circular or oval in shape, slightly elevated above the surface, red and covered with scales and little elevations, marking the positions of the hair follicles. The colour of the patches is deeper, and the irritating and itching more severe than in ordinary *trichosis*. The fungi also differ. "In ordinary ringworm (*trichosis furfuracea*) the fungoid cause exists mostly in the spore state. The plant does not advance beyond its cell condition. Its growth seems to be confined simply to cell-multiplication by pullulation. In this disease (*trichosis felinis*) the plant cells multiply by pullulation, and these advance to the filamentous stage of growth." From the drawings of the fungi which Dr. Salisbury gives, the spores are seen to be oval, containing one or two nuclei, provided with buds or off-sets, or occurring in chains. The mycelium is branched, jointed, with cellular interruptions, and contains no granules or spores. These characters are unaltered by the passage of the fungus from the cat to the child. In this respect the cases differ materially from those detailed in the present paper.—*Dublin Quarterly Journal, Aug.*, 1867, *p.* 66.

79.—THE PERMANGANATE OF POTASH IN THE TREATMENT OF CARBUNCLE.

By Dr. Thad. L. Leavitt, Germantown, Pa.

[The beneficial results of the local use of permanganate of potash in the treatment of sloughing ulcers, phlegmonous erysipelas, and hospital gangrene, having been thoroughly tested in America during the late war, it follows naturally that like good results will probably ensue from its employment in the treatment of carbuncle.]

Mrs. R., aged about 60 years, was visited, during the absence from town of her family physician, and found suffering terribly from a carbuncle located upon the left shoulder-blade, just above the spine of the scapula, and occupying the supra-spinous fossa. Loss of sleep, constant pain, and a naturally nervous temperament combined, induced a mental disturbance almost amounting to delirium. The tumour was in its sixth day, with all the general accompaniments, of the size of a hen's egg, tumid, tense, and shining. A free crucial incision had been made two days before, but with no relief; dense areolar tissue, puffy granulations, and sanious oozings crowded the track of the knife, with no appearance of separation or healthy action. The pulse was quick and compressible, 110 beats in the minute; countenance anxious and expressive of great pain; bowels regular. A strong solution of the permanganate of potash (ʒss. to f. ʒ j.) was immediately applied with a brush, and a dressing saturated with it, covered with oiled silk, placed upon the shoulder. Anodynes, beef-tea, milk-punch, tincture of the chloride of iron, and quinia were administered. The same evening, the patient was again seen, and expressed herself as feeling much relieved; pulse 98, and gaining in volume and elasticity. The next morning the dressing was removed, and already, although

but twenty-five hours had elapsed, true pus had begun to form, the intense pain had subsided, and the patient, to use her own language, declared it "a miracle;" the pain had vanished, the fever was gone; she had slept well, and felt some appetite for food. A few days longer the potash was continued; the slough separated, and the wound healed in the short space of one week.

Mr. C., aged 50 years, shoemaker; was visited July 30th, 1866. Had been sick three days, was found suffering intensely from a carbuncle, situated upon the abdomen just below the umbilicus, of the size of a large walnut, and involving the surrounding structures in an erysipelatous inflammation. Bowels constipated; high fever; pulse 120; heavy breath; tongue furred; anxious countenance; great restlessness and general uneasiness characterised his principal symptoms. Hop and laudanum poultices had been applied, but he had been gradually growing worse, and approaching the position described, the tumour increasing daily, the parts becoming more dense, and at last an ichorous pus exuded from several small openings. Mild purgation, after which supporting and stimulant treatment was instituted. A slight incision was made, and the permanganate applied, as in the previous case, the dressings being removed once in twenty-four hours. This case was seen seven days successively; the 13th August he returned to his work, the severity of the suffering having been arrested after the first application.

Mrs. A., aged about 49 years, having suffered a few days from a supposed furuncle, and the pain becoming intolerable, called in medical aid. There was found upon the inner face of the left thigh, just below the nates, a well-marked, though small, carbuncle; a very slight incision was made and the potash dressing used. No constitutional treatment at all was inaugurated; in three days all signs of carbuncle had disappeared and the line of incision was healing nicely.

The results in this case were mutually gratifying, from the fact that about six years ago the patient suffered from a series of carbuncles appearing in succession, along the spinal column, from the back of the neck to the region of the lumbar vertebræ, and, lasting all through the winter months, her dread and fear of similar suffering were very great. The permanganate of potash has been eminently successful with me in the treatment of chronic ulcers. The following case, of many years' duration, and which had resisted all efforts, yielded to the remedial qualities of this preparation:—

Arthur M., tavernkeeper, aged 45, had a chronic indurated ulcer, of sixteen years' standing, extending over the superior face of the right leg, about four inches below the tubercle of the tibia, and spreading backward on both sides to the malleoli, covering a surface of about twenty-eight square inches, deep and burrowing in some localities, and in others merely superficial; the whole leg and foot were much swollen and anasarcous, the toes merely protruding from a shapeless mass of flesh, closely resembling the foot of a young elephant. An ichorous discharge of a horribly offensive character, together with filthy dressings, augmented the destruction of the surrounding parts.

The advice of an eminent surgeon had been secured a few weeks previously, to the effect that but one alternative remained, amputation; and indeed, all appearances favoured such a decision. Proper abstinence, tincture of iron and good diet were directed. The local use of a strong solution of the permanganate of potash and judicious bandaging have already done so much for this case that, at the date of writing, the tenth application of the potash, six square inches, will more than cover the small amount of ulceration remaining, so rapid have been the healing process and the formation of firm, healthy tissue; and, in a few days more, we can confidently prognosticate a complete cure.—*Amer. Jour. Med. Sciences.*—*Canada Medical Journal, April*, 1867, p. 462.

80.—SULPHURET OF CALCIUM IN SCABIES.

VLEMINCKX uses in scabies the sulphuret in the following manner:—Quick lime, one pound; *spring water, a sufficiency*; sulphur, two pounds; water

twenty pounds. Boil to twelve pounds. The patient is placed in a tepid bath for half an hour; then, on leaving it, all the parts affected are rubbed with a piece of flannel dipped in this solution. The patient is then placed in a second tepid bath for another half hour. The same measures are repeated the next day, and the itch is conquered. Eczema may follow this mode of proceeding, and this complication may take several days to control. —*Lancet, Sep.* 21, 1867, *p.* 361.

81.—ITCH IN DELICATE SKINS.

Hebra uses the following compound:—Petroleum and spirit, of each one ounce; balsam of Peru, one fluid drachm; oil of rosemary and lavender, of each fifteen minims.—*Lancet, Sep.* 21, 1867, *p.* 361.

SYPHILITIC DISEASES.

82.—ON THE REPORT OF THE VENEREAL COMMISSION.

By Holmes Coote, Esq., Surgeon to St. Bartholomew's Hospital.

The Report of the Committee appointed by the Lords of the Admiralty to inquire into the best mode of treatment of the venereal disease, has at least had the good effect of clearing the subject of much that was ambiguous, theoretical, or irrelevant, and has left it open to yet further inquiry. A syphilitic virus is recognised; the disease is pronounced as specific as small-pox. But here the resemblance must cease: there is no further analogy between the two diseases; and I trust never again to see repeated those cruel experiments by which it was attempted to render the body insusceptible to the further action of the virus by repeated inoculations.

The weight of evidence went to prove that syphilis, under favouring circumstances, might be generated spontaneously. And permit me to ask, as regards this point, in what respect the history of syphilis differs from that of any other known disease? Do we know the origin of small-pox, of measles, or of scarlatina? Can we explain the nature of the cholera poison? To say that syphilis may possibly have been derived from the horse is a purely fanciful statement, which is unworthy of any scientific work.

It being, then, established, first, that there is such a disease; and, secondly, that there is a specific virus,—the Committee next proceeds to the subject of primary sores; and here, in my opinion is the first break-down.

Two species of sores are described—namely, first, the "simple local sore, the influence of which never extends beyond the inguinal glands, but is eminently contagious, though incapable of infecting the constitution. This is the most common, and prevails over all other varieties in a ratio of about 4 to 1:" secondly, the syphilitic sore, which, according to this Report, is not always indurated, as was the prevailing opinion some weeks ago, but which may be indurated throughout its entire course; or soft in its early stage, and subsequently indurated, or soft throughout its entire course, but which, unlike the simple local sore, is followed by constitutional disease. The evidence, further, is conclusive that it is impossible to pronounce with certainty on the characters of a sore on its first appearance.

Now, I maintain that such conclusions as these are useless to us in a medical point of view, and may become mischievous to our patients. The theories about "induration" as characteristic of syphilis are gone. The soft syphilitic sore cannot be distinguished from the soft non-syphilitic sore, except by its progress and history. If, according to the Report, a rash come over the body, it is syphilitic; if not so, it is a simple sore. But the surgeon, who honestly entertains this belief, can only reply to his patient's anxious inquiries respecting the character of the disease, "I cannot tell you its nature until it has *come to an end.* If you suffer from constitutional symptoms, it is syphi—

litic; if not, it is non-syphilitic." The patient might reply, that he could have told the surgeon as much himself.

Such arguments remind me of those once entertained respecting the use of mercury. If the sore is cured by mercury, it is syphilitic; if not so, it is non-syphilitic.

But these arguments may become mischievous. Is there a sane surgeon living who, if his life depended on it, would undertake to guarantee a patient, the subject of any form of venereal sore whatever, that he should be free from the occurrence of constitutional infection? Should there be any one who really attaches importance to the matter of "induration," I could speedily produce evidence to bring him to a very different opinion. That some persons suffer constitutionally, and some do not, is a matter of general notoriety; but surely all individuals are not equally susceptible of general syphilitic infection.

According to this Report, the frequency of constitutional symptoms to primary sores of all kinds should be four to one. Taking the female venereal cases which are in constant succession under my care, I should pronounce the number at least one-half; and then, many who quit the hospital apparently well, and not generally infected, suffer from secondary symptoms afterwards. So also, among the men, secondary symptoms are common; but the induration of the base of the primary sore is of rare occurrence, and is scarcely ever seen in women.

I see no reason to change my opinion from that which has been already published and expressed—namely, that there is but one venereal virus, the action of which is usually chronic, and always ulcerative. The character of the sore thus produced depends, as in any other case, on the nature of the tissue on which it acts; and, *cæteris paribus*, the same tissue presents the same kind of venereal ulcers. These ulcers are not precisely similar in the male and female; nor in the former are they always the same, inasmuch as they vary in character just as often as the tissues composing the penis vary in their intimate structure.—*British Medical Journal, June* 22, 1867, *p.* 728.

83.—OIL OF SANDAL-WOOD IN GLEET.

Mr. Berkeley Hill, of University College Hospital, has tried the oil of sandal-wood in nineteen cases of urethral discharge of some weeks' or months' duration. Its efficacy, though uncertain, is often very marked. In several cases, it arrested the discharge by the third day; and, when taken for seven days, prevented any further return. Of the nineteen cases, the oil was of marked benefit in thirteen. In six, no benefit resulted from its use; and in four of these, any dose, however small, produced nausea or disturbance of the stomach. In two of the six the drug had no controlling effect even when taken in two-scruple doses twice daily.

Of the thirteen favourable cases, cubebs and copaiba had been employed previously in seven without any advantage. In three of the seven, the copaiba produced too much disturbance to be borne; so that from these it would seem that some stomachs tolerate oil of sandal-wood that cannot bear copaiba. In all the patients one injection, in some several, had been employed before the oil was prescribed.

In the nineteen cases, the dose most often tolerated was fifteen drops, taken three times a day, with a little liquor potassæ, in peppermint-water; but eight drops in two patients had the desired effect, while forty drops were taken by three patients at a dose without producing intolerance. In these cases all other treatment was laid aside while the oil was being given. The oil was generally taken a week before the patient was able to leave it off.

Mr. Hill has not employed oil of sandal-wood in the acute stage of gonorrhœa. In these cases he gives alkalies in frequent doses, with or without purges and anodynes, as required.—*British Medical Journal, July* 6, 1867, *p.* 7.

84.—STARCH INJECTIONS IN GONORRHŒA.

Bismuth has of late been largely employed with a view of obtaining a deposit of the powder into the follicles of the urethra. M. Luc, a military surgeon, proposes a cheaper injection of the same character—viz., finely-powdered starch mixed with lukewarm water, so as to obtain a fluid of the substance of cream, but thin enough to allow of injection. M. Luc has been very successful with this compound, either in painless discharges, or where the inflammatory stage was over.—*Lancet, Sept.* 21, 1867, *p.* 361.

85.—USE OF THE SPRAY-PRODUCER IN SYPHILITIC ULCERATION OF THE THROAT.

Case under the care of Dr. Murchison, at the Middlesex Hospital.

[Speaking of Dr. Richardson's now common spray-producer, the writer observes:]

By means of this little instrument a fine spray of sulphurous acid was showered over the tonsils and palate of a man who was suffering much distress from syphilitic ulceration of the throat. The improvement which took place was very rapid. The smell and bad taste which had been a great source of annoyance ceased immediately, and the unhealthy aspect of the sores gave place quickly to signs of healing. The man expressed himself very strongly as to the marked relief which the sulphurous acid, thus administered, had given him, and on looking into his mouth, some days after the application, we found the throat all but healed. The spray, he told us in reply to a question, produced no smarting.—*Lancet, Oct.* 19, 1867, *p.* 486.

DISEASES OF THE EYE AND EAR.

86.—ON THE EXAMINATION OF THE EYE WITH THE OPHTHALMOSCOPE.

By Dr. Henry Wilson, Member of the Royal Irish Academy.

[All that is required for purposes of illumination with the ophthalmoscope is a steady broad flame. It need not be very large, or very brilliant; on the contrary, a subdued light is to be recommended as being more grateful to the patient, and more suitable for the recognition of finer details. A German reading lamp, with the shade removed, is the illuminator generally used by the writer of this paper.]

For ophthalmoscopic purposes it is well to have the glass chimneys faintly tinged with blue, so as to modify the light. The flame should occupy a position behind the examined eye, so that its rays shall not directly impinge on the eye. I prefer, as a rule, having the light in one given position—a little above and behind the middle of the patient's head: others, however, prefer the light behind and to the side of the patient's right ear; and this position is probably the best where the examiner uses his right eye only. All direct sunbeams or other light which might interfere with the rays going to or from the examined eye, must be avoided. It is, however, not necessary that daylight should be completely excluded; a small amount of diffused and modified daylight may be present, and will be found very convenient. Sunlight is also employed in ophthalmoscopy, but principally in warm climates, and where the facilities we possess in our cities cannot be had. It is admitted through a circular orifice in the window, shutter, or door, or reflected by a plain mirror into the darkened chamber through the open door or window.

The eye should always be first examined without dilating the pupil arti-

ficially, as there are various objections to the latter proceeding. When the patient sits with his back to the light for a few minutes in the darkened chamber, and regards some distant object, the pupil will generally be found sufficiently dilated naturally for an experienced examiner to see the fundus, and to ascertain, in a large number of instances, the cause of the impaired vision. If, however, the pupil remain too small, or it be desirable or necessary to obtain a view of the whole of the lens or of the fundus, it can be readily dilated at any time; for the purposes of teaching, demonstrating, or learning also, it is well to have the pupil dilated; the substance used for the purpose is belladonna or its alkaloid atropia, the latter being preferable on account both of its activity and its cleanliness; half a grain or a grain of sulphate of atropia to the ounce of distilled water will suffice to dilate the pupil in healthy eyes in about half an hour (the strength of the solution I generally employ in diseased conditions of the cornea, iris, or lens, is one grain to the drachm). Where much inconvenience may be arise or cosmetic effects demand it, the pupil may be again contracted by a solution of the extract of Calabar bean.

Prior to studying on the human eye it is advisable to practise ophthalmoscopic illumination of other objects. The beginner should take the plate representing the arteria centralis retina in Gray's Anatomy, place it upright in front of a lamp or candle, so that it shall be in the shade, and then practise illuminating it from various positions, with and without the interposition of the lens, and with each eye alternately. Rabbits' eyes afford an easy and a beautiful object for study; the fundus in these animals differs, however, from that of the human eye; the eyes of persons, who are completely and incurably blind from cerebral disease, may be selected by the beginner, with advantage to himself, and without inconvenience to the patient; highly myopic eyes should also be selected, as the fundus becomes very readily visible.

The following observations refer, in particular, to the examination of the reversed or aërial image with Liebriech's small ophthalmoscope. The patient should be comfortably seated beside or with his back to a table, on which the light is placed; he should hold his head fully erect in one fixed position, and not move it about from side to side. The examiner should occupy a seat a little higher than that of the patient, and have his eyes on a somewhat higher level than those of the patient; he should also fairly front the observed. The distance between the two faces must depend on the refractive condition and the power of accommodation of both eyes. When these are normal it will be about eighteen inches—a short and convenient distance. I myself prefer standing in front of the patient for short examinations, and having the patient's head slightly inclined backwards, with the light vertically above and a little behind it. When dexterity and facility in examining have been acquired, this method affords many advantages; it allows of greater control over the patient; the examiner can move more readily in all directions, practise oblique illumination, and examine a number of cases in succession more expeditiously.

All preliminaries being now arranged, and the left eye the one to be examined, the transparent media are to be observed by oblique illumination; and when it has been ascertained that these are transparent, the mirror is to be applied into the angle formed by the eyebrow and nose on the left side somewhat in the same manner as an eyeglass; the central aperture in the mirror should then be opposite the observer's pupil; the handle of the instrument is to be held horizontal, its extremity being grasped somewhat in the same manner as a flute between the thumb below and the three first fingers above; the little finger should be free, project beyond the handle, and be held erect. The upper arm may be retained at the side, or project from and be at right angles with the body. While being kept close pressed into the angle between the nose and eyebrow, the mirror should be moved vertically by rolling the handle between the fingers and thumb, and horizontally by movements of the handle backwards and forwards until the light is made to fall on the eye under examination. It is most important to recollect that the patient and examiner must retain their original facing position.

The right or non-examining eye may be kept open so as to control the patient's movements and the proper position of the reflected light; it should, however, be occasionally closed, so as to make sure that the left or examining eye is looking through the ophthalmoscope opening. When the patient is unsteady it is advisable, in the beginning of the examination, to place the open right hand on his head, so as to keep it in the erect position, and prevent its moving. The examination having proceeded thus far, and the eye being now fairly and steadily illumined, the observer should perceive a brilliant red glow behind the pupil, which will be fixed or transitory according as the illumination or position is properly maintained or not. The patient is now desired to regard (without moving his head) the point of the observer's erect little finger, and when he has done so the red brilliancy will assume a somewhat white character, which indicates that the optic nerve entrance is then in the exact visual line. While the patient keeps steadily regarding the point of the little finger the examiner takes one of the large convex lenses —two inch focus—between the finger and thumb of his right hand, and holds it about two inches in front of the eye in such a manner that the rays from the mirror shall traverse it before entering the examined eye; the hand carrying the lens may be steadied by resting the little finger on the malar bone or the brow of the patient. The iris should not enter into the formation of the visual field; the lens must be moved to and fro until the image of that membrane and its pupillary margin disappear altogether, and nothing remains but the circular bright red field. These directions being followed, and the patient's and examiner's eyes being normal, the latter should perceive the image of the fundus. In regarding this image, its position between the lens and the observer should not be forgotten, and the latter should, therefore, not endeavour to look through the lens, but should accommodate or adjust his vision for a near distance, and suppress the vision of his right eye. The lens must be moved from or approximated to the observed eye, or moved on its axis until the greatest degree of distinctness in the image is obtained; it must also be held quite vertically (to the axis of the eye), as otherwise inaccuracies, deceptions, or distortions may arise, which would lead to erroneous conclusions.

The entrance of the optic nerve into the eye is not exactly in the optic axis, but a little below and to its inside, having the yellow spot external to and on a level a little above it; the patient's eye must, therefore, be turned inwards and a little upwards, in order to bring the optic disc into the visual line, and the point of the erect little finger affords the most convenient and generally the best object for fixation. When it is wished to observe the position of the yellow spot or its vicinity, the little finger is closed, and the index finger is raised close to the mirror for the patient to regard; in this manner each finger may be raised successively, and a prolonged and steady examination made of the most important portions of the fundus. The little finger may be armed with a bright polished metallic thimble, which will readily attract the attention of amblyopic persons.

In the examination of the right eye the proceeding should be similar to that just mentioned, except that the ophthalmoscope is to be held in the right hand and to the right eye, and the lens in the left hand. We should in all instances make a comparative examination of both eyes.

I would particularly recommend this mode of examination to all who have equal vision in both eyes; it affords great facilities; the optic papilla comes into view the moment the patient regards the point of the extended little finger, and the hand carrying the lens is never in the way. When from imperfect vision in one eye, or other cause, the examiner can only use one of his eyes, he should have the light placed beside and a little behind the patient on the side of the examining eye, and direct the patient to look inwards and a little upwards towards the examiner's ear; thus, if the right eye be the examining one, the light is to be placed on the patient's left side, no matter which eye is to be observed; and if the left be the examining eye, the light should be on the patient's right side; the ophthalmoscope is to be held by the hand on the same side as the observing eye, and the lens in the opposite

hand; by this means the illuminating rays are not intercepted. Where the patient is quite blind, or from other cause cannot fix his vision on the little finger, or in children, the examination is more difficult; in the case of adults the individual's hand may be placed in the wished-for position, and the person desired to look towards it; in the case of children the parent or attendant should occupy the position the child is to regard, and attracts its attention by speaking to it, or holding up a watch or other bright object. If we fail by these means to obtain a view of the optic disc, we must alter our position. While the patient retains his head erect, and looks straight in front, the observer moves to the temporal side of the eye under examination, and getting about a three-quarter view of the patient's face, illumines the fundus in that position, when he will, as a rule, be able, with some little difficulty, to obtain the desired view. Where the eyelids are not opened sufficiently wide, the upper lid may generally be raised by the two middle fingers of the hand carrying the lens, these being disengaged while the thumb and index finger hold the lens, and the little finger rests on the malar bone. It should be our object, first, to obtain as extensive and general a view of the fundus as possible, so that any isolated irregularity or lesion will present itself at once as contrasting with the surrounding parts; and we should endeavour to have the optic nerve forming the centre of the picture. A slightly concave, or even a plane mirror, and a convex lens of 1½″ focus will give the desired results. Subsequently the individual portions may be magnified by low-powered object lenses, such as those of 3″ or 4″ focus, or by magnifying the aërial image by a convex ocular lens inserted into the clip behind the mirror.

The beginner must guard against being led into error by the *reflections* which are caused by the cornea and the convex lens; these reflections are at all times more or less present, and are sometimes very embarrassing. One of these reflections is a small but exact image of the ophthalmoscope mirror, and is liable to be mistaken for the optic disc. It may, however, be distinguished from this by the dark spot in the centre, answering to the orifice in the mirror, by the absence of vessels and by its position; these reflections may generally be obviated by slight movements of the lens on its axis, and to and fro. Occasionally a thread or film of mucus adheres to the cornea, and simulates, at first sight, an opacity in the transparent media; when such occurs, it must be removed by gently rubbing the upper lid over the globe.

The student must not be discouraged by getting mere momentary and passing glimpses of the optic disc. It requires considerable practice before we are able to obtain a steady and prolonged view of the parts. When the disc comes into view, and then suddenly disappears, it is attributable to some movement or unsteadiness on the part of either examiner or patient—most frequently, I think, of the former. When in searching for the optic papilla, a vessel comes into view, it should be steadily regarded and traced backwards in the direction of its increasing thickness, until finally the papilla is found where the vessel appears to terminate.

As I have already explained, the convex object lens produces the inverted image; and in reference to this image Mr. Carter, in his excellent translation of Zander's *Augenspiegel*, or rather his new, improved, and annotated edition of that work, says:—"In order to understand clearly the effect of the inversion, it is well to take a piece of thin writing-paper, and to draw upon it a circle to represent the fundus of the left eye of a person placed opposite to the spectator. The circle may be surrounded by the letters Na Su Te In, so arranged as to indicate the superior, inferior, temporal, and nasal boundaries of the figure. To the nasal side of the centre a small circle may be drawn for the optic disc, with lines radiating chiefly towards the temporal side to represent the blood-vessels. The sketch, as it stands, will give an idea of the erect image, with its temporal side to the right of the spectator and the vessels passing to the right. If the piece of paper be turned bottom upwards, with the written side still towards the spectator, the ordinary conditions of

the inverted image will be fulfilled, and the inversion of all parts will be complete."

The chromo-lithograph, copied partly from Jaeger's *Beitrâge zur Pathologie des Auges*, gives an idea of what the student is to look for; it represents the inverted image of the fundus of a healthy left eye, of a dark-complexioned adult. Although emanating from a concave surface, the image, as seen by the monocular ophthalmoscope, presents a flat appearance; at its centre is the optic papilla, contrasting markedly by its whitish colour with the surrounding parts, which are of a brilliant uniform red colour, traversed by fine dark-coloured lines, representing the retinal vessels. The red colour varies according to the individual; it may be a very pale or very dark red, or may contain a yellow or orange, or even a greyish tint. The fundus, as already stated, consists of the optic nerve, the retina, choroid, sclerotic, and vessels and nerves belonging to these structures.

The intra-ocular end of the nerve, termed *optic papilla*, or *optic disc*, is circular or nearly so, and, as contrasting with the surrounding surface, appears of a white colour; it is, however, of a soft cream colour or pinkish grey, and not unfrequently conveys the idea of relief; it forms the most prominent and important feature in the image; it may be either sharply demarcated from the surrounding fundus, or may be partially or completely surrounded by one or two circles indicating the sclerotic and choroidal openings—the former white, the latter dark coloured. When the choroid is abundantly pigmented close up to the optic nerve, there is no appearance of the white or sclerotic ring round the disc, but when it is not we look down upon the sclerotic opening, which appears as a whitish ring round the nerve. Occasionally we find at one side—generally the outside—of the disc a very dark or black semicircular line; this is simply an accumulation of pigment in the choroid at this situation, and is seen in perfectly healthy eyes. Starting from the centre of the disc we find the branches of the centralis retinæ artery and vein, the former are the smaller of the two sets, and light red in colour. As a rule the artery, after emerging from the porus opticus, divides into two main stems, one above and the other below; these subdivide and branch out into numerous small stems, which take a direction towards the temporal side, coursing somewhat circularly round the yellow spot towards the periphery of the retina. The course of the veins is similar, both sets of vessels avoiding the yellow spot; the veins are larger in calibre, darker in colour, and a little tortuous; in addition to these larger branches several minute ones may be seen ramifying on the disc. The vessels do not usually subdivide until after leaving the disc; they do not always pass through the nerve in one and the same opening, and one vessel is often seen to cross the other—sometimes the artery beneath, sometimes the vein. In the drawing the artery is seen to cross the vein above, and to pass beneath the vein on the lower part of the disc. Occasionally the disc presents a slightly mottled appearance, attributable to a number of small oval, dark, or bluish spots; this appearance is due to the anatomical arrangement; the intraocular portion of the nerve being almost transparent, we can see completely through it as far as the *lamina cribrosa*, which reflects the light very strongly; occasionally some of the nerve tubules continue transparent for some distance on the cranial side of the perforated plate, and we are thus enabled to look down into them beyond the cribriform plate; hence they appear as dark spots, while the lamina cribrosa itself appears as a glistening white surface. It is not uncommon to find in the centre of the disc a depression; this may be of greater or less magnitude and simulate the cupped optic nerve seen in glaucoma. This *physiological excavation*, as it is termed, is due to the circumstance of the nerve fibres bending round suddenly at right angles into the retina, leaving a space at the centre between them; it is seen as a bright white spot, generally at the outside (real) of the papilla, or it may appear as an extensive cup-like depression of the nerve. In this normal excavation, however, the margin of the depression never corresponds with the margin of the nerve disc, which it usually does in glaucomatic excavation. Another remarkable physiological *phenomenon consists in* the visible pulsation of the veins of the disc, which

is not a very uncommon appearance during health; it is confined to the veins, and does not occur outside the optic disc. This *venous pulsation* is attributed to the temporarily increased intraocular pressure caused by the influx of blood brought to the eye by the arteries at each systole of the heart; an increase in volume occurs in the coats of the globe and compresses the vitreous humour, which on its part reacts and presses on the veins, they being the most yielding parts; a complete or partial momentary stoppage of the column of blood in the vein is thus produced, and as soon as the transient pressure is removed the vein expands and the arrested column passes rapidly on. Immediately after the pulsation at the wrist the vein may be seen to fill from its peripheral end, and then, after a moment to empty. This venous and normal pulse must not be confounded with an *arterial pulsation*, which only occurs during disease, when the pressure of the vitreous is continuous and greater than the lateral pressure exerted in the artery itself. This pulse cannot be easily mistaken, as it occurs synchronously with the systole of the heart; it appears as a rhythmical movement of the red column of blood, and as the vessel appears empty during the diastole of the heart. Both these pulses may be produced by pressing on the outside of the globe with one of the disengaged fingers of the hand carrying the lens. Slight pressure will, as a rule, suffice to produce the venous pulse; if the pressure be kept up steadily and firmly the venous pulse ceases, the optic papilla pales, the veins empty, and the arteries pulsate, and dimness of vision ensues. When on slight pressure being made the arterial pulse becomes apparent, it shows that the intraocular pressure is already abnormally great. The physiological explanation of the arterial pulse was first given by von Gräfe, who likewise drew attention to its great practical importance and significance in the diagnosis of glaucoma.

In shape the optic papilla is, as a rule, circular. Sometimes, however, it is oval from above downwards, and very rarely horizontally oval. Whenever it is seen to vary from the circular shape, we should be on our guard and satisfy ourselves that it is actually anatomically oval, for by a false position of the lens its shape may be distorted. In case the papilla appears oval in the reversed image, we should examine it by the direct method, and *vice versâ*, as a comparison of the results of both methods may lead to the recognition of a rare anomaly of refraction—astigmatism. Minute spots of pigment have, on rare occasions, been observed as congenital peculiarities on the optic papilla of normal eyes.

The *retina* cannot, as a rule, be distinctly seen during health; it is, if not perfectly transparent, at all events highly translucent, and we look through it on to the choroid. When the choroid is deeply coloured with abundant dark pigment, the retina may be indistinctly visible as a slightly greyish cobweb-like membrane; occasionally delicate, isolated lines, representing the nerve fibres, may be detected radiating towards the periphery. The presence of the retina may, however, always be recognized by means of the branches of the arteria and venacentralis retinæ, which, after passing the disc, course towards the periphery in the nerve-fibre layer of the retina. These vessels are, as I have stated, distinguished by their size and colour. The arteries are small, and filled with bright red blood, while the veins are larger, somewhat more tortuous, and of a darker colour. The arteries appear sometimes as if transparent along the centre of the vessel, or, which is the same, as if the lateral walls were opaque; this is, however, solely owing to the reflection of light, in the direction of its incidence, from the most prominent part of the vessel (that towards the observer). The retinal vessels appear almost to stand out in relief, and are easily visible. When one of these comes into view, while the observer is looking for the optic disc, he should trace it backwards in the direction of its increasing thickness, or in the contrary direction to that in which its vessels are given off.

The *macula lutea*, or yellow spot of Sœmmering, is situated to the outside of the optic disc in the retina. Its position in the inverted image is indicated by the dark shading to the inside or left-hand side of the disc. This so-called yellow spot is only exceptionally to be distinctly recognised; its

position may, however, be at all times easily seen. The patient should regard the image of the flame in the mirror, or the point of the erect index finger of the hand carrying the ophthalmoscope, the finger being close beside the mirror. The position of the yellow spot is characterised ophthalmoscopically by the absence of retinal vessels of any size, and by the dark colouration which is due to a more abundant pigmentation of the choroid at this than at other places. The fovea centralis is said to appear as a small, bright spot about the middle of this dark-coloured portion. Those wishing to study this part more particularly should employ a plane mirror, with a concave lens behind it, and get the observed to regard steadily the flame of the image in the mirror; there will then be formed an image of the flame, or part of it, on the fovea centralis.

The *choroid* membrane lies beneath the retina, and forms one of the most important features in the ophthalmoscopic image, as to it is due the peculiar red colour of the fundus; in the examination we are, in fact, regarding not the retina but the choroid. The colour of the fundus depends on the illumination and on the pigmentation of the choroid and its epithelial layer; it is a bright blood-red, with an admixture of yellowish brown due to the blood in the tunica vasculosa and the chorio capillaris; and to the pigment in the stroma and in the epithelial layer; in fair and blonde persons the amount of pigment in the cells is small, and the fundus appears of a light yellow red, whereas in dark-complexioned individuals, with brown or black irides, the pigment is abundant, and the fundus appears of a brown red. Dark, brown, or black spots, with irregular outlines, are of not uncommon occurrence in the fundus of healthy eyes; they are simply accumulations of pigment corpuscles, and are most frequently seen at the outside of the optic nerve entrance. As a rule, the fundus presents a uniform colour, and a faintly granular or stippled appearance, in front of which the retinal vessels stand out in relief; or the retina itself may be recognisable as a delicate bluish grey film in front of the dark choroid. Occasionally the internal (epithelial) layer of pigment is so scanty and diaphanous that we are able to see the large choroidal vessels through it; these are always of a lighter colour, and are likewise broader than the retinal vessels; they are likened to flattened ribands, of a yellowish or orange tinge; they are not sharply defined, take a winding course, anastomose, and divide frequently, and may be distinctly seen lying beneath the retina; by fixing a large dark retinal vein, we can satisfy ourselves of the existence of a space between it and the choroidal vessel, and by means of the binocular ophthalmoscope this relation becomes very manifest. Occasionally the pigment is so abundant and dark in the meshes of the stroma between the vessels, and so scanty in the epithelial layer, that the choroid appears as if mapped out into irregular, angular, dark, island-like spaces, bounded by yellowish lines, the latter representing the large choroidal vessels. In healthy eyes the fundus should be perfectly clear and brilliant, without any haziness or indistinctness of its parts. At the entrance of the optic nerve into the globe the choroid appears sometimes as a dark circle or half circle around the nerve.

The *sclerotic*, although entering into the formation of the fundus, is but rarely visible during health, being concealed by the choroid; it contributes, probably, in a slight degree, to the colouration of the ophthalmoscopic image. Occasionally a small circle of this membrane is exposed around the optic disc at the nerve entrance, and appears of a brilliant white, or tendinous aspect. As a result of pathological alterations, this membrane frequently becomes visible.—*Dublin Quarterly Journal, August*, 1867, *p.* 25.

87.—THE OPHTHALMOSCOPE AND THE PHYSICIAN.

By J. Vose Solomon, Esq., Surgeon to the Birmingham and Midland Eye Hospital.

Since the fact has become generally known to physicians that certain cerebral diseases afford pathological signs in the optic nerve, which are demon-

strable by the ophthalmoscope and by no other means, an increased desire has arisen on the part of the medical profession to be able to focus the fundus oculi with sufficient facility and clearness to obtain a perfect view of its condition.

Hitherto, the elementary difficulties have been such as to deter many physicians and some oculists from giving that amount of persevering labour to the method as would be crowned by success. It is evidently, therefore, the duty of the profession who have acquired familiarity in the use of the ophthalmoscope to aid, by bridging over difficulties, in its more general application to the discovery of morbid phenomena.

Many students get so far with the instrument as to be able to reflect and maintain the light of the lamp upon the eye of the patient, to apply their own organ of vision to the aperture of the mirror, and to bring the convex lens to a proper distance in front of the observed eye, yet without obtaining a definite outline of the optic nerve entrance and its vessels. At this point, so often do they stick, that at last, after repeated trials, the instrument is put aside, an instance of the *non possumus*.

The bar to further progress in such cases has its origin in an inability on the part of the physician to relax the accommodation of his own eye to the precise degree that will permit of a distinct aerial image of the parts named being seen by him.

I find all impediment to focussing of the fundus to be at once overcome if the student suspends the power of accommodation of his right eye by the application of a four-grain solution of atropia (British Pharmacopœia) half an hour before making his ophthalmoscopic examination.

This being done, and the iris of the patient fully dilated, he has only, after having observed the usual rules laid down in ophthalmic works, to gently move his head a little backward or forward, as the case may be, and a complete picture of the optic nerve and its surroundings starts into view.

The physician will do well to select a case of extreme short sight, atrophy of the optic nerve, or a light blue normal eye, for his first experiment, and continue his observations every day. By the time the action of the atropia has subsided, he will, if endowed with good vision and some manual tact, find himself master of a step in ophthalmic investigation which had hitherto been considered by him as insurmountable.—*British Medical Journal, June* 1, 1867, *p.* 628.

88.—ON AN IMPROVED SUCTION-TRACTOR.

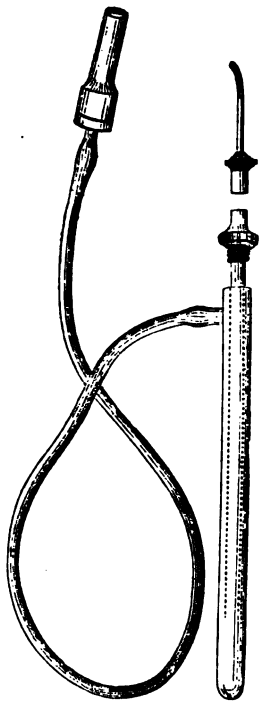
By Henry Greenway, Esq., Plymouth.

The employment of suction in ophthalmic surgery has of late occupied the occasional attention of the profession. Some years since I published an account of an instrument I invented for the purpose of forming artificial pupil by the aid of suction, mentioning at the same time that the principle might be applied to other operations by merely altering the size and shape of certain parts of the instrument. The suction apparatus was composed of a plano-convex canula, having an aperture on the plane surface near its point, as an inlet for the iris. The near end of this canula was furnished with a screw and milled rim, whereby it was attached to a tubular handle. From this handle proceeded, at right angles, a short branch tube, on which was fastened an india-rubber tube, about fifteen inches in length, at the free end of which was a mouth-piece. Within the canula lay a blade for excising the portion of iris drawn in. This blade was acted on by pressing the end of a small rod which passed up through the handle.

In 1864, Mr. Pridgin Teale, jun., of Leeds, proved that this very same combination of tubes, unfurnished with a blade, could be employed simply as a suction instrument for the removal of soft cataract, thus successfully reviving an operation which had become obsolete from the difficulty or danger attending the use of former instruments. Other suction instruments for this pur-

pose have since been devised, Mr. Bowman's ingenious contrivance being one of them.

The following additions which I have made to my original instrument have been considered improvements:—Within the mouth-piece lies a valve, made of extremely thin india-rubber (oiled silk will not answer), which is opened with the slightest amount of suction, and is self-acting in closing. This prevents an accidental back current of air—a very important point when the eye is the part being operated on; for such accident has happened where there has been no valve. It also serves the purpose of a maintaining power for holding whatever has been sucked into, or laid hold by, the canula. The open end of the tubular handle receives a nozzle. In the inner opening of this nozzle is fixed a narrow tube, which extends nearly to the upper, closed, extremity of the handle. The chief use of this tube is to make the current more circuitous, and, consequently, the suction action more gentle. The outer opening of the nozzle receives the canula. Various sizes and shapes of canulæ can be fitted according to the requirements of the case. The one shown in the engraving is small, and suitable for extracting fluid—such as soft cataract, &c. I have another for laying hold of a hard cataract, in a case of linear extraction, where there is a difficulty in dislodging the lens from the capsule. Also, another having a very small india-rubber cup appended, whereby a foreign body lodged in the ear, nostril, or other cavity can be withdrawn.



This last idea has led me to propose, to a naval surgeon, who has seen much service, the employment of an instrument for the extraction of balls from gunshot wounds by means of suction. My suggestion was received favourably, and I trust I may soon be enabled to lay before the profession the result of my experiments.

I have been asked, How is a surgeon to judge as to the amount of suction-power required for the removal of soft cataract? I believe no writer on the subject of extraction by suction has thought it worth while dwelling on that point; as it is generally supposed the operator would naturally be cautious in the employment of a force within the eyeball. If, however, any answer be required, I would suggest the following, which is somewhat in accordance with the rule for speaking in public: "Begin low; proceed slow." Commence sucking with the slightest effort possible; and if this should not suffice, then increase the power almost imperceptibly until the desired result is obtained. Should the canula become choked, which would be known by suction becoming difficult, it would be unsafe to increase the suction-power to a great extent; for the plug might suddenly be dislodged, thereby causing a rush of the iris or some other structure towards the aperture of the canula. In the event, therefore, of any plugging, the canula should be withdrawn from the eye, and the obstruction forcibly removed, as recommended by Teale and others; the operation to be then resumed.

In holding the suction instrument, the india-rubber tube, where it joins the handle, should be directed downwards; for by the observance of this caution any twisting motion of the instrument in the fingers is obviated, and the operator's hand is more at ease.

I think the term "suction-tractor" a suitable name for the instrument, whether it be used for drawing a fluid *through* it or a solid *on* it.

In the engraving (half-size of instrument) the nozzle is unscrewed and *slightly withdrawn* from the handle, thus exposing a portion of the inner

tube, the remainder of which is indicated by the dotted lines. The canula is shown, disconnected from the nozzle. The tubular handle and the tube within it may be made either of glass or metal. The latter is to be preferred if the surgeon reside in a locality where a breakage could not be repaired.

The Messrs. Weiss supply the instrument, their name being a sufficient guarantee for good workmanship.—*Lancet, June* 8, 1867, *p.* 693.

89.—ON THE OPERATIONS FOR ARTIFICIAL PUPIL.

By Haynes Walton, Esq., Surgeon to the Central London Ophthalmic Hospital.

Should an artificial pupil be made in the one eye while the other is yet sound? My own opinion is against operating, as a rule, so long as the one eye is efficient, except the pupil can be made in the centre of the iris of the disabled or blind eye, the lens being present or absent; or upwards or downwards, the lens being present: for if the pupils do not have this correspondence, there will most probably be confusion of sight, double vision, or squint. A dissimilarity in the positions of the pupils is generally, but not always, followed by this disturbance of vision: why only occasionally I am unable to say; I only know the fact that, under apparently the same circumstances, when the pupils disagree, sometimes there will be the derangement, and sometimes not any. I recommend the operation, even although sight should not be altogether restored, but only a moderate amount of it acquired; because there is much gain, especially in the lateral use of the eye.

This is a subject that I have carefully examined in a practical manner, for theory can afford no help. Moreover, I am giving my own conclusions, unbiassed by those of others; for I am totally unaware of any published opinions, or of conclusions come to, by any observer in the same field. But I know that there is a general impression in the profession, and out of it, against interfering; and this seems to have come down traditionally from an age when ophthalmic subjects were but little understood.

When a person has lost an eye, he is not only blind on one side, but is reduced to a very limited range of vision for near objects beyond the centre of the face, and the angle of which is regulated by the prominence of the nose. The definition of sight, on which binocular vision depends, is lost. The power of accurately estimating distance is lost. Still worse than this, with labour requiring minute sight, there are more readily developed the many defects of impaired vision than when two eyes are used, because one eye cannot with impunity do the work of two. Therefore that measure is admissible which, while it restores the sight, does no harm elsewhere.

It can happen but as a rare opportunity that an artificial pupil can be made in the centre of the iris while the crystalline lens is present and transparent. Where this can be effected, the highest attainable perfection is necessarily gained. All the beautiful functions of the eyes are restored. When the pupil is lateral, even when the lens is present, the quality of sight is necessarily lessened. Nearly always, however, the lens is absent when a pupil is made, and minute sight must depend on the use of glasses of a high power, commonly called cataract glasses. The adjusting power of the eye is gone, and for seeing at different distances glasses of different foci are required. But, practically, an eye cannot be fitted with glasses and brought up to a state to match the other. Therefore arises this important question, on which the whole matter hinges: Will this kind of sight, which is inferior to that of the other eye, and at times in marked degrees, be really of material service? I say yes; I am satisfied about it; I act on the belief of it. I would be deterred from operating only by the probability of the eye being too much damaged to afford that amount of sight known as "useful." There can be no mistake about the results I have obtained. They are these:—Side blindness has been removed, and direct vision assisted. In those cases in which the lens was present there has been restoration of the ocular adjustment, even when the pupil was lateral.

I have been careful to test the statements of patients by my own observation. In casting my eye over the notes of the cases in which I have made a central pupil, the lens being absent, I find that pains and care were taken in jotting down the effects. In all the patients expressed their satisfaction and pleasure for what I had done. According to my experience, then, confusion of vision does not follow the restoration of the pupil when there is perfect vision in the other eye. This agrees with the fact that in "coloboma iridis" on one side, with the usual imperfection to vision that it causes, the other eye being sound, there is no confusion of sight; and that, when the defective eye is closed, much useful vision is cut off.

When two artificial pupils are made, the crystalline lenses being absent, both must be central, or that confusion of sight will ensue which will oblige one of the eyes to be closed. Several times persons with dissimilar pupils, suffering the annoyance, have called on me, told their trouble, and asked to have one of them closed.

When, after the loss of the crystalline lens, inflammation has destroyed the pupil, the iris does not necessarily alter its position. It does not bulge from the loss of communication between the chambers of the eye, provided the tonicity is preserved; and then the size of the anterior chamber is not only undiminished—a matter of importance in operating—but may be actually increased by the falling back of the iris, in consequence of the absence of the lens. There may be but a mere trace of the remains of the pupil; or the aperture, very much contracted, may be closed by lymph or capsule.

When prolapse of the iris through the cornea has shut up the pupil, there must always be a reduction in the size of the anterior chamber, the diminution depending upon the position at which the iris has escaped. The two states—closure of the pupil from inflammation, and prolapse of the iris—may coexist.

The operation of "incision," with the iris knife, is the most appropriate in any case where the iris retains enough of its physical properties to gape on being divided; and the less it has suffered from inflammation, the more certain will be the result. I invariably operate through the cornea. The following methods of steadying the eye are applicable to all these operations:—An assistant draws down the lower eyelid, resting his finger on the malar bone. I raise the upper eyelid with my forefinger, and with the tip of it and of that of the middle finger steady the eyeball. If in any case the steadying of the eyeball by the assistant by holding the conjunctiva with a pair of forceps be serviceable, it should be adopted. Sometimes I retract the eyelids with a spring-wire retractor, while the assistant steadies the eyeball with the forceps. This is more applicable when it is important to retain all that can be kept of the aqueous humour.

With a large or small knife, according to the circumstances of the case, I divide the cornea as near to the circumference as practicable, and penetrate the iris as centrally as possible, thrusting the blade up to the shoulder. Except the knife be sharp and well used, the iris may be dragged on and torn from its ciliary attachment. The aperture thus made is about the third of the diameter of the iris, ellipitical and vertical.

A great deal of stress is usually laid on the special fitness of "incision" in cases where the iris is on the stretch from prolapsus. This property is much overrated. The choice of the operation, so far as the state of the iris can be taken as a guide, should be made to depend on its actual structural condition; because any advantage that the mere stretching could afford might be lost by slight interstitial change, and inflammation of the eyeball is often a cause of the prolapse.

The pupil may be closed after the absorption of the lens in capsulo-lenticular cataract from the effects of inflammation prior to an operation or after it. "Incision" is suited when the iris will retract.

It was by "incision" performed through the sclerotica that Cheselden, our countryman, and surgeon to St. Thomas's Hospital, the originator of artificial pupil, used to operate. His method fell into disrepute and was almost *abandoned,* when Sir W. Adams revived it.

The great advantages of the operation I advocate are—its being executed through the cornea; the certainty of being able to make the pupil at the desired spot; the division of the iris before the aqueous humour is lost, and therefore while it is tense, by an incision which, owing to the form of the knife, is effected with such slight pressure that there is no risk of detachment from its natural connexions.

To render "incision" with the knife more generally applicable, by adapting it to cases in which the iris has not sufficient tone to contract when merely incised, and when it is more or less tied or fixed by being prolapsed, or adherent to capsule, I proposed some years ago a modification of the above operation—namely to divide the iris, and with a hook to draw towards the outer lip of the wound till a sufficient gap is made. An aperture is thus effected by tearing and by the folding inwards of the flap. This may be denominated "incision with extension."

It not unfrequently happens that a dense layer of lymph or thickened capsule blocks up the pupil, and forms an impediment to "incision" with the knife.—at least to the formation of a central aperture by it. Perhaps, too, without any capsule the iris is not healthy enough to retract when cut. With, however, the combination of another mode of operating "excision"—the details of which will be subsequently given—a pupil can be made towards the centre, and of better shape than by any other means. I have performed this compound operation many times. I divide the iris in the centre, as above described, with an iris knife; then seize the outer portion with an iris hook, draw it without the cornea, and cut off a piece. The chief nicety required in the operation is not to excise too large a bit.

[The mode of procedure described below, is that recommended in a case of partial closure of the pupil by lymph, the lens and its capsule transparent.]

Tearing with excision; iriddesis.

Tearing out a strip of the iris, and then cutting it away, is the compound operation chiefly applicable here. The cornea should be opened at the margin, with a small iris knife or broad needle, a Tyrrell's hook introduced, the point carefully inserted into the opening of the pupil, which would necessarily be very small, and an endeavour made to tear out a piece of the iris. Sometimes a thin strip is brought away, which may or may not be snipped off, according to the length of the piece. Sometimes only a fissure is made, and therefore an inadequate aperture; the result depending very much on the condition of the iris, whether it be considerably altered in structure or not: if much damaged the hook readily tears out. It may therefore be necessary at a future period to enlarge the fissure. The hook is more in contact with the capsule of the lens in this than in any other operation for artificial pupil; yet, if properly made and carefully used it will not inflict any injury.

It matters not, as far as the performance of the operation is concerned, at which side of the eye the free portion of the pupil be situated, for the stem of the hook can be so bent as to allow the instrument to be used in any direction; but it is better, when the iris is unadherent in a direction not the best suited for a pupil, to disregard any advantage, so far as mere manipulation is concerned, and with the canula forceps make an aperture in the best situation.

The operation "iriddesis" is also applicable here, if not by one tying, by two, at a few lines apart, whereby a triangular aperture is made. It must be mentioned that the iris may be too rotten to be tied.

DIMINUTION OR CLOSURE OF THE PUPIL FROM PROLAPSE OF THE IRIS THROUGH THE CORNEA; OR ADHESION OF IT TO THE CORNEA IN CONSEQUENCE OF A WOUND, A PENETRATING ULCER, A SLOUGH, OR SUPPURATION OF THE CORNEA; THE CORNEA ITSELF MORE OR LESS OPAQUE IN THE CENTRE, THE LENS AND THE CAPSULE TRANSPARENT OR OPAQUE, OR THE LENS LOST.

Cutting or tearing through the adhesions; excision; incision; iriddesis; separation.

Under this head occur the greater number of cases requiring an artificial pupil; and of these the majority arise from ulceration of the cornea and

prolapse of a part of the iris. But as, in a surgical point of view, it is the same whether the pupil be lost by prolapse or by mere adhesion of the iris to the cornea, I shall not practically recognise any difference.

When a part of the pupillary margin is prolapsed, the pupillary aperture being merely diminished, the corneal opacity limited, or, if extensive, not so dense as to obstruct light, the iris should be detached from the cornea. The cornea should be punctured, with the smallest iris knife, at the spot where the adhesion can be most readily reached, the blade directed between the iris and the cornea, and applied at once to the part to be severed. If the knife be carried too far and then withdrawn, the aqueous humor will escape, and the operation probably fail. I have torn away such connexion with a blunt hook. Should a large portion of the pupil be adherent, it is impossible to operate by these methods. In such cases I cut through the connexions with a blunt iris-scissors, or sometimes only some of them. Less frequently, I have used the scissors to the utmost, and torn through the remaining adhesions with a blunt hook.

"Excision" is often well adapted. I have made excellent pupils by it.

When the pupil is quite closed, the choice of the operation must depend chiefly on the extent of the corneal opacity, and the state of the lens.

When the lens is present, and supposed not to be opaque, the question is whether a portion of the iris should be torn away with the capsule forceps, as already described, or Tyrrell's operation adopted—that of incising the iris close to the cornea with the smallest iris knife, and with the blunt hook withdrawing a portion of it, and then cutting it off.

When the lens is absent, in consequence of its having escaped through a wound, or having been absorbed, "incision" is applicable, in combination, perhaps, with "excision"

When cataract is present, the operator must decide, according to the rules already laid down for parallel instances, whether he shall extract it at the time of making the false pupil or subsequently, or whether he shall perform the operation for the pupil, and afterwards treat the cataract by "solution."

When the pupil is closed from extensive adhesion of the iris, and only just the circumference of the cornea is transparent, "separation," or the tearing away of the iris from its natural attachment, is the only resource; for then it would be most imprudent to incise that part of the cornea for the execution of any of the other operations, lest opacity of it should ensue.

"Separation" therefore should be restricted to those cases in which none of the above-described operations can be executed, as it inflicts much injury on the eye, and according to my own experience, is less successful than any of the methods of making a false pupil that I have described. Besides this, it may be stated in general, that eyes requiring it are most unfavourable for an operation, as they are usually the wrecks of disease, and fall only just within the compass of operative surgery. The cornea, besides having dense opacities from slough or penetrating ulceration, is frequently staphylomatous; and adhesions of the iris to it diminish, or even destroy, the anterior chamber, whereby the operation is rendered very difficult.

To perform the operation of "separation" the cornea should be divided obliquely with the second-sized iris knife, opposite, or as nearly as possible opposite, the portion of the iris to be detached, and so far only from its margin that, when enough of the iris is torn away, it shall readily reach the incision, so as to allow a part of it being withdrawn, and cut off or strangulated. If the incision be very far, too much will be separated. The next step is the important one of extracting the piece of iris. The canula forceps are very applicable, and I have used them with very perfect satisfaction. They are far preferable to the hook, because they more certainly retain the tissue of a diseased iris, and are less likely to injure the capsule of the lens, and are easier withdrawn. The hook that I have employed is the ordinary "lens hook" sometimes used in the operation for cataract.

Whichever be the instrument selected, it should be carried in front of the *iris to the margin*, where it is implanted and separation effected, and as the

circumference of the iris is a little behind the sclerotica, the extremity must pass out of sight to seize it. Then the instrument must be kept close to the cornea while being withdrawn, lest the capsule of the lens be injured. When the hook is used the withdrawal through the cornea requires adroitness, and unless the aperture be ample, the iris will probably be shaken off; the size should, therefore, bear some relation to that of the hook, being, of course, always much larger.

There is greater uncertainty in the execution of separation than in any of the foregoing operations. Instead of a portion of iris of the required size separating, a mere strip of it may give way, or what is more common, the instrument may tear out without effecting any separation.

I must consider in this place that displacement of the pupil, and generally, too, that diminution of it, which may occur after the operation for "extraction," and which is nearly always produced by prolapse of the iris. When the aperture is very small, and the iris therefore tense, I have enlarged it by simple "incision" with the knife. When, although perhaps large enough, it has been too much displaced to be of full benefit—that is, by being concealed under the upper eyelid, "incision" with the knife is not generally applicable, but it may be done with the scissors. I divided the cornea to the required extent with the cataract knife, and with the scissors I made a snip in the centre of the free edge. I have had occasion to adopt "excision."

There are a few topics that may for convenience be introduced here as concluding general remarks.

The classification I have made has been solely for the sake of perspicuity and brevity, and I hope it has been made sufficiently clear, that the choice of any particular operation must be based on a careful examination and analysis of all the particulars of the case. The peculiarity in each instance must be the guide to the method most suitable.

An operator should not follow implicitly any rules that have been given, if he should deem it advantageous to depart from them. I profess to give only general principles. It is practically impossible to supply minute details for the guidance of a beginner in any class of operations.

The success of a case must often depend on the manner in which the corneal incision is made, whether sufficiently ample or not, and the selection of the best position for it. If it be too narrow it should at once be enlarged with the secondary knife, and if it be more convenient to make the incision at the line of junction of the cornea and the sclerotic, or even through the sclerotic half an inch behind the cornea, such spots should be chosen

When the size of a pupil is a matter of consideration, it should be remembered that "excision" invariably gives the largest aperture. The smallest is mostly got when a piece is torn out of an adhering iris.

The bleeding which may ensue is sometimes a hindrance to the subsequent steps of the operation. If it be limited in quantity, slight pressure on the cornea may squeeze enough out to clear the objects. If the hemorrhage continue, the operation should be suspended, and pressure applied on the eye by cotton wool and a bandage. If blood be poured out into the chambers of the eye after an operation, supposing it to be confined there, and to have ceased to flow, there need not be any apprehension. I am not aware that it is attended with any disadvantage; it is soon absorbed, and does not require any special treatment.

If an artificial pupil should close, and the eye be not too much damaged by the inflammatory action that has closed it, a second attempt may be successful. I have known a third required before a result was obtained. It is possible that atropine may be beneficial after operating, by keeping the edges of the pupil more apart. It should always be used, except when a part of the iris has been left strangulated.

The after-treatment is the same as when the operation for "extraction" has been performed, the most important points being the quiet of the patient, perfect rest to the eye, cold lotions, and, if there be pain, opiates.—*Lancet, June* 1 *and Aug.* 31, 1867, *pp.* 659, 257.

90.—ON A METHOD OF OPERATING FOR DIVERGENT SQUINT.

By Dr. C. R. Agnew, Lecturer on Ophthalmic Diseases at the College of Physicians and Surgeons, New York.

The method is as follows:—The patient having been placed upon his back, and under the full influence of an anæsthetic, and the eye to be operated upon exposed by the wire speculum, an assistant draws the cornea as much as possible towards the outer canthus by catching the tissues over the tendon of the external rectus muscle in the blades of a pair of fixation forceps. The operator then makes a horizontal opening over the internal rectus muscle, midway between its borders, and extending from a point one line distant from the cornea down to the semi-lunar fold. This opening should be made by lifting a vertical fold of the conjunctiva and sub-conjunctival tissues with forceps, and cutting it with scissors in a horizontal direction. If care is exercised, the internal rectus muscle will be exposed without any difficulty, or the occurrence of much bleeding. The next step is to secure the entire tendon of the muscle which is to be brought forward. This is especially essential in those cases in which the divergent squint has been the result of the operation for convergent squint, for in such cases the tendon and theca, having been much haggled in the original tenotomy, fall back irregularly, and, being split more or less, form false insertions, which are zigzag or interrupted.

After the apparent insertion of the muscle has been brought into view, a strabismus hook, having an eye drilled in its free extremity, and armed with a waxed silk, is made to sweep beneath it, from below upwards, care being taken to keep the instrument in close contact with the sclerotic, and carried so far back as to include every straggling band of muscle or theca which is to be advanced.

The uplifted mass should then be tied close to its sclerotic implantation. The next step is to divide the external rectus muscle freely through a vertical wound in the conjunctiva, and thus complete the preliminary steps for the advancement of the internal rectus. The latter step is effected as follows:—The operator, holding the ligature firmly in one hand, should cut with scissors the insertion of the muscle, and gently break up any bands of connective tissue which may attach it to the sclerotic. As he does this, he should draw upon the ligature and sway it from side to side until it becomes evident that any adhesions which might obstruct the advancement of the muscle, have been overcome. He should now estimate the amount of adduction which may be necessary to cure the divergence. This he can do by catching with forceps the sclerotic edge of the cut tendon of the external rectus, and drawing the cornea towards the inner canthus, while he holds upon the stretch the muscle to be advanced. The retentive sutures are now to be placed. For this purpose two delicate, short, and sharply curved needles are to be armed with fine, well waxed silk, and adapted to a needle-holder.

Having measured the extent to which the eyeball must be adducted in order to correct the divergence, the sutures should be passed as deep in the muscle and theca as may be necessary. The muscle and theca should be drawn well out and kept upon the stretch, so that the sutures may be passed as deeply as possible behind the caruncle, to secure a firm hold, and to leave a somewhat longer mass between the perforations made by the sutures and the ligature upon the cut end of the muscle than the original divergence measured. The course of the sutures should be perpendicular to the plane of the muscle, one passing through near its upper margin, and the other near its lower. After the sutures have been placed in the muscle the end included in the ligature should be cut off, care being taken to leave enough to prevent their tearing out. The amount cut should nearly equal the degree of divergence to be corrected, allowance being made for shrinking which has followed the detachment of the muscle from the sclerotic. The next step is to carry the sutures beneath the conjunctiva above and below the cornea. It is better to *place the upper suture* first. This also requires the curved needle. The

point aimed at in carrying the needle along the sclerotic, beneath the conjunctiva, should be about a line above the cornea, and over the centre of the line of implantation of the superior rectus muscle, and there the suture should emerge. Before tying the upper, the lower suture should be brought out at a corresponding point over the inferior rectus insertion. While the operator is cautiously tying the sutures, his assistant should, catching hold of the insertion of the external rectus, carry the cornea towards the internal canthus as much as possible, and thus effect what may be considered the real intention of the operation, namely, to adduct the eye strongly, and place the end of the *shortened* internal rectus in coaptation with the sclerotic at or near the natural line of sclerotic implantation. The exercise of a little care will cause the muscle to spread out and be hidden behind the horizontal pillars of the wound, through which the retentive sutures have been carried, and thus insuring a consolidation of the wounded parts, obtain the aid of the subsequent cicatricial contraction of the soft parts intervening between the cornea and the caruncle in the ultimate result. Whether this method be adopted throughout or not, I am quite sure that the use of the strabismus hook, armed with a ligature, will be found of great advantage in getting control over the tendon to be advanced, and making the surgeon feel sure that he has secured the entire mass. I have employed the method upon two eyes which had been rendered divergent by operations for convergent squint performed by a wandering quack. In one of these eyes the divergence was more than five lines; in the other about four. In the first eye I induced a small amount of convergent squint, which was cured by applying the glass, which neutralized existing hypermetropia. In the second case I produced a very slight degree of convergence, not exceeding a line, which was also removed by the use of the proper glasses. And in both these cases the existence of hypermetropic asthenopia, would have rendered glasses necessary, aside from any convergence.

I have also satisfactorily performed the operation in two cases in which slight paresis of the internal rectus, the result of injury, had led to divergent squint.

In one case in which the divergence was so great that one-fourth of the cornea was buried beneath the external canthus, the result was only partially successful; but I propose to repeat the operation after the cicatrix over the region of the internal rectus has so matured and softened as to render a repetition of the steps possible.

I have not seen any inflammation of an annoying character following the procedure, and my experience thus far justifies me in employing the method in preference to any other, as I believe that the risk to the eye is very much less than by any other, and the probability of success greater. I believe that the chance of success is greatly increased by dividing the external rectus of the fellow-eye, even though you propose to advance the internal rectus of one eye only.—*New York Medical Journal, July*, 1867, *p.* 317.

91.—ON THE REMOVAL OF OPACITY OF THE CORNEA.

Some very curious facts relative to the action of sulphate of soda on the cornea, are pointed out in a memoir just read before the French Academy by M. de Luca. It occurred to him that as sulphate of soda retains the fibrine of blood in solution, it might have the effect of dissolving away opaque portions of the cornea. At first he tried the experiment with solution of the sulphate in distilled water. The fluid he dropped into his patient's eyes, and the result was found to a certain extent satisfactory. Believing that the solid sulphate would produce still better results, he employed it in the state of very fine powder; dropping it into the eye of the patient. This method proved to be most effectual. In one or two cases where the opacity was so decided as to produce nearly total blindness, a considerable amount of vision was restored.—*Medical Press and Circular*, Sept. 11, 1867, p. 251.

92.—ON THE PATHOLOGY OF TINNITUS AURIUM.

By John Rhmor, Esq., F.R.S.

Every practical surgeon is well acquainted with the numerous accidents to which the bones of the head and the membranes of the brain are liable, and is aware that some of these may and really do affect the mechanism in the labyrinth, more especially since we know that the membranous tissues, as well as those of the osseous framework of the parts, are all liable to become inflamed. Let us now ask ourselves what are the cause and seat of those sounds in the ear which arise spontaneously, and which not only trouble the invalid, but which cause so much embarrassment for the aurist to relieve. It cannot be true, as some have supposed, that this malady arises either from the spontaneous movements of the organs external to the labyrinth, or otherwise from the immobility of the membrana tympani, as Sir W. Wilde has imagined, since we know that by plugging the external meatus, so as to prevent the vibratory movements of the external air from reaching the tympanum, we merely hear the sounds produced by the rush of blood in its transit by the ear to the brain, and we can very clearly distinguish the sound of a tuning-fork in the stopped ear when applied in a state of vibration to the opposite side of the head, whilst the ear whose auricle has been left perfectly free receives no sound, thus proving that the vibratory movements are transmitted to the stopped ear by the bones of the head to the semicircular canals, as Weber has suggested. However, the history of the cases under consideration proves that they take their origin in a deeper seat than the external organs of hearing. The cases of tinnitus aurium which I have more recently examined have been caused by exposure of the head in warm or tropical climates, or they have occurred in persons who have suffered from violent blows on the head on being thrown from open carriages, &c. If the external organs of hearing were the seat of the malady, it might justify some of those experiments which have so often been tried and have so signally failed. If we include in the term all the sounds affecting the organ of hearing resulting in defective states in the external mechanism, as well as those within the labyrinth, the seat of this complaint must necessarily remain vague and undefined: but if we restrict it to those sounds which are traceable to some existing defects in the nervous structure of the labyrinth, we shall then be enabled not only to form a more correct diagnosis of the malady, but shall be able to form our determination of the kind of treatment best calculated to procure relief, if not entire recovery.

The most practical and common-sense views for the treatment of these affections have been discussed in the work by Sir W. Wilde, who is very justly severe against the nostrums recommended by empirics. He puts no faith in the vaunted virtues of galvanism, which might have been predicted to be an agent unsuited to the pathological states of these nervous structures: and with respect to the injection of liquids, vapour of ether, or any other agents into the tympanum through the Eustachian tube, as recommended by Kramer and practised by others, he declares that he has never seen the slightest benefit from this practice, either in cases under his own care, or under the treatment of others. According to Cheselden, the injection of water into the Eustachian tube was followed immediately after by deafness: and although Saunders has described a case in which the hearing was improved as long as water was retained in the ear, this effect might have been produced by its supplying a medium for the transmission of sound: at any rate, deafness returned as soon as the fluid was withdrawn. Indeed, it is difficult to discover, on any acoustic or therapeutic principles, that good can arise from injections into the Eustachian tube: and experience teaches us that its practice is often productive of mischief. It is observed that the symptoms which accompany tinnitus aurium in severe cases are confused noises in the ears during the whole of the day, accompanied with pains in *the head*, and often complicated with some mental confusion. In others, the *noises occur periodically*, and most commonly at night, as soon as the head

is laid on the pillow; when either a tinkling, or a roaring noise like the waves of the sea breaking on the shore, has been heard. These symptoms are often aggravated by extraneous noises, and by intolerance of loud sounds. In cases such as these, the sense of hearing is greatly impaired. The history of the extraneous or internal disorders which prevail either before or at the time when the symptoms commenced, will furnish the best data for forming a correct diagnosis of the course and seat of the affection: but in doing this, great care and attention should be given, not only to the statements of the patient, but to the discrimination of his power of giving a correct account of his symptoms, and to the history of the causes by which they were assumed to be produced.

The maladies of the internal ear form one-third of the cases of deafness; and since these are seated in the labyrinth, a part inaccessible to manual operations, we see the fallacy of attempting to cure these affections by tampering with the external ear. The delicate and complex structure of the labyrinth requires that it should be protected by a strong, dense, and elastic covering, such as is found in the bony structure by which it is enveloped; but notwithstanding these natural means for security, they afford no absolute immunity from inflammatory attacks and extraneous injuries. With regard to the treatment of these intractable cases, it is obvious that when mental excitement prevails from irritation of the membranes of the brain, the exhibition of sedatives is necessarily indicated; and in the chronic state of inflammatory action of long standing, a seton in the neck, kept open during a lengthened period, has been productive of salutary effects, and it seems to be more beneficial than the usual treatment of applying blisters and counter-irritants behind the ears. The constitutional treatment must necessarily depend on the condition of the patient and the functional derangement concurrent in each case. It is owing to their variable states and conditions that much must be left to the discretion and judgment of the surgeon. It has been reported by Sir Gilbert Blane that two cases of tinnitus aurium were cured by means of belladonna. One was relieved after a short interval by its application to the ear externally; and the other was cured in three days by the administration of one-third of a grain of the same remedy three times a day. The symptoms were noises in the ears, like the rushing of wind. However, it is obvious that these cases described by Sir Gilbert Blane could not have had their seat in the vestibule or in any portion of the labyrinth: nor could they have been of the aggravated form we have been considering, since much more energetic treatment, and much greater length of time and care, have failed to procure relief; indeed, the second case appears to be an instance of nothing more than what is sometimes produced by the mere presence of wax in the auricles, or the effects of cold on the external ear. But when we turn to the consideration of cases of tinnitus aurium, arising from irritable conditions of the membranes of the brain affecting the mechanism of audition in the labyrinth, or morbid affections of the bones which protect the nerves of the internal ear, we may easily arrive at the conclusion that it is only by appropriate constitutional and local treatment that we can expect any satisfactory results: and that tampering with the ear by means of galvanism, injection of fluids or vapours, must necessarily be injurious to the patient; and we have the melancholy example of the fatal effects of injections into the tympanum of powerful poisons in the case of the late Mr. Toynbee.

In conclusion, it is now pretty well known to the medical profession that the greater the difficulties, and the more intractable the maladies affecting the human system, the greater are the opportunities for empirical practice. No faith should be placed in the agents for assisting the functions of the ear —such as are called "organic vibrators," "atomised fluids," &c.; and the same remark will apply to another subject, in the term "visible speech," since the phenomena of sight and hearing are separated physically in such a manner as to render the sight of a sound, or the perception of light by the ears utterly impossible. These phrases may therefore serve to amuse, if not to mislead the ignorant, but they cannot bear the test of scientific research. —*Lancet, July 27, 1867, p. 96.*

93.—NEW MODE OF EXAMINING THE MEMBRANA TYMPANI.

By Dr. W. Spencer Watson.

[There have been introduced within the last few years several forms of ear speculum, both convenient and efficient for the purpose intended. Dr. Watson thinks that focal illumination by means of a biconvex lens has some advantages not possessed by other means of illuminating the membrana tympani. It must be used in the same way as ophthalmic surgeons employ it for the examination of the crystalline lens and the vitreous chamber of the eye.]

The objection to the use of a polished speculum is that the reflected image of the membrana tympani or the lining membrane of the meatus causes some amount of confusion to the observer. Such, at any rate, has occurred to myself in the very limited experience that I have had of its use, and I have consequently had the inside of a bivalve speculum (made for me by Mr. Matthews, of Portugal-street) blackened, and without any reflecting surface whatever.

To compensate for the loss of illuminating power which the absence of a reflecting surface necessarily brings with it, I use a biconvex lens of 3½ inches focal length, which I hold at such a distance from the meatus that a cone of light is directed on the surface of the membrane. This has the effect of strongly illuminating the membrane itself, and at the same time keeping the surrounding parts in comparative darkness. For this purpose the lamp or other artificial light should be four feet from the head of the patient and behind the head of the observer.

It is to be observed that the cone of light should be of such a length that a diffused light should fall on the membrane, and the latter should be a little beyond the focus of the lens; otherwise the illuminated part would be too limited in extent to be practicably available for the purpose. By moving the lens nearer or further away from the meatus, this object can easily be obtained. When the meatus is sufficiently large in calibre, the speculum can of course be dispensed with.

I have generally found that, when obtainable, direct sunlight gives the best view of the membrane. But of course, if this means of illumination is used, it will not be safe to use a lens at the same time, nor is it at all necessary. It is, I think, a great advantage, when direct sunlight is used, to have a blackened speculum, if a speculum be employed at all.—*Medical Times and Gazette, Aug.* 31, 1867, *p.* 239.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

94.—ON THE ADMINISTRATION OF NARCOTICS TO PREGNANT WOMEN.

By the late Dr. Adams, of Banchory.

[Amongst the late Dr. Adams's posthumous papers, his son, Dr. Leith Adams, of the 22nd regiment, has found the following article; also a paper "On a Variety of Impetigo Contracted from Cattle," which the reader will find in the Edinburgh Medical Journal for November, 1867, p. 422.]

I have long been convinced that the administration of opium in full doses to pregnant women is highly dangerous to the child, and wish to call attention to the subject as being a point of great practical importance which would not appear to be sufficiently attended to. It must have struck every person of experience how easily infants are narcotised by opium. It has been affirmed that a single drop administered to a very young child has proved fatal. Mothers giving suck have frequently told me of the striking effects which opium and other medicines, taken by themselves, have produced on their sucking infants. One may readily infer, as the relation between the mother and child during gestation is more intimate than during lactation, that any powerful substance affecting the nutrition of the mother must have a still more powerful effect on the child in the former case than in the latter; and experience proves this to be the fact. A good many years ago, a midwife, whilst describing to me the case of a female of my acquaintance whom she had lately delivered, said, that when called to the woman, as she found that labour pains were slow of coming on, she gave her patient "a good large dose of laudanum," which suspended the process for about six hours; afterwards the pain set in strong, and in a very short time the child was born. I asked, "Was it alive?" The woman *naively* replied, "She never was more disappointed in her life, for that it was a remarkably fine, plump child, but was born in a dying state, and in spite of all she could do it never could be awoke from the lethargic condition, and died soon afterwards." I cautioned the worthy obstetrix against using the same medicine in future. I think I am also confident that opium given in cases of threatened abortion almost invariably ends in the expulsion of a dead fœtus, and therefore I think it should never be given unless when all hopes of prolonging gestation are gone. From personal experience, then, and reasoning from analogy, I cannot but think that the administration of opiates to pregnant women ought to be proscribed. I should be glad to know what is the experience of my professional brethren in such cases, as my attention has lately been directed to several published cases of a nature similar to that here stated.—*Edinburgh Medical Journal, Nov.*, 1867, *p.* 422.

[How far Dr. Adams may be right in explaining the mode of action of the opium we cannot say; this, however, we have often noticed, that opium will act like secale cornutum and cause almost a violent *uninterrupted* action of the muscular parietes of the womb, thus stopping or considerably diminishing the circulation in the womb, and consequently, if prolonged too far, causing the child to die of *asphyxia*.—Ed.]

95.—OBSERVATIONS ON THROMBOSIS AND EMBOLISM OF THE PULMONARY ARTERY AS A CAUSE OF DEATH DURING THE PUERPERAL STATE.

By Dr. W. S. Playfair, Assistant Obstetric Physician to King's College Hospital.

[Out of twenty-five cases of death due to pulmonary obstruction in the puerperal state, in twelve there were either well-marked symptoms of phlegmasia dolens, or the veins of the lower extremities contained coagula. In all these cases death occurred after the 18th day from delivery. In all those cases in which no clots were found in the peripheral veins, with a single exception, death occurred before the eighteenth day. This difference is curious, but it is capable of explanation.]

The first step towards the production of an embolus is admittedly the formation of a coagulum in some of the peripheral veins. But no sooner is the fibrin deposited than it begins to undergo certain changes, which have been carefully studied by Virchow and other pathologists, the primary object of which is evidently to produce absorption of the coagulum, so as to restore the circulation in the occluded vessel. It is unnecessary to describe here what these changes are. They consist chiefly in the retrograde metamorphosis of the fibrin, which is generally either an amylaceous or fatty degeneration. The result is that the coagulum becomes softened; and then some accidental cause gives rise to the detachment of a portion, which, being carried to the right side of the heart and thence to the pulmonary artery, at once produces the phenomena of embolism. It can be readily understood, therefore, why the fatal result should occur after a considerable lapse of time, and when convalescence seems to have been fairly established. For, first, we have the local clotting, due very probably to a similar blood dyscrasia as that which produces spontaneous thrombosis in the pulmonary artery; and it is not till the clot so formed has had time to change and soften that portions of it become detached and form the embolus which proves fatal. When spontaneous thrombosis occurs, on the other hand, it must arise from the state of the blood rendering it peculiarly apt to coagulate; and this is more likely to be met with at a much nearer period to delivery. The pulmonary obstruction may then, indeed, be considered as taking the place of, and occurring about the same time as, the peripheral coagulation in undoubted cases of embolism.

It is interesting to observe how the condition of the blood in the puerperal state increases the tendency to thrombosis, and in this we have a sufficient explanation of the frequency of its occurrence after delivery. Indeed there is probably no other condition of the body in which so many circumstances concur to favor coagulation. For not only is there an actual excess of fibrin as the result of pregnancy, but a vast quantity of material destined for excretion is circulating in the blood for some time after delivery, the product of the involution which is going on to reduce the uterus to its usual size. And, in addition to this morbid constitution of the blood, an examination of the cases will show that when spontaneous thrombosis has occurred, the patient has, in a large majority of instances, been in a very feeble and exhausted state. Thus, out of the fourteen cases in which no clots were found in the veins after death, in no less than eight was there either post-partum hemorrhage, or the patient was expressly described as being extremely weak and anæmic. Now, taken by itself, the state of the blood thus produced would, independently of all other circumstances, greatly increase the tendency of the fibrin to become deposited. Dr. Richardson's opinion is very clear on this point. "There is," he observes, "a condition which has been long known to favour coagulation and fibrinous deposition. I mean loss of blood, and syncope or exhaustion during impoverished states of the body. The explanation here is, that these states are attended, as a general rule, by an excess of fibrin in the blood, as well as by an excess of water. The fibrin solvent is thus widely distributed, the density of the blood is reduced, the *fibrin superabounds*, and stasis only is required to give all the circumstances

favourable to deposition." When, therefore, we have the alteration so produced, added to the already vitiated state of the blood due to puerpery, we cannot but regard the occurrence of thrombosis as a very probable circumstance.

It would be a question of great importance to decide if there is a sufficient difference in the character of the clots found in the pulmonary arteries to enable us to decide by inspection whether they are due to spontaneous thrombosis or embolism. Dr. Ball seems inclined to answer in the affirmative. He believes, if I understand him aright, that in a clot depending on embolism the embolus may be observed of a similar texture to the clots in the peripheral veins from which it was originally derived, surrounded by more recent fibrin, which can be readily distinguished from it; while, on the other hand, clots of spontaneous origin are identical in appearance with thrombosis of the peripheral veins, being dense, firm, white in colour, homogeneous in structure, and having a smooth, rounded head pointing in the direction of the heart. This form and direction he explains on the hypothesis that spontaneously formed clots always originate in the minute ramifications of the pulmonary arteries, gradually creeping backwards towards the larger branches. For this assumption, however, I do not see sufficient grounds. Dr. Ball considers it difficult to believe that the blood can coagulate in the primary divisions of the pulmonary arteries, because the propulsive movements of the heart would disturb it, and effectually prevent the separation of the fibrin. Dr. Humphry, however, has shown that the anatomical arrangement of the artery is such as to favour stasis and coagulation of the blood in the main trunks at the root of the lungs. He observes that "In this situation the artery breaks up at once into a number of branches, which radiate from it, at different angles, to the several parts of the lungs. Consequently a large extent of surface is presented to the blood, and there are numerous angular projections into the current; both which conditions are calculated to induce the coagulation of the fibrin." And if, as I have pointed out, it be true that spontaneous thrombosis generally occurs in exhausted and enfeebled patients, subject to syncope, and in whom there is a very weak action of the heart, it seems to be by no means unlikely that the deposition may primarily take place in the larger branches, where it is found after death. However, I think that we are scarcely yet in a position to speak dogmatically on this point. When more attention has been paid to the characters of the clots in various cases, and when the results of a larger number of post-mortem examinations have been recorded, it is by no means improbable that much light may be thrown on the origin of the obstruction.

A question of great interest arises as to whether recovery does not in some cases take place after the pulmonary artery has been occupied by a coagulum. This is a point of much importance, which has as yet scarcely received any attention. We are every now and then meeting with cases after delivery in which there are formidable symptoms of syncope, attended by embarrassed respiration and all the phenomena usually ascribed to embolism, in which, nevertheless, a more or less perfect recovery eventually takes place. Such cases have been usually ascribed by authors who have written on the subject to what they call "idiopathic syncope," a term invented to account for phenomena otherwise inexplicable. The symptoms, however, are so precisely those of pulmonary obstruction that it seems far from unlikely they may really be due to that cause. Mr. Paget has published a case of pulmonary thrombosis in a male patient, from which he draws the inference that "in certain circumstances a great part of the pulmonary circulation may be arrested in the course of a week (or a few days more or less), without immediate danger to life, or any striking indication of what has happened." Virchow also found clots in the pulmonary artery in several cases in which there had been no symptoms during life to give rise to a suspicion of their existence.

The history of many of the fatal cases render it quite certain that the deposition of the fibrin had existed for a considerable time without causing much distress, until some unusual exertion induced a call for blood which

could not be supplied through the obstructed vessels, and hence the sudden syncope which carried off the patient. Is there anything improbable in the supposition that some of the cases of so-called "idiopathic syncope" may really have been of the same kind, with this exception, that the patient rallied and survived, and that the obstruction in the artery has been eventually removed?

Clots in the peripheral veins are certainly frequently absorbed, and the circulation in the occluded vessels becomes as perfect as ever. So strong is the tendency to this that Humphry observes with regard to it:—"It appears that the blood is almost sure to revert to its natural channel in process of time, unless the vein be completely destroyed." If this, then, be so frequent an occurrence in the venous system generally, surely it is more than probable that it may now and then occur in the pulmonary artery also. The following cases, which have come under my own observation, seem to me to be explicable on no other hypothesis.

Case.—Post-partum hemorrhage; symptoms of syncope on the eighth day; recovery.—On the 27th of April, 1864, I attended a young lady, 23 years of age, in her first confinement. She was a healthy person, and had a perfectly natural labour. It was followed, however, by hemorrhage, but not to any excessive amount; and this was soon checked by pressure on the uterus, the administration of ergot, and cold applications. On the sixth day the lochia became very fetid, but were not diminished in quantity. In all other respects she seemed very well. She was, indeed, weak from loss of blood; but her pulse was under 100, the skin was cool and moist, there was no tenderness over the abdomen, and there was an abundant secretion of milk. I ordered her injections of tepid water and Condy's fluid, to remove the fetor of the discharges. On the next day she had one or two fits of hysterical crying, and felt low and depressed. The lochia was still intensely fetid, but she was no worse in any other way. On the following morning (the eighth from her delivery) she awoke feeling better and ate a hearty breakfast. About half an hour afterwards the nurse, going to her bedside, found her in a state of collapse, gasping for breath, and unable to speak. She immediately gave her some brandy, but with little effect. I was sent for at once, but in the meantime, her condition being so alarming, they also sent for the nearest medical man, Mr. Willington, of Sumner-place, who administered brandy, ammonia, chloric ether, &c., freely. I did not reach her until an hour and a half after her first attack, when I found her somewhat rallied. She was then ghastly pale, with an anxious, pinched look about the face. The pulse was 110, soft and compressible. The respirations were only slightly increased in frequency, and no abnormal sounds were audible about the heart and great vessels. She was ordered a mixture containing sesquicarbonate of ammonia every hour, and a dessert-spoonful of brandy every second hour. The most absolute rest was enjoined, and the nurse was directed not even to allow her to raise her head off the pillow. She felt "faintish" every now and then for several days, but eventually made a perfect recovery, and has since had another child, after a natural and easy labour.

The second case is perhaps still more to the point.

Case.—Severe post-partum hemorrhage; intense syncope a week after delivery; frequent recurrence of the attacks; recovery.—This case occurred in a patient of Dr. Arthur Farre's, with whom I attended. A young lady, of feeble and delicate health, was confined of her third child on the 19th of October, 1866. She had an easy labour, which was followed by very profuse hemorrhage. All the ordinary means—pressure, ice, the cold douche, ergot, &c.—failed to check the flow, which was eventually arrested by injecting the cavity of the uterus with a solution of perchloride of iron. She was much exhausted, her countenance was blanched, and she had sighing respiration. Perfect rest was enjoined, and for a week she seemed to be doing well, taking strong essence of beef, and six drachms of brandy every second hour.

During the whole of the seventh day after her confinement she felt unusually weak, complained of faintness, and said that her head felt dizzy whenever she moved it. About six o'clock the same evening she suddenly called

out that she was dying, and fell into a state of syncope. She rallied on the administration of stimulants, and when I saw her, about an hour after the attack, she was still highly excited, and said she felt as if she was going to die; but the respiration was not then rapid, the pulse was feeble and small, about 130; the hands and feet were numb and cold. The most absolute rest was enjoined, and stimulants and restoratives were administered freely. For several days she remained in a very critical condition, the slightest exertion bringing back the tendency to faint. On one occasion I detected a faint blowing murmur at the base of the heart, but it was not permanent. The auscultatory phenomena were in other respects perfectly normal.

For nearly two months she continued in the same feeble state. As long as she remained in the recumbent position she felt pretty comfortable, but any attempt at sitting up in bed, or any unusual exertion, immediately brought on embarrassed respiration. It was also found necessary to continue the same amount of stimulants she had been taking from the first, as any diminution in the quantity at once brought on a tendency to syncope. She is, as I write, getting slowly and gradually better, but is unable to sit upright for more than a few minutes at a time.

The next case I did not see during the attack, but the circumstances were related to me by the patient herself, a person of great intelligence, and I took every means in my power to satisfy myself as to the details of her illness. I have therefore no hesitation in recording it, and believe her account of the case to be substantially correct.

Case.—Symptoms of pulmonary obstruction eleven days after labour; phlegmasia dolens; recovery.—On August 20th, 1866, I was consulted by a lady 44 years of age, the mother of twelve children. She complained of general weakness and debility, and she had phlegmasia dolens on the left lower extremity, which was hard and swollen up to middle of thigh. She informed me that she had had an easy labour on the 6th of July. She made a good recovery, and on the eleventh night went to bed feeling very well. There was no swelling or discomfort of any kind about the lower extremities at this time. About half-past three a.m. she was sitting up in bed, when she was suddenly attacked with an indescribable sense of oppression in the chest, and fell back in a semi-unconscious state, gasping for breath. Her husband, greatly alarmed, went for the nearest medical man, who administered stimulants freely. She remained in this condition for about three hours, when she commenced to rally. The secretion of milk, previously abundant, was suspended. Two days *after* the attack, symptoms of phlegmasia dolens came on; the left leg swelled, and has since remained in the same condition as it was when I saw her. She has since been very feeble and out of health, but there has been no recurrence of syncope.

Now in all these three cases the symptoms and history are exactly those of pulmonary obstruction. Had the patients died, and no post-mortem examination been allowed, I think no one would have hesitated a moment as to the diagnosis. They also illustrate well the points in the history of the disease I have endeavored to explain. In two out of the three there was postpartum hemorrhage, and a tendency, therefore, to coagulation from the altered condition of the blood. In all the three the attack occurred before the twelfth day after delivery—just the time, as I have previously observed, at which spontaneous thrombosis most generally takes place. The last case was certainly complicated with phlegmasia dolens; but this did not show itself until two days after the chest symptoms. On this point there can be no doubt, as the patient was perfectly sure that, at the time of her attack, no swelling of the leg existed. I believe, therefore, that this is an instance of pulmonary and peripheral thrombosis occurring simultaneously, from some condition of the blood tending to coagulation. I have no doubt that many practitioners have met with similar cases, which seemed at the time inexplicable; and it is in all probability simply because the attention of the profession has not been directed to the subject that more are not recorded.

Dr. Arthur Farre agreed with me as to the probable cause of the attack in his case, and informed me that he had seen more than one case in his practice

which he now believed to have been caused by pulmonary obstruction, and in which the patients eventually made a good recovery.

If I have rightly interpreted these cases, they teach us the lesson that we should not entirely despair when we meet with a case presenting the symptoms of embolism. Unfortunately, the question of treatment is one on which little can be said. By far the most important indication is, doubtless, to enforce the most rigid rest, scarcely even permitting the patient to move for a long time after the occurrence of the suspicious symptoms, so as to enable sufficient blood to pass through the obstructed vessels to carry on the animal functions until a free channel for the circulation is in the course of time restored. The history of all the fatal cases clearly shows how dangerous is any action which calls for an increased supply of blood; for the fatal attack has almost invariably been the result of some unusual exertion. In addition to this, a free administration of stimulants, and a liberal supply of strong animal soups, will be indicated, to support the strength as much as possible. It is to be feared that no medicinal agents are likely to be of any service, except in the way of stimulants and restoratives.

No less than twelve out of the twenty-five cases occurred in primiparæ; while of the remainder there are several in which nothing is said as to whether the patient had previously borne children or not. Whether this is a mere accidental circumstance, or whether there is actually a greater tendency to deposition of fibrin in first than in subsequent labours, I am unable to say. It is sometimes said, on the other hand, that phlegmasia dolens is more common in multiparæ. Probably a much larger mass of statistics is required before we can come to a conclusion on this point, but as far as our present cases go the number of primiparæ greatly predominates.

Finally, I think the facts before us justify the following conclusions:—

1. Obstruction of the pulmonary artery after delivery may depend either on embolism or spontaneous thrombosis.

2. The former usually occurs at a much later period after delivery than the latter; and spontaneous thrombosis probably corresponds with, and is due to some cause similar in its nature to, that which produces the obstruction of the peripheral veins in true cases of embolism.

3. Both thrombosis and embolism are much more common in patients who are anæmic and weak, either from hemorrhage or other cause.

4. It is probable that obstruction of the pulmonary artery sometimes occurs without proving fatal.

5. The main element in the treatment of such cases is the most rigid rest, and a nourishing supporting regimen.

6. As far as present statistics go, thrombosis and embolism seem more common in primiparæ than in multiparæ.—*Lancet, July 27 and Aug. 10, 1867, pp. 94, 153.*

96.—ON SAFE DELIVERY FROM THE PAINS OF LABOUR.

[Mr. Isaac Brown, in his work on this subject, recommends]—

For the purpose of procuring "painless parturition with full mental consciousness," one part of rectified eau-de-Cologne with two of chloroform. The result is, he says, a clear, colourless, and highly agreeable liquid, having a specific gravity of 1·152.—*British Medical Journal, Sep. 7, 1867, p. 208.*

97.—ON THE USE OF THE FORCEPS IN LABOUR.

By Arthur B. Steele, Esq., Lecturer on Midwifery, Liverpool Royal Infirmary School of Medicine.

[The opinions given and the rules laid down by different writers on the use of the forceps are very various. Referring particularly to the following sentence *from Dr. Churchill's* writings, "in no case is the forceps to be applied until

we are perfectly satisfied that the obstacle cannot be overcome by the natural powers with safety to the mother and child," the writer observes:]

Those who fully appreciate the evil results of protracted labour, will hold that its fulfilment implies interference at a very much earlier period than would be deemed proper by others, who maintain that the natural course of labour should not be interfered with, until certain definite symptoms of approaching danger have appeared. What I desire chiefly to enforce is, that the use of the forceps should not be deferred until bad symptoms have appeared, but that they should as a general rule be applied at once, whenever the second stage of labour ceases to be actively progressive; excepting of course when contraction or distortion of the pelvis absolutely necessitates turning or craniotomy, or when the os uteri is only partially dilated, and at the same time undilatable.

I hold that it is not necessary to wait for six, four, or even one hour, so long as we are satisfied that the labour, if left to nature, will be tedious and protracted; a point which, I admit, may require much judgment and experience to determine. I altogether discard the rule that before the forceps are applied the ear should be felt, neither is it always necessary to wait until the os uteri is fully dilated; for with ordinary care the blades may be passed beyond the os uteri without inflicting the least injury, and with properly constructed forceps the os uteri is not stretched until the head itself is brought down to assist in the dilatation in the natural way.

When the fœtus is known to be dead, although under certain circumstances craniotomy, as usually recommended, may be more suitable, yet, as a general rule, I prefer to deliver a dead child by the forceps, for two reasons; first, because, as we have seen, perforation is fatal in one case in five, while forceps mortality amounts to one only in thirty-three; and secondly, assuming even that craniotomy is not more dangerous than the forceps, it is objectionable on other grounds as being, in appearance at least, a repulsive and somewhat barbarous proceeding, even though the child be dead. It must never be forgotten that, although we often have clear and positive evidence that the child is living, we cannot so surely determine that it is dead, although the usual signs of its vitality are absent.

I reject, as opposed both to theory and to experience, the notion that the forceps ought never to be used as compressors. I believe, on the contrary, that when properly applied, and skilfully manipulated, they conduce materially to the moulding of the head in its passage through a tight pelvis; and I have more than once proved in practice that a child may be delivered alive and unharmed, when a considerable amount of compressing force has been required. I advocate the early application of the forceps, because the earlier we apply them the better will be the position in which we grasp the head, and because we thus anticipate the disadvantages which ensue when the head is left to press for a long time on the soft parts of the mother, causing congestion and effusion, and so leaving less room for operating at a more advanced stage, besides rendering the vagina more liable to injury.

I recommend as the best instrument for all cases, the long curved forceps of Simpson or Roberton; and on this point I speak with much confidence, from considerable practical experience, having found them more easy of application, and much more satisfactory in their effects, than the short forceps. I have for several years abandoned the use of any other except Roberton's forceps, and I feel quite sure that any practitioner, who will give them a trial, will arrive at the same conclusion. The chief points to be borne in mind in operating, besides the general directions of authors, are as follows: The operator should have constantly in his mind's eye, the curved line described by the head in its passage through the parturient canal, and direct his extractive force in accordance therewith; it will almost invariably be found that the rotation of the head as it passes through the pelvis, will be effected by nature, without assistance from the instruments, thus rendering any rotatory motion with the forceps unnecessary. The extraction should never be unduly hurried; nay, often, when the head is at the outlet it is necessary even to delay it, and thus save the perineum. This is a use of the forceps

which I think has not been sufficiently dwelt upon, and which can only be satisfactorily accomplished by the long curved forceps.

I repudiate the dogma, that force is never to be used in artificial delivery: in employing it judiciously we imitate nature herself, and it is only when abused that it becomes dangerous. I have over and over again verified in practice that, with proper precaution, and with appropriate manipulation, delivery may be accomplished with safety to both mother and child, even when considerable force has been necessary to effect the extraction of the head.

I regard the use of the forceps as more appropriate in those cases in which many obstetricians recommend the ergot of rye, or other oxytoxics, for the following reasons: it gives the accoucher a more certain, regular, and complete power over the course of labour, especially during the exit of the head, enabling him either to hasten or retard its progress as circumstances may require.

Their use does not involve certain risks which are admitted to attach to the action of ergot, even by those who advocate its employment in tedious labour. Dr. Johnson, for instance, says, "That when ergot is given it brings matters to a crisis, for if the child be not delivered within a certain time it will in all probability be destroyed." Drs. Hardy and McClintock state they "have on many occasions observed the ergot of rye to exercise a very depressing influence upon the maternal circulation." More recently Dr. McClintock has advocated the view, taught by Dr. Robert Lee when I was a student, that danger to the child arises from the incessant uterine action, and the consequent pressure, which the ergot is so liable to occasion. Dr. Churchill attributes to its use "retention of the placenta, from irregular uterine action, after the birth of the child." (*Theory and Practice of Midwifery*. 1856. *p.* 264.)

In my own practice I rarely use it in actual labour, preferring it as a means of completing abortion, when all hope of saving the ovum is abandoned, or of producing contraction in *post partum* hemorrhage. It is my firm conviction that the more the forceps come into favour, the more rarely will ergot of rye be used, and that ultimately it will be altogether set aside as a substitute for what I cannot but regard as a safer and more efficient means of affording assistance in tedious labour.—*British Medical Journal*, *May* 11, 1861, *p.* 353.

98.—ON THE POWERS OF THE FORCEPS.

By Dr. Robert Barnes, Physician to and Lecturer on Midwifery at St. Thomas's Hospital, &c.

The Powers of the Forceps—the Force by which it holds the Head—the Compressibility of the Child's Head.—Three distinct powers or forces can be developed in the forceps. First, by simply grasping the head and drawing upon the handles, it is a *tractor*, supplementing a *vis à fronte* for the defective *vis à tergo*. Secondly, the forceps consisting of two blades having a common fulcrum at the joint or lock, we can by a certain manipulation use it as a *double lever*. Thirdly, if the blades and handles are long enough and strong enough, and otherwise duly shaped, the forceps becomes a *compressive power* capable of diminishing certain diameters of the child's head, so as to overcome minor degrees of disproportion.

Now, all these powers may be brought into use, and all may be in great measure lost, according to our choice of a good or a bad model. Thus, if we rest satisfied with the short forceps of Denman, we shall only have a feeble tractor, a feeble lever, and an instrument having almost absolutely no compressive force. It is obvious that such a forceps can have but a restricted application. It can only serve to deliver the child when the head is in the pelvis, when very little tractile power is required. Ask yourselves what this means. What are the consequences in practice? Simply this: you are driven in a multitude of cases to perforate, to destroy the child. Such an alternative may well make us reflect whether we cannot extend the powers and the application of the forceps. By simply lengthening the blades and

shanks and giving the blades an additional curve adapted to the curved sacrum, we can reach the head detained at the brim of the pelvis. By moderately lengthening the handles and making the instrument stronger, we increase the leverage and tractile power, and we gain a moderate compressive power. Thus we bring within the saving help of the forceps a further number of children that must otherwise be given up to the perforator, or run the risk of turning. You ask, Why hesitate to endow the forceps with this great privilege? Why has the feeble forceps of Denman so long held its sway in this country? The reason is that there are limits beyond which we cannot push the saving powers of the forceps. If we pass beyond these limits, we run into danger of injuring the mother and of losing the child. Now, the great contest in all matters of strife is about boundary lines; and it is concerning these limits that authorities have differed. Some men are afraid of giving power, lest it should be abused. They are so terrified at the possible mischief which great power may work, that they would rather abandon the good which great power is equally capable of working. They tremble lest we should be unable to acquire the skill and the discretion necessary to direct the greater power. Such men virtually say, You shall not apply the forceps where the head has not descended into the pelvic cavity—an arbitrary limit dictated by fear, and fixed by ignorance, that the forceps is just as capable of safely delivering a child whose head is arrested at the brim. For here, as is contiuually the case in medicine, experience arbitrarily limited excludes progress in knowledge and bars improvement in practice. For example, how can a man acquire a just knowledge of the power of the forceps to deliver a head delayed by slight disproportion at the brim, if he always delivers under this difficulty by perforating? Clearly, he bars himself from acquiring that knowledge; and, giving up his intelligence to the delusive dictates of his wilfully limited experience, he refuses even to accept the evidence of those whose experience is greater, because it is directed by a freer spirit of research, by greater confidence in the resources of art.

Let us, then, go back to the study of the powers of the forceps, unshackled by any preconceived opinions as to what the instrument can do or can be permitted to do. First, as to its *tractile* powers. In order to draw, the instrument must take hold. How does it take hold? You may at first sight suppose that this is accomplished by grasping the handles. But in the case of the ordinary forceps, especially the short-handled forceps, there is little or no compressive power, so that the hold cannot be due to the handles. The hold is really due to the curvature of the blades, which fit more or less accurately upon the globular head, and the compression of the bows of the blades against the soft parts of the mother, supported by the bony ring of the pelvis. This may be made clear by a simple experiment. Take an india-rubber ball, slightly larger in diameter than a solid ring; place the ball upon the ring. Then seize the ball through the ring by the forceps. The blades will be opened out by the ball. Then drawing upon the handles, even without squeezing them together, you will see the blades pressed firmly upon the ball by gradual wedging, as the greatest diameter or equator of the ball comes down into the ring. Just so is it with the child's head and the pelvic brim and canal. The blades are held in close apposition to the head by the soft parts and the pelvis of the mother. In many cases this outward pressure upon the bows of the blades is enough to serve for traction. It is not necessary to tie the handles of the forceps. You may even do without handles altogether. Thus, one of the earliest attempts, stimulated by the desire to realize the concealed discovery of the Chamberlens—that of Palfyn—consisted in applying two opposed levers, which did not cross, and therefore could not exert any compressive force. Assalini's forceps was constructed on this principle. It is essentially a tractor, with slight leverage power. Professor Lazarewitch, of Charkoff, brought to the Obstetrical Exhibition a beautiful forceps constructed on Assalini's principle. This instrument I applied in two cases. It held admirably; but all its holding power is due to the pressure exerted by the mother's parts upon the blades.

Mattéi, of Paris, has made another instrument, whose blades do not cross,

whose shanks parallel are set in a cross bar of wood to serve for traction; and quite recently Dr. Inglis has proposed a forceps in which the handles are done away with altogether, there being nothing but a short curve of the shank, representing the shoulders on the handles of Simpson's forceps, to serve for traction. I think this sacrifice of all compressive and leverage power, reducing the instrument to a weak tractor, is a retrograde movement. But it proves the proposition that the hold upon the child's head is the result of the adaptation of the curved blades and the outward wedge-pressure of the mother's parts upon the bows of the blades. Now, the strength of the hold depends mainly upon the degree of curvature of the blades and the width of the fenestræ. If the curve is one of large radius, so that the two blades, when in opposition, approach parallelism, and especially if the fenestræ be narrow, the hold will be feeble, and moderate traction will cause the forceps to slip, and this in spite of any compression you can exert upon short handles. But increase the curve so that the blades in opposition form nearly a circle, and the instrument will not slip. This increased head-curve is one feature of the French or Continental forceps. The hold is further strengthened by making the points approach nearer together. In the English patterns the points are generally distant from each other an inch or more. In the foreign forceps the distance is often much less than an inch. There is some danger from this proximity of pinching or abrading the skin of the face. So much for the grip and traction.

Let us now study the *compressive power*. This is inconsiderable in almost all the English forceps, but is an important feature in most of the foreign long forceps. The essential condition for compression is, indeed, present in English and foreign. This consists in the crossing of the blades, and in the greatest divergence of the blades, when the handles are brought together, being less than the greatest transverse diameter of the child's head. This diameter is normally from 3¼ to 4 in.; the greatest divergence of the blades is rarely more than 3 in. Therefore, when the blades are sitting loosely on the head, the handles diverge. Practically, the head is rarely grasped exactly in its transverse diameter, but generally in one more or less oblique—something between the transverse and the longitudinal diameter. This, of course, is even longer than the transverse. Now, if we are to exert any direct compression upon the head, we can only do it by squeezing the handles together. For this purpose, the handles must be long and strong on one side of the lock, and the blades must be strong, but not much longer, on the other side of the lock, than are the handles. It would be useless to provide this compressing power if the head were not compressible. That the head is compressible—that is, that we may diminish some of its diameters by lengthening others—is easily proved.

Firstly. It is known that in normal pelves the head in passing, if the labour be protracted, undergoes elongation; from round it becomes conical; the greatest transverse diameter—the interparietal—becomes merged in the lesser or interauricular, whilst the longitudinal diameters are correspondingly increased.

These changes I have demonstrated by actual measurements and outlines.

Thus, just as the pressure of the soft parts and the pelvis is a main agent in fixing the forceps upon the head, so it is in moulding the head to allow of its passing. Indeed, I think this pressure almost entirely accounts for the alteration of form the head undergoes when the English forceps is applied. I can show outlines of heads as strongly altered under the natural forces of labour as they often are under forceps delivery.

Secondly.—Numerous experiments have been made with strong forceps upon dead children to determine this point. Baudelocque found that he could lessen the transverse diameter by a quarter to a third of an inch. Siebold gained half an inch. Osiander and Velpeau claim quite as much. More conclusive are the observations of M. Joulin and M. Chassagny. These *gentlemen*, in experiments designed to demonstrate the utility of continuous *compression* and traction by powerful forceps upon the head in difficult

labour, have completely proved that a degree of moulding may be effected much beyond that commonly observed. This moulding consists in the elongation being gained by the lessening of the equatorial diameter. The process resembles that of reducing wire by drawing it through holes in an iron plate.

Now, another question arises: the head is indeed compressible, but to what extent is it compressible without sacrificing the child's life? For if the maximus of plasticity compatible with life is represented by that degree which is common in severe first labours, then we ought to give the mother all the ease in our power by lessening the diameter of the child's head by perforating. It is very difficult to fix this limit with accuracy. Baudelocque thought compression to the extent of a quarter or a third of an inch was compatible with the safety of the child. The important fact is, that in many cases the child survives, although its head has undergone very great compression and moulding.

The following conditions influence the result:—The degree of development of the head as to size and ossification; and the mode in which the compressing force is applied. If this force be applied *gradually* and *continuously*, a much greater extent of moulding with less injury to the child may be obtained than what Baudelocque thought possible.

At one time it was the practice—more probably with the view of securing the hold than of compressing the head—to tie the handles together; and even now that tying is generally abandoned and condemned, the old custom asserts itself in the preservation of the grooves near the extremities of the handles made to receive the ligature. The objection to tying is this—the continuous compression is opposed to the course of nature which intermits the expulsive act, giving periods of rest during which it is presumed that the brain may better adapt itself, and its circulation be maintained. Hence the law that we ought in forceps labours, and, generally, in all operative labours, to imitate this intermitting action, by interposing intervals of rest, endeavouring so to time our efforts as to be simultaneous with, and in aid of, the natural expulsive efforts. The argument is good both in logic and in physiology. It is not wise to disregard it. But experience proves that there are cases where the moulding of the head can be accomplished more quickly, and without endangering the child, by continuous pressure. Some practitioners, therefore, have recurred to the old practice. Dr. Gayton has adapted a clip to the handles of the forceps, which answers much better than the ligature. Whatever the mechanism resorted to, it is essential that it admit of being instantaneously removed, in order to allow the blades to be taken off. Delore, who has made many dynamometric observations, concludes that pressure exerted either by the forceps or by the genital organs may be harmless to the head if spread over a large surface. It is limited and angular pressure that is dangerous. He has also shown that *the greater the traction the greater is the pressure.* The pressure is equal to about half the traction. Thus, if you exert a traction force of fifty pounds, the pressure upon the head is about twenty-five pounds.

To economise traction, then, is to economise pressure. How do we economise traction?

There are three principal rules.

First.—Take sufficient time to allow the head to mould.

Secondly.—Take care to draw in the axis of the brim—that is, traction must be perpendicular to the plane of the brim. If this is neglected, additional force is required, increasing with every degree of angular difference.

Thirdly.—To use slight movements of laterality or oscillation.

This uncertainty and inconstancy in the degree to which compression may be carried with safety to the child is a justification for tentative or experimental efforts with the forceps. It is the reason why, in doubtful cases, where the disproportion in size between pelvis and head is not very decided, we are called upon to make a reasonable trial of the forceps before resorting to craniotomy. It appears to me quite certain that in this country we are yet far from having utilised the powers of the forceps to the highest legiti-

mate extent. I might go further, and say that during Denman's time and until quite recently we had actually lost ground in this respect, and had reverted to the use of instruments scarcely better than the original rude forceps of the Chamberlens. More than one hundred years ago Smellie contrived and used the long forceps. Perfect used it, and it seems that in his time the long forceps was better known than it was at the beginning of the present century.

Application of the Long or Double-Curved Forceps.—The application of this instrument is governed by a different law from that which governs the use of the short forceps. The short forceps, according to the recognised rule, must be applied with the blades quite or *nearly* over the transverse diameter of the head. The head determines the manner of applying it. But with the long forceps it is the pelvis that rules the application. The position of the head may be practically disregarded. The pelvic curve of the blades indicates that these must be adapted to the curve of the sacrum in order to reach the brim. They must therefore be passed as nearly as may be in the transverse diameter of the pelvis. One blade will be in each ilium, and the head, whatever its position in relation to the pelvic diameter, will be grasped between them. The universal force of this rule much simplifies and facilitates the use of the instrument. Not only does it apply to the position of the head in relation to the pelvic diameters, but also to all stages of progress of the head from that where it lies above the brim down to its arrest at the outlet.

It has been contended that the short forceps should be preferred in cases where the head is arrested in the cavity, and as a corollary it is urged that in cases of arrest at the brim, where the head has been brought into the cavity by the long forceps, this instrument, after serving so far, should be discarded and replaced by the short forceps. I do not concur in this view. I doubt whether any one who has had any considerable practice with the long forceps has found it worth while to change instruments in the course of delivery. The long forceps possesses a more scientific adaptation to the pelvis throughout the whole canal than the short forceps. And if the long forceps is found in practice capable of taking the head through the pelvis from brim to outlet, it follows that, since the whole contains the parts, the long forceps is qualified to take up the head at any point below the brim.

The pelvis has been compared to a screw. I think a better idea may be formed of its mechanical properties by comparing it to a rifled gun, and the child's head to a conical bullet. But even then the comparison is not complete, for the pelvis, unlike a gun, is a curved tube. Now, just as the head must traverse the pelvis in a helicine course, determined by the relation of form between pelvis and head, so is it natural that an instrument designed to grasp the head should be so modelled as to be fitted to follow this helicine course during introduction and extraction. This indication a well-modelled long forceps fulfils; no single-curved forceps can fulfil it.

First, as to the application when the head is delayed at the brim.

Mode of Applying.—Position of the Patient.—The patient should lie on her left side, the knees drawn up towards the abdomen; the head should be only slightly raised. She should lie across the bed, with the nates near the right edge, about midway between the head and foot. This will facilitate the introduction of the blades, and give room for the sweep of the handles round the pubes at the end of the operation. I do not find it necessary to bring the nates to hang over the edge of the bed. I have often passed both blades when the patient has been lying in the middle of the bed. Sometimes it is very desirable to move her as little as possible.

First Act—Introduction of the Blades.

Selection of the Blades.—First dip them in warm water, wipe dry, and lubricate with oil, or lard, or cold cream. Join them, and, holding the instrument with the concavity of the pelvic curve forwards, and the blades in the positio

which they are to occupy in the pelvis, you take that one first which is to lie in the left or lower side.

First Stage.—One or two fingers of the left hand are passed inside, in at the perineum and between the cervix uteri and the head. Then, bearing in mind the relative forms of the instrument, the head, and the pelvic canal, the point of the blade is passed along the palmar aspect of the fingers at first nearly directly backwards towards the hollow of the sacrum.

Second Stage.—The handle is now raised so as to throw the point downwards upon the left side of the head. As the point of the blade must describe a double or compound curve—a segment of a helix—in order to travel round the head-globe, and at the same time to ascend forwards in the direction of Carus' curve so as to reach the brim of the pelvis, the handle rises, goes backwards, and partly rotates on its axis.

Third Stage.—The handle is now carried backwards and downwards to complete the course of the point around the head-globe and into the left ilium. Slight pressure upon the handle ought to suffice. This will impart *movement* to the blade; the *right direction* will be given by the relation of the sacrum and head. The blade is now *in situ;* the shank is to be pressed against the coccyx by the back of the operator's left hand whilst he is introducing the second blade.

Introduction of the Second Blade.

First Stage.—Two fingers of the left hand, the back of which is supporting the first blade against the perineum, are passed into the pelvis between the os uteri and the side of the head which lies nearest to the right ilium. The instrument held in the right hand lies nearly parallel with the mother's left thigh, or crossing it with only a slight angle. The point of the blade is slipped along the palmar aspect of the fingers in the vagina, across the hank of the first blade *in situ*, inside the perineum towards the hollow of the sacrum.

Second Stage.—As the point has to describe a helicine curve to get round the head-globe and forwards in the direction of Carus' curve, the handle is now depressed and carried backwards until the blade lies in the right ilium. When it has reached this position the handle will be found near the coccyx, nearly in opposition to the first blade.

The Locking.—This is effected by a slight movement of adaptation. A handle is seized in each hand. The handle of the first blade is brought a little forwards over the handle of the second blade. If one blade is a little deeper in the pelvis than the other, it is either brought out, or the other is carried in until the lock is adjusted. This is commonly facilitated by pressing both handles backwards against the coccyx. This movement, by throwing the blades well into the ilia, where there is room, allows the handles to be rotated a little, so as to fall into accurate relation.

Accurate locking is generally evidence that the blades are properly adjusted to the head, and that the pelvis admits of the successful use of the instrument. On the other hand their not locking is proof of their not being properly introduced, or *of the pelvis not admitting of their application.* In the first case, that of improper introduction, the failure is generally due to neglect in passing the blades exactly in the same diameter of the pelvis—that is, in passing the second blade exactly opposite to the first, so that if the first blade is applied to the left ilium, opposite one end of the transverse diameter, the right does not lie at the opposite end of that diameter. To remedy this error, the blade must be partly or wholly withdrawn and readjusted.

In the second case, that of pelvic unfitness, the locking is prevented by the projecting promontory, or other deformity, so distorting the pelvic diameters that the two blades cannot find room to lie in the same diameter opposite to each other. It will commonly be found that the blades will pass one on each side of the promontory, the inside of the blade not looking towards its fellow, but towards the opposite foramen ovale, where you cannot get a blade to lie. When you find this happen you must give up the attempt to use the forceps.

Pass the hand into the pelvis, if necessary; explore its dimensions and form carefully; and determine between turning and craniotomy. A correlative proposition may here be stated:—*Wherever the long forceps will lock without force, it may be reasonably concluded that the case is a fit one for the trial of this instrument; and a reasonable attempt should be made to deliver by its aid before passing on to turning or perforation.*

3. *The Extraction.*—Get the nurse to press upon the right hip and support the back. Grasp the handles with one hand, and apply the fingers of the other hand to the ring or shoulders at the lock. Draw it first backwards in the axis of the brim, during the pains, if any be present, and at intervals of a minute or so if there be none. Concurrently with traction, alternate slight leverage movements may be executed by swaying the handles gently from side to side, always taking care not to press the shanks against the pelvic walls. Each blade is the fulcrum to its fellow. The finger which is used in the ring from time to time gauges the advance of the head.

The advance of the head is measured by the following standards:—First, you feel if the occiput approaches the pubic arch by passing a finger below and behind the pubic bones. Secondly, you sweep your finger round the circumference of the brim, and thus feel if the equator of the head-globe is pressing lower down through the brim. Thirdly, by feeling the direction of the sagittal suture. If you find that it is approaching parallelism with the conjugate diameter, you may be certain that the head is descending. Further evidence is found in the rotation of the forceps. As the head can hardly turn upon its cervico-vertical axis without at the same time descending in the pelvis, if the handles of the forceps are observed to rotate, this rotation, being imparted by the head, is evidence of advance. Again, as the head descends, of course more and more of the shanks and blades will become visible. This, indeed, is open to a fallacy. Allowance must be made for some degree of slipping, which takes place with all the English instruments whose blades have only a moderate bow. And further, when the head is fairly in the pelvic cavity, the blades lose something of that external support which is the chief force in maintaining the grasp upon the head. This is still more marked when the head has partly emerged from the vulva. At this time the blades will be apt to slip away altogether, and it will be necessary to increase the compression on the handles in order to keep your hold. Fourthly, by two or more fingers you measure the space or degree of tightness between the vertex and the floor of the pelvis. At first the fingers find free space; gradually the vertex leaves no room for the fingers. Then the soft floor of the pelvis, the perineum, is distended by the advancing vertex; it bulges out; it puts the perineum tightly on the stretch. The anus is protruded. Faces are often squeezed out. Indeed, the pressure upon the sphincter ani at this stage sets up reflex action. The call to strain or bear down to expel the pelvic contents, whether uterine or rectal, is uncontrollable. Turbulent expulsive action, then, and defecation, constitute certain signs that the head is advancing. To some extent the increasing scalp swelling or caput succedaneum may give a false impression that the cranium itself is descending. But a little practice and attention will correct this error. When the vertex has reached the floor of the pelvis, the handles of the forceps are found to have turned a little upon their axis, to lie more nearly in the transverse diameter of the pelvis. This is the result and the indication of the screw-rotation of the head. You have no hand in producing it. It is effected by the descending head adapting itself to the cavity of the pelvis. The handles may now be directed more forwards during traction. The shanks thus avoid stretching the perineum, and the traction is in the axis of the outlet. An assistant is now useful in holding up the right knee, so as to leave room for the operator to carry the handles well round the pubes in Carus' curve. Here it is often convenient to push the handles forwards rather than to pull.

During extraction it occasionally happens that the blades will lose their hold, that the handles will twist in opposite directions, and thus unlock. This is generally owing to the operator carrying the handles forward too *early*. The effect of this is to throw the blades off the head-globe over th e

face. It is another illustration of the law that the position of the forceps is determined by the relation of the head to the pelvis, and that if you reverse the order by attempting to make the forceps alter this relation, you are immediately at fault. The remedy is to carry each handle well back again towards the perineum, when they will re-lock.

If the head is in the genital fissure, and there is sufficient uterine energy, you may proceed to the

4th Act. The Removal of the Blades.—If the head should not be propelled, you may often assist it by a manœuvre which it is well to understand. You apply the palms of both hands, one on either side and behind to the perineum distended by the head; and bearing upon this structure so as to press it a little backwards, while the head is pushed forwards towards the pubic arch, the head is, as it were, shelled out by being made to complete its movement of extension. Steady pressure by the hands of an assistant, or by a binder upon the fundus uteri, will much assist the extension of the head. In this manner I once extricated myself and my patient from an awkward predicament. I had been summoned into the country without knowing the nature of the case, and had no instruments. I found a lady who had been many hours in labour, the head on the perineum, and no pains. The lever or the forceps would have delivered her in a minute. Neither was to be had. But the manœuvre I have described perfectly succeeded, and put an end to a state of extreme anxiety, and even danger.

Another manœuvre is occasionally serviceable. This is to pass a finger into the rectum, so as to get a point of pressure upon the forehead. In this way it is sometimes possible to bring the face downwards, to start the extension movement, and thus to extricate the head delayed at the outlet. And if at the same time firm downward pressure be made upon the breech through the fundus, the force propagated through the spine will aid materially in giving the extension movement. This combination of the principles of "pushing," of leverage, and of "shelling-out," may in certain cases enable you to deliver without resorting to the forceps or lever.

When the blades are adjusted, they will not lie exactly in the transverse diameter of the pelvis. The head, lying between the transverse and right oblique diameter, will tend to throw off the blades towards the opposite or left oblique diameter. The head then will be seized obliquely, one blade grasping the right brow, the other the left occiput. This is clearly demonstrated by the impressions of the fenestræ left on the scalp. The blades naturally find their way into this position if they are introduced gently. One tendency of this oblique seizure is to assist the head in its axial rotation, face sacrumwards, as it descends into the pelvis. It is also an answer to an objection urged against the use of the long forceps at the brim—namely, that by seizing the head in its long or fronto-occipital diameter, compression in this direction makes the opposite or bi-parietal diameter bulge out, thus increasing the difficulty of passing the small or conjugate diameter of the pelvis. In most cases the objection is theoretical only—it is mainly based upon experiments made on the dead fœtus on the table.

Elongation or moulding, we have seen, is the result of gradual compression of the equatorial zone. Now the pelvis and the forceps together constitute the compressing ring. Pressure, then, upon the transverse diameter of the head by the opposing points of the sacrum and pubes, simultaneously with pressure upon the longitudinal diameter, between the blades of the forceps, tends to *diminish both diameters* by lengthening out the head. Of course it must be understood that the pelvic contraction is of moderate degree only—in short, that the case is a proper one for the forceps. If the conjugate diameter be less than 3·25 inches, the prospect of effecting the desired elongation within a reasonable time is greatly diminished.

I have said that the head is very rarely seized exactly in its longitudinal diameter. An exception occurs in the case of the very flat pelvis, in which there is conjugate contraction with very little projection of the promontory. In this case the head will lie very nearly in the transverse diameter. If, in

presumed contraction of the brim, the marks of the blades are on the brow and side of the occiput, the projection of the promontory is not great.

The Time required for Extraction.—If the head be delayed in the cavity of the pelvis for want of expulsive action, or because it rests upon the ischia, maintaining a too near approach to the transverse diameter, and there is no marked hindrance on the part of either the anterior or posterior valve, it is generally sufficient to use slight traction and oscillation for a few minutes. As soon as the head is started by the forceps, the uterus takes up its work, helps the operator, and the labour is quickly over.

If the uterine and perineal valves obstruct the passage of the head, a little more time and caution are required.

If the head has to be seized at the brim on account of delay from want of uterine action, time may often be saved by placing the patient on her back, and supporting the uterus against the spine by the hands of an assistant or a binder. This proceeding, by adjusting the axis of the uterus to that of the brim, will greatly facilitate the entry of the head, and encourage the action of the uterus. If there is no obstacle from narrowing of the pelvis or want of dilatation of the soft parts, gentle traction and oscillation during ten minutes will generally complete the labour.

In the event, however, of arrest from pelvic contraction or from want of dilatability of the soft parts, time is a necessary element. The process of moulding, of elongation of the head, can only be effected gradually. Here oscillation or leverage must be used with great care. What is wanted is steady compression and traction extended, with moderate intervals of rest, over thirty minutes, or even an hour. Should the head be found to make no advance in entering the brim at that time, the question whether the forceps must not be laid aside for turning or perforation will have to be considered. —*Medical Times and Gazette, July* 27, *Sept.* 7 *and* 21, 1867, *pp.* 85, 249, 313.

99.—ON DR. BEATTY'S MIDWIFERY FORCEPS.

By Dr. Thomas Edward Beatty, Dublin, and Dr. Robert Barnes, London.

[Dr. Beatty considers that Dr. Barnes has fallen into a mistake in describing this instrument, in his lecture published in the Medical Times and Gazette for Aug. 24.]

In speaking of the short forceps, Dr. Barnes remarks, "The best short forceps is perhaps that of Dr. Beatty, of Dublin. It much resembles the short forceps of Smellie. I used it for some time, but I have given it up because of these two faults, and its inadequacy to cope with a large range of cases which come within the power of the long forceps." From this description I can only conclude that the instrument spoken of is not mine at all, for it does not resemble that designed and published by me twenty-five years ago, and reprinted in my recently published volume, "Contributions to Medicine and Midwifery."

My forceps are not short, but were designed as intermediate between the long clumsy French forceps and the old short ones of Smellie. A pair of the latter in my possession, and which I used before I contrived the longer instrument, measure ten and a half inches in the entire length, of which the blades measure six inches. My forceps, spoken of by Dr. Barnes as resembling that of Smellie, is thus described at page 116 of the volume of "Contributions" just alluded to :—"The entire length, including the handle, is twelve and a half inches; of the blade to the lock, eight inches; of the fenestrum, five and a quarter inches; the greatest breadth of blade, one inch and three-eighths; the widest part of the fenestrum, one inch; the distance between the blades when joined, three inches at widest part; distance between the extreme points, one inch and one-eighth; weight of the whole, ten and a quarter ounces. The sides of the blades enclosing the fenestra are nearly round, slightly flattened upon the inner and outer surfaces, but having *no sharp edge either* upon the outer border or the margin of the fenestra."

From this it will appear that my forceps are well beyond the limit to which the term short can be applied, and that the resemblance to the instrument of Smellie is very remote. But on looking at the description of the instrument recommended by Dr. Barnes, and comparing the measurements of it with those of my own, quoted above, I find that there is a much greater resemblance between them than between mine and Smellie's. In fact, in the blade, the most important part of the instrument, mine is an inch longer than that adopted by Dr. Barnes. In the latter he describes the length of the blade to be seven inches—in mine it is eight. His handle is five inches, while mine is four and a half. In Dr. Barnes's forceps the length is eked out by what is called a shank intervening between the point where the blades begin to separate and the lock, the length of which shank is not mentioned.

I trust it will not be considered that I am encroaching too much on the valuable space of this journal if I make a few observations on Dr. Barnes's two objections to straight or single-curved forceps, which mine are.

Objection 1.—"To introduce the second or upper blade, the handle must be depressed at nearly right angles with the mother's left thigh, which is flexed upon her abdomen. Now, to do this, the patient's nates must be dragged over the edge of the bed; to procure and maintain this position is often a matter of great difficulty and inconvenience."

Objection 2.—"In extraction, the handles nearly to the last moment must be directed more backwards than is necessary with the double-curved forceps, and owing to the bows springing directly from the lock, the perineum is wedged open, and not seldom unavoidably torn. In some cases this injury may be avoided by taking off the blades before the greatest diameter of the head passes, but then the work is not always done, and you have to put them on again."

Now, as to the position of the woman to enable us to pass the second blade of the straight forceps, it differs in nothing from that required to pass the first, or a double-curved blade, if such be used. It is the well-known obstetrical position in which we place our patients in ordinary labour in the last stage. It is that in which she must lie for catheterism, in which the operation of turning is performed, and which she must observe when craniotomy is necessary. And when it is recollected that delivery by the forceps is usually performed under the influence of chloroform, when we can place and maintain our patient in any position we like, this objection ceases to have any force. But in the application of my forceps it is of no moment, for the depression of the handle nearly at right angles with the mother's thigh is not only unnecessary, but wrong.

I think the best comment I can make on this, and the second objection just quoted, is to ask Dr. Barnes to be kind enough to open my volume of "Contributions" at page 118, where, in reference to this very subject, I make the following observations:—

"From having at times seen awkward attempts made to introduce the blades, I am induced to add a few remarks upon the method of using the instrument. The point should never be urged forward in the direction of the long axis of the blade. Such a proceeding is calculated to inflict injury on the mother, if the blade is sharp on the outer edge, as it is in most forceps found in cutlers' shops. On the contrary, the operation should be commenced by laying the concave surface of the blade flat upon the lowest part of the child's head, and when so placed *the handle will project between the thighs of the patient;* then bringing the handle slowly down, and, by a series of lateral-sweeping movements, during which the point is at the same time slowly passed over the side of the head, describing on it a curved line, the blade is coaxed, as it were, into its proper place. No force should be ever used; if any resistance is encountered, it is an indication that some error has been committed. Better to stop, and even withdraw the blade and begin afresh, than use violence to urge it forward. When one blade is thus placed, the handle will be found to lie close to the fourchette; it should be given to an assistant to hold steady, and then the same proceeding should be adopted to ensure the safe introduction of the other. Having got the head fairly em-

braced by the instrument, the extracting force should be applied in the direction in which the handles point. If the head be high up in the pelvis, they will be found hard pressed against the fourchette, and pointing backwards. In that direction, then, the handles must be drawn in the first instance, and, as the head is moved, the handles come more directly downwards; in which case the direction of the force is to be changed, still following the rule that it must be used in the direction of the handles. By degrees, as the head comes to rest upon and distend the perineum, the handles will point forwards, and finally they will be found between the thighs of the patient, in which position the last part of the delivery is accomplished. This quality of pointing out by necessity the axis of that part of the pelvis in which the head is lodged, and thus enabling us without fail to use our extracting force in the most suitable direction, gives a great superiority, in my mind, to the straight blades over those with the lateral curve, which are so much used (in England and Scotland). The latter, from their curved form, give an erroneous idea of the position of the head, and may cause the expenditure of much unavailing force in a wrong direction."

I have only to add that the narrowness of my blades, they being only one inch and three-eighths broad, together with the elongated curve which they describe, render their introduction and application, in the most difficult cases, a matter of very easy performance.

I am sure Dr. Barnes will excuse me if I make use of one of his own diagrams to substantiate the opinions and precepts set forth in the passage just quoted. Figure 13, in his third lecture, p. 196, is what I would have drawn if I wanted to demonstrate the superiority of my straight forceps over that with the double curve. The head is represented in that diagram in a position that very often demands delivery by the forceps. The greatest portion of it has passed through the brim of the pelvis, but the lower surface has not come to touch the perineum, and the hollow of the sacrum is still unfilled up. It will be manifest to any one looking at that diagram that the short forceps is well applied to the head, and that the first direction in which that head must move is backwards and downwards until it comes to occupy the hollow of the sacrum. The short forceps are placed precisely in that direction, and extracting force exerted in the direction pointed out by the handles is what the case requires in the first instance. If traction be thus employed, it is manifest that the bulk of the head will be drawn into the concavity of the sacrum, the chin will be removed from the breast of the child, but as this takes place the handles of the forceps will gradually come forward, and as the head descends will follow the curved dotted line on the diagram until, the head having rotated on an imaginary axis running through the parietal protuberances, the face comes to occupy the hollow of the sacrum, and the occiput protrudes beneath the pubes. Now, if we examine the position of the long curved forceps, as represented in the diagram, it will be manifest that traction cannot be made in the proper direction—viz., backwards and downwards. If force be used in the direction of the handles, the effect must be to draw the occiput down on the symphysis pubis, which will of course resist its further descent, giving rise, as I have already stated, to "the expenditure of much unavailing force in a wrong direction." If my straight forceps had been placed on that head instead of the short ones that are there delineated, the contrast would have been still more strongly marked.

The last objection that Dr. Barnes makes to my forceps is "its inadequacy to cope with a large range of cases which come within the power of the long forceps." I have already stated my belief that Dr. Barnes never had a true copy of my forceps, and this observation is quite conclusive upon the matter, for the ease with which heads can be seized when they have scarcely entered the brim of the pelvis, and the facility given to their extraction by the shape of the blades, are points dwelt upon by all who used the genuine instrument. Any one who will take the trouble to look into my volume above alluded to will find abundant evidence of the power of my forceps to cope with every case. I will content myself with the following extract:—

"As an illustration of what can be done with the forceps above described, I will quote one case from my case-book (page 120).

"September 16, 1859.—A lady, tall, thin, and delicate, eight months pregnant for the third time, living three miles from Dublin, had taken a house in town by my desire for her confinement, and was in the act of getting into her carriage to come to town when she was seized with a convulsion. This was at about 12 o'clock in the day. I was sent for, and reached her as soon as possible. Her former labours had been natural. I learned that for a week previous to the present attack she had been complaining of headache; I found her after a second fit quite collected and free from pain in the head. I examined and found no sign of labour. The convulsions returned every hour, increasing in severity, but with intervals of complete consciousness. At 9 o'clock p. m. there was some slight labour pain, but still no dilatation of the os uteri. At 11 o'clock the os was open to the size of a shilling, and the convulsions still kept recurring every hour. At 12 o'clock she had another bad fit, when I found the os uteri about as large as a two-shilling piece and soft. I tried dilatation with my finger, and succeeded in enlarging the opening to such an extent that, if it had been desirable to turn the child, one would have been able to force the hand into the uterus.

"The head of the child lay well down on the cervix uteri. A moment's reflection convinced me that if I could get my hand into the uterus, I could as well introduce the blades of my forceps, and accordingly, having ruptured the membranes, I proceeded to pass the instrument, first one blade, then the other, along the sides of the head, and I readily closed the handles. Then, by slow traction, I drew the head through the os uteri and into the pelvis, and delivered a live boy, who is now seven years old, strong and healthy. I think an instrument capable of accomplishing a feat such as this does not require any further commendation."

In conclusion, I will observe that the old short forceps is totally abandoned in Dublin, and that for many years no forceps has been used in the Lying-in Hospital of this city but my straight forceps. I have used no other myself for twenty-five years, and I believe the instrument is very generally employed by other practitioners. We do not meet the torn perineums that Dr. Barnes fears as the result of its use, and those who use it the oftenest are disposed to prize it the most.

[Dr. Barnes replies to Dr. Beatty as follows:—]

I have read with great pleasure, and I trust with profit, the criticism of Dr. Beatty upon my objections to his forceps. That criticism, at once judicious and generous, as might be expected from a man so eminent for experience and kindly courtsey, impels me to offer some further observations upon the subject.

1. I have said that "the best short forceps is, *perhaps*, that of Dr. Beatty, of Dublin. It *much* resembles the short forceps of Smellie." I would, at the outset, make two corrections—by striking out the words "*perhaps*" and "*much*."

2. The term "short," to which Dr. Beatty objects, also requires explanation. I use it as an alternative with "single-curved," as distinguishing this class of instruments from the "double-curved," or "long" class. It would be more accurate to discard the terms "long" and "short," for a single-curved forceps may be "long," as is the case with Dr. Beatty's, and a double-curved one may be "short," as are some that I have seen.

3. As to the resemblance of Dr. Beatty's forceps to Smellie's short forceps. Here further explanation is necessary on my part. Dr. Beatty's instrument is considerably longer than Smellie's. The resemblance consists in the general form of the blades and fenestræ, and in a great measure *in the use to which both can be applied*. This use is essentially that to which the single-curved forceps is nearly entirely, and, in my opinion, properly restricted—namely, the application to deliver a head wholly or partly engaged in the pelvis. I have, of course, discharged my duty and gratified my desire for enjoyment and profit by studying Dr. Beatty's "Contribution to Medicine and Midwifery." I find in his excellent and most useful memoir on the forceps these

words:—"In the observations that follow . . . I will allude only to the more common cases of difficult labour, in which, after several hours of severe pain, *the head has been forced to a greater or less extent into the cavity of the pelvis*, and is there arrested." (p. 97.) And in the histories of the fifteen cases narrated, it is stated in every instance that the head was partly or wholly in the pelvis. At p. 116, it is said that, "from the length of the blades, the lock will not be within the vagina." This, to my mind, is further evidence that Dr. Beatty does not insist upon the fitness of his instrument to seize and deliver a head arrested *above the pelvic brim*. I have tried his instrument in such cases, and the lock had to go fairly within the vagina. Being longer, however, Dr. Beatty's forceps will command a greater number of cases than Smellie's, and yet it leaves many cases behind which a perfect forceps ought to command.

4. I will now say a few words on Dr. Beatty's reply to my "Objections." First, as to the necessity, alleged by me, that the patient's nates must be dragged over the edge of the bed in order to allow the handle of the upper blade to be sufficiently depressed to enable the point to travel over the head-globe. I contend that in certain cases—I have met with them—as of convulsions, hemorrhage, great restlessness of the patient, faulty structure of the bed, and want of assistants, it is difficult, highly inconvenient, or even impossible, to get or to maintain the patient on her left side, with the nates well over the edge of the bed. I have had to deliver by forceps a woman in convulsions on the floor. In such cases the advantage of possessing a forceps like my "double-curved," and of using it as I describe, is very great. It can be applied whether the patient be on her back, or on her side in the middle of the bed. Dr. Beatty seems to claim a similar quality for his forceps, when he says the position on the side over the edge of the bed "is of no moment, for the depression of the handle nearly at right angles with the mother's thigh is not only unnecessary, but wrong." He then describes how to introduce his forceps. "The operation should be commenced by laying the concave surface of the blade flat upon the lowest part of the child's head, and when so placed *the handle will project between the thighs of the patient;* then bringing the handle slowly down. . . . When one blade is thus placed, the handle will be found to be close to the fourchette; it should be given to an assistant to hold steady, and then the same proceeding should be adopted to insure the safe introduction of the *other*." This is a description of the mode of introducing the pubic or upper blade, which, I believe, it is the Dublin practice to introduce first. It is the pubic blade which makes the difficulty. Now, I freely admit that in the first stage you may pass the blade into the vagina to touch the child's head in the way described, so that the handle shall be between the mother's thighs. So far you may, indeed, pass the blade in this or in any way that may be most convenient. But in the next stage, that in which the point of the blade has to travel over the head-globe, it appears to me axiomatically necessary that the handle must be depressed just in proportion to the elevation the point must take to rise over the head. Dr. Beatty himself directs "the handle to be brought slowly down." He does not indeed say how far down, but this is indicated by the conditions of the case. The point must, to get over the equator of the head, run nearly vertically upwards: the handle, therefore, must be turned nearly vertically downwards —that is, across the mother's right thigh. And the length of the forceps, "twelve and a half inches," must carry the end of the handle into the bed-cloths if the nates are not drawn clear over the edge. Is not this matter of common experience? I confess I am unable to understand how to avoid this dilemma.

5. Dr. Beatty "concludes that the instrument spoken of by me is not his at all." This forces upon me the welcome suspicion—which I am sure Dr. Beatty never dreamed of suggesting—that he has provided a good instrument, but I have not known how to use it. Accepting this alternative, I will humbly follow Dr. Beatty's instructions, and try again. I must, however, plead that I have used this instrument in at least twenty-five specified cases. *In most of these* cases I was greatly satisfied with its efficiency. It is admi-

rably light, well-balanced, elegant, and very easy of introduction. Its length makes it a good lever, and I am glad to observe that Dr. Beatty recognises the principle of using the forceps as a lever. But I would also observe that owing to the shortness of the handles—the absence of shoulders or ring reducing it to a one-handed instrument—the slimness of the shanks and blades, so great that these easily "spring" or bend under compression, and the length of the radius of the bow, the grasping, compressing, and traction powers are not great. This Dr. Beatty and the adherents of the single-curved forceps will probably regard as a merit, and so it is for a forceps whose duty is limited to delivering a "head arrested in the pelvis." Moderate leverage power is often all that is wanted in such cases. But the instrument fails before those more difficult cases of arrest of the head *on the brim*, whether inertia or slight pubic contraction, I say this from ample experience, having in several such cases first tried Dr. Beatty's instrument without success, and then succeeded readily with the long double-curved forceps. The case narrated in detail by Dr. Beatty was an eight months' child. I admit that Dr. Beatty's forceps will often prove sufficient in cases of arrest of the head only partially engaged in the brim; and even in some cases where the head is quite above the brim; but then the lock runs into the vagina, power is lost, the perineum —I insist upon this—is put unnecessarily on the stretch, for in proportion as the point is directed forwards to get above the brim, the handles must be forced back against the perineum and coccyx. These inconveniences and others are quite obviated by my double-curved forceps.

6. The shank intervening between the lock at the commencement of the bow of the blades, adding two inches and three quarters to the length of the handles—of which Dr. Beatty speaks a little slightingly—has these great uses: it carries the lock clear out of the vagina, even when the head is grasped above the brim; it increases leverage and traction powers; in conjunction with the pelvic curve of the blades, it obviates all strain upon the perineum; and by adding to the length of the handles, and supplying a ring for holding, it makes a *two-handed* instrument, to the great increase of power, delicacy, and comfort in use. I will only further remark upon Dr. Beatty's plea that in Dublin "we do not meet the torn perineums that Dr. Barnes fears as the result of its use" (Dr. Beatty's forceps) by citing a statement lately made in my presence by Dr. Denham at the Rotundo Hospital—namely, that they there had to perform the operation for lacerated perineum very frequently. Of course, I do not imply that this necessity always arises from the use of Dr. Beatty's forceps. But the risk of tearing the perineum with *any* straight forceps, *the bow of whose blades springs direct from the lock*, even if the handles are carried well forwards, must be serious. The blades will form a double inclined plane or wedge beyond the head. No ingenuity short of taking off the instrument will always avail in preventing this wedge from splitting up the perineum, especially of a primipara.

In conclusion, I feel bound to say that, thirteen years ago, when on a visit to Dublin, I was so struck with the elegance of Dr. Beatty's forceps, that I adopted it in practice, the particular specimen I used having been recommended to me by Dr. McClintock. It would have given me sincere pleasure to find such an instrument do the work—I mean *all the work*—that came before me; but I soon discovered that it was not equal to the exigencies of my practice. I wanted an instrument that would more largely supplant the perforator, that would save more children, and rescue the mother more quickly from the dangers of exhaustion. I therefore fell back upon my "two-handed double-curve."—*Medical Times and Gazette, Sept.* 21 *and* 28, 1867, *pp.* 314, 342.

100.—ON DR. BEATTY'S FORCEPS.

By Dr. Henry E. Eastlake, Welbec-street, London.

The object of the following is to show the vast superiority of Beatty's forceps over those which possess a double curve, in cases where the fœtal head is placed in the occipito-posterior position.]

The very fact that the natural mechanism of these cases during parturition is somewhat more complicated than those of the occipito-*anterior* variety, and that the expulsion of the head is, as a rule, more slowly effected, suggests the idea that instrumental aid may not unfrequently be required in this special class of cases to which I allude. At any any rate, it cannot be denied that an accoucheur is every now and then called upon to apply the forceps in cases where the child's face is directed anteriorly; and in order to advocate the special advantages of the straight forceps under these circumstances, I cannot do better than quote the expressions of Sir James Simpson upon this important point. I am led to do so from the fact that neither Dr. Beatty nor Dr. Barnes alludes to it as an advantage in the discussion which has appeared in the Medical Times and Gazette of the merits and demerits of their respective instruments. In the first volume of his "Obstetric Memoirs," Sir James writes: "*In occipito-posterior positions, the mechanism of the extraction of the head with the forceps should be an exact imitation of the mechanism of the expulsion of the head by nature.* In other words, I am strongly convinced that, in the artificial extraction of the head in occipito-posterior positions, we should make the forehead rotate backwards, and the occiput forwards, according to those rules which we have seen nature following under the same conditions. For here, as elsewhere, the more perfectly we imitate her principles, the more perfect will our practice be. . . . I have now happened to be called to several cases of occipito-posterior positions, in which the forceps had been applied with the greatest adroitness and dexterity; where, subsequently, every allowable degree of force had been employed, but employed in vain, to pull forth the head in its original position, with the forehead directed anteriorly; and where I have succeeded, with a tithe of the power previously used, by adding to the requisite act of extraction a simultaneous act of rotation of the head, so as to turn the occiput anteriorly and to the right, and the forehead posteriorly and to the left." The Professor goes on to state that so long ago as 1745 Smellie became aware of the importance of *rotating* as well as extracting the head, "in a case in which the large fontanelle was at the left groin." He then finishes his paper on this question with the following remark: "It is in occipito-posterior positions, above all others, that we see the superior advantage of employing a *straight* pair of short forceps, such as those of Dr. Denman or Dr. Ziegler. They enable us to rotate the head easily and safely, as I can testify to you from sufficient experience. If we employed a *curved* pair in this position, and tried to turn the head with them, we should be obliged either to introduce them at first, or extract them at last, with their concavity, instead of their convexity, looking backwards, and consequently with great and unnecessary risk of contusion and laceration of the soft structures of the mother from the projecting ends and sides of the blades."—*Medical Times and Gazette, Oct.* 12, 1867, *p.* 417.

101.—ON THE CEPHALOTRIBE.

By Dr. J. Braxton Hicks, F.R.S., Examiner in Midwifery at the University of London.

[From a consideration of the cases in which he has used the instrument Dr. Hicks regards it rather as a substitute in the severe forms of obstruction, for the other craniotomy instruments after perforation, and not so much a substitute for Cæsarean section.]

It is, no doubt, very valuable both in doing away with the danger arising from spicula of bone, and from the crotchet, &c., as well as tending to shorten the duration of the operation; but, if we rely upon it as an instrument capable of accomplishing the whole delivery by itself, we shall be overrating it, and exposing ourselves to disappointment. For it is not a tractor; and, therefore, should pains be absent, we shall generally have to draw down the head by some other means. When it is regarded as a substitute for *the Cæsarean section*, it seems that the advantage of removing the calvarium,

and inducing face-presentation, as recommended by me in a paper in the *Transactions of the Obstetrical Society of London*, Vol. vi., has not been fully recognised; because I believe that there are very few cases of contracted pelvis in which we cannot as certainly deliver by this means as by the help of crushing, though perhaps not so readily. Hence the comparison rests rather between these two plans, than between the cephalotribe and Cæsarean section; for the latter would be reserved for a more complete obstruction.

I am here, of course, discussing the subject on the assumption that we are acting under necessity (for instance, where the child is dead). Where we are employing the Cæsarean operation as one of election, other considerations enter. I think that, by means of this instrument, or the plan to which I have alluded, we can employ cephalotomy to its extreme point; so that, if the outlet be good, we can deliver in a brim with an anterior-posterior diameter of 1¼ to 1¾ inches without serious risk, provided the transverse be above 3¼ inches, and the fœtus of medium size.—*British Medical Journal*, *Oct.* 19, 1867, *p.* 337.

102.—ON THE CEPHALOTRIBE.

By Sir James Y. Simpson, Bart., Professor of Midwifery in the University of Edinburgh.

Under certain circumstances in practice, obstetricians were often obliged to reduce the size of the child's head, and for that purpose they had recourse to a variety of measures, craniotomy being one. At present that was not a very favourable operation in hospital practice. He (Sir James Simpson), having looked at Dr. Churchill's book, found that there were 310 cases of craniotomy performed in the Dublin Hospital. Out of these 65 died, or more than one-fifth. The operation was attended with great danger for various reasons; and especially for this reason, that when we perforate the child's head and take away a piece of bone, we are extremely apt to injure the vagina; and, again, by the operation, the size of the child's head was often increased. Dr. Hamilton, many years ago, was in attendance on a young woman whose pelvis was very deformed, and, in order to deliver her, he had to remove the whole arch of the cranium; and he states that after having done it, which took him eight hours, he had to be rolled up in blankets and carried home himself; so what must have been the state of the patient when the doctor was so bad? Notwithstanding all that, they had the base of the skull quite entire; and in putting on the cephalotribe, it broke the head at once. In reference to the size of the pelvis, M. Pajot stated that he delivered a woman with a pelvis of an inch and a-half. That, in Sir James Simpson's opinion was too small a pelvis to be delivered at all, and it was one where the woman ought to submit at once to the Cæsarean operation. He himself had attended a case where a very deformed woman was attempted to be delivered by craniotomy. He used that instrument (exhibiting a cephalotribe), and the delivery was over in two or three minutes. The instrument used was quite curved, and the advantage of it was, that it would not slip when a straight pair would. One of the great difficulties is this, that after fixing the head it slips backwards. The instrument needs to be pushed very far forward, and with a long straight forceps it is almost impossible to hold the head. The instrument exhibited was a very powerful one and very heavy. The cephalotribe, with teeth for fixing the scalp of the child, he regarded as a great improvement; but it is a little more complicated in its arrangement than others. It was an instrument capable of crushing any head, and rendered the operation of craniotomy easy and safe for the patient.—*British Medical Journal*, *Oct.* 19, 1867, *p.* 337,

103.—OBSERVATIONS ON THE CONSTRUCTION OF THE CEPHALOTRIBE.

By Dr. George H. Kidd, Assistant Physician to the Coombe Lying-in Hospital, Dublin.

In the Dublin Quarterly Journal of Medical Science for February and May of the present year, I have published cases in which the operation was per-

formed, and where there was more or less marked narrowing of the antero-posterior diameter of the pelvis. In one instance there was but one inch and three-quarters in this direction, and yet the mother was delivered safely and made a good recovery, as did the mothers in all the others, except one, where she was in a state of such extreme exhaustion when I first saw her, that I feared she would sink under any operation, and though, owing to the ease with which the head was extracted by the cephalotribe, her remaining powers were not overtaxed, she never rallied, and died in eighteen hours.

In speaking of these cases, and subsequent experience has confirmed me in the opinion, I have stated that I was convinced that the cephalotribe should be used in all cases of embryotomy in preference to all crotchets, hooks, and craniotomy-forceps, and that the use of it would reduce the dangers of embryotomy to a minimum, and allow its performance in cases where delivery could not otherwise be accomplished except by Cæsarean section, and that even though the child were known to be dead. My earliest operations were performed with Sir James Simpson's first modification of the instrument, but in the use of it I saw, from time to time, that some further changes in it were desirable, and it is my object now to show an instrument that I have found to answer all the purposes I have hitherto required. The action required of a cephalotribe is twofold :—

1st. As a compressor, to compress the head, and especially the base of the skull, so as to allow of its passing through a narrowed pelvis.

2nd. As an extractor, to hold the part to which it is applied, so as to allow of its being rotated in the smallest possible space, and of the most diminished portion of the presenting part being brought into the narrowest diameter of the pelvis. Moreover, its hold must be so secure as to allow of the use of a sufficient amount of extractive force to draw the child through the pelvis.

Authors differ as to the relative importance of the action of the instrument as a compressor and as an extractor. Hennig, of Leipzig, has added crotchet points capable of being projected when the instrument is applied, to increase its power as an extractor; and Depaul aims at the same object, while M. Pajot and Dr. Hodges look on its use as a compressor as the most important, and, in extreme cases, Pajot is satisfied with breaking up the head by repeated applications of the instrument, and allowing it to be expelled by the uterine contractions. I believe myself that in a very narrow pelvis the cephalotribe should be applied and re-applied so as to break up the head sufficiently to enable it to pass through with ease and safety, and that then the labour may be shortened by using a certain amount of extractive force. But, whether used as a compressor, extractor, or both, the form of the instrument should be such as to admit of its easy introduction, application, and locking, with the greatest possible facility and safety, and in the narrowest pelvis in which it could be used.

In the instrument I have had made, I have endeavoured to meet all these requirements. Instead of a blade with a pelvic curve, I have adopted a straight blade. I believe this has four great advantages. First, it permits of the head being rotated in a smaller space than with a curved blade. It seems obvious that a head seized in a curved instrument must, when rotated, either turn on the axis of the handle of the instrument or on its own axis. If it turn on the axis of the instrument it will pass through a segment of a circle whose diameter will be double the length of the curve. If it turn on its own axis then the handle of the instrument must sweep through the circle. Now, as the instrument is chiefly required in cases of narrowing of the antero-posterior diameter of the pelvis, and is applied in the transverse or oblique diameter, and it is desirable that when the head is crushed in this direction it shall be turned so as to bring its crushed and shortened portion into the antero-posterior diameter of the pelvis, it is of the utmost importance that it should be capable of effecting this rotation in the smallest possible space, and this, it is evident, can only be done with a straight blade.

Secondly, it is easier to guide a straight instrument when introducing it than a curved one. In introducing a straight instrument we have but one *element to consider* as to its progress, whereas, with a curved blade, we have

to follow with the mind's eye, not only the onward course of the point, but the peculiar direction the blade may take in consequence of its curve, and to guard against injury to the parts met with in this course; and, as it has often to be passed up far above the brim of the pelvis, the importance of this cannot be over-estimated. The pelvic curve in Baudelocque's original instrument measured three-inches and three-quarters; to facilitate the turning of the head, Braun, of Vienna, reduced it two inches. I advocate doing away with the curve altogether.

Thirdly, it holds the head more securely. When the blades are applied, and we begin to make compression, if they rest near the posterior part of the head, it is liable to slip from between them, and so elude the grasp of the instrument. In one of my operations, in which I used Simpson's instrument, this occurred repeatedly, but was obviated by fixing the head by making pressure over the pubes, which was done by my colleague, Dr. Ringland. Cazeau and Busch tried to prevent this slipping of the head by increasing the pelvic curve, but a little consideration will show that this was a mistake, for, as the head, in the great majority of cases, lies with the face to one or other sacro-iliac synchondrosis, and the occiput directed forwards, and as the point of the curved blade is always directed forwards, the curve actually increases the risk of slipping. Schölles attributed the difficulty, more correctly, to the shortness of the blades, and consequently lengthened them, and for this and other reasons I have followed his example; but while I believe the only effectual way of preventing it is to fix the head by a hand placed above the pubes, I have no doubt it is much less likely to occur with a long straight blade than with a short curved one.

Fourthly, in making extraction, the handle of the straight instrument indicates the direction in which the traction should be applied. With an instrument having a pelvic curve, a calculation has to be made at each stage of the operation as to the direction in which the force should be applied. With a straight blade, the handle changes its position as the head advances, and the force is always to be applied in the line of the instrument, as has been ably shown by Dr. Beatty, in reference to the forceps, in his *Contributions to Medicine and Midwifery*.

The length of the blade in my instrument is ten inches, nearly equal to Baudelocque's and Scanzoni's, and greater than in most others. By this increased length the base of the skull is more surely reached, and therefore more surely broken up. The hold of the instrument is more secure, and the head less likely to slip from between the blades, and when the instrument is introduced the lock is external to the vulva, and nipping of the labia is therefore avoided. In considering the length of the blades it is to be remembered that in the cases of extreme narrowing the head is always quite above the brim of the pelvis, and the depth of the pelvis itself is often much increased. When the blades are closed, their extreme width is one inch and a half, and the cephalic curve is formed by a curve commencing at an inch and a-half from the points. The space between the blades at the widest part when closed is one inch. The gradual cephalic curve, and the closeness of the blades, will allow of the instruments being applied in a narrower space than most others. It is true that they diminish the extracting powers of the instrument, but this I look upon as an advantage, as, for the safe use of the cephalotribe, the head should be crushed to such a degree by repeated applications that it may be extracted without force. The lock in my instrument is the reverse of that in general use. It is made so, that when the upper, or right, blade is introduced, the patient lying in the ordinary obstetric position, on her left side, the lock of that blade looks forwards. I have spoken at some length on this formation of the lock in my papers in the *Dublin Quarterly Journal of Medical Science*. I believe no change could be made in the cephalotribe, or in the midwifery forceps, that would tend more to facilitate their correct application and use than this. Formerly forceps were made in this way. I have a pair showing this, made, I believe, in 1790. In the museum of the Rotunda Hospital there is a similar pair. Dr. Radford, in his volume of Essays published in 1836, has devoted a section to this form of lock, but

still instrument makers have not adopted it. Messrs. Fannin, of this city, however, who made the cephalotribe I exhibit, now manufacture forceps with the same form of lock, and I am greatly mistaken if any operator who has once used either instrument will ever willingly operate on a case with the head high up in the pelvis with one having the lock differently placed. The handle of my instrument is half-an-inch shorter than Simpson's or Hicks', the shortest hitherto in use. This, added to the increased length of the blades, tends, no doubt, to diminish its compressing power; but I have ascertained, by repeated experiments, that there is still abundant power to compress the most firmly ossified head, and the instrument gains somewhat in lightness and portability by the change. The screw used is the same as Simpson's, which is not only most simple and easy of application, but forms a very convenient and firm cross-handle for manipulating the instrument.—*British Medical Journal, Oct.* 19, 1867, *p.* 335.

104.—THE NATURE AND TREATMENT OF PUERPERAL CONVULSIONS.

By Arthur B. Steele, Esq., Lecturer on Midwifery, Liverpool Royal Infirmary School of Medicine.

An interesting report of a fatal case of this formidable puerperal complication, published by Mr. Evans in the British Medical Journal of May 25th, induces me to offer some brief remarks upon a subject always interesting to those engaged in midwifery practice.

I wish to observe, *in limine*, that the case to which I allude appears to me, from the lucid history given by Mr. Evans, to have been of such a nature—namely, accompanied with intense toxæmia, from the presence of albuminuria; and that, in all probability, a fatal issue would have taken place, whatever treatment had been adopted. Therefore, the remarks which follow, will not, I trust, be regarded as a criticism upon the remedial measures employed in the particular case in question, but as suggested by a desire, if possible, to throw additional light upon some of the more perplexing differences of opinion which exist among accoucheurs upon this disease and its appropriate treatment.

First—As to general depletion by bloodletting—we have the highest authority for copious venesection as an essential part of the treatment of puerperal convulsions; and of its good effects under certain circumstances I can add my own testimony from observation. But then by others of equal repute—M. Trousseau, for example—we are forbidden to have recourse to the lancet in this affection. How can such opposite views of treatment be reconciled? The solution of the difficulty consists in a consideration of the pathology of the disease. Without going minutely into the subject, it may be sufficient to state the well-known fact, that puerperal convulsions may arise from two distinct and opposite conditions of the system—one in which excessive distension of the vessels of the brain or spinal cord, or both, produces eclampsia by compression; the other, in which, from excessive loss of blood from hemorrhage, natural or artificial, the nervous centres are drained of their blood, giving rise to convulsions from anæmia, as was first pointed out by Dr. Marshall Hall. Thus we see that bleeding in the one case is remedial, by relieving the oppressed nervous centres and restoring the balance of the circulation; while, in the other, it only increases the evil—"the therapeutics of the disease," as tersely put by Dr. Tyler Smith, "trenching distinctly upon its pathology." (Journal of Obstetrics, page 499.)

Such, in brief, is the principle by which we are to be guided in the matter of bleeding in this disease. My own experience induces me to believe that, notwithstanding the high authority of M. Trousseau, cases of puerperal convulsions are sometimes met with, in which early and copious venesection is an essential element in their successful treatment. I am unable to appreciate the principle upon which local bleeding can be useful, except, possibly, *to relieve the secondary* effects of continued pressure upon the brain from a

long continuance of the paroxysms, which would, I think, be more effectually *prevented* by the relief afforded in the earlier stage by reducing the quantity and force of the general circulation by venesection.

The foregoing apparently simple and conclusive statement of the question, however satisfactorily it may dispose of the difficulty in deciding when to bleed and when to stimulate, does not, unfortunately, apply to a wide range of cases, in which the state of the circulation in the nervous centres is not the main cause of the convulsions; in many of which it may be, as M. Trousseau states, the effect of eclampsia; and in others it may not exist to such an extent as to become a subject of material consideration in the treatment.

Besides plethora, and its opposite condition, anæmia, we have other centric causes of convulsions, which may exist either with or without the first two conditions. Such are the various forms of toxæmia depending upon those agencies which interfere with the proper depuration of the blood, as constipation, and insufficient secretion from the bowels caused by mechanical pressure of the gravid uterus upon the intestines; cholæmia from insufficient action of the liver, or uræmia from affection of the kidneys. In addition to all these, we have the eccentric causes of convulsions, such as emotion, irritation of the uterus and the uterine passages; and, again, irritation through the medium of more distant incident excitor nerves, as irritation of the bowels, especially the lower part of the intestinal canal, from the presence of indurated fæces, worms, or the severe action of purgative medicines, all of which have been known to produce convulsions. Gastric irritation, and even irritation of the mammæ, caused by excessive soreness of the nipple, with mammary induration, have been the cause of convulsions.

The indications for treatment in these varied conditions are too obvious to require comment; and, under such circumstances, either depletion or stimulation, if indicated at all, can only be regarded as subsidiary or secondary measures. Next to blood-letting, the most vexed question is that relating to the propriety of artificial delivery. In forming a conclusion upon this point, we must still be guided by the pathological conditions present, and their relations to the convulsions as causes. When there is reason to believe, from the absence of any other source of irritation, that the presence of the fœtus in the uterus is the exciting cause of eclampsia, delivery by the speediest method, and that least likely to add to the irritation already existing, is the manifest indication. The propriety of emptying the uterus must entirely depend upon the peculiar circumstances in each case. If the convulsions depend upon causes evidently independent of direct uterine irritation, delivery will, I believe, usually (and I speak from observation on this point) produce no impression whatever upon the course of the disease. I have frequently found the paroxysms to continue unabated for some hours after delivery has been completed.

With reference to the mode of effecting delivery, the rule must be to adopt the means least calculated to increase irritation to the uterus; and certainly I cannot but think that the forceps, on that account, should always be preferred whenever their application is practicable. The passing up of the small blades of the forceps through the vagina (and os uteri, when necessary) is far less calculated to produce irritation of the incident excitor nerves of the parturient canal than the introduction of the hand and arm in the operation of turning, or the more complicated manual operations involved in craniotomy. Wherever the forceps can be applied, their use need not be, in skilful hands, more dangerous, and, in the circumstances under consideration, I believe they would be as safe, or safer, than any other method of artificial delivery.

In conclusion, I would observe that the treatment of puerperal convulsions must, like that of almost every other disease, be founded on a system of pure eclecticism. The object is, if possible, to trace the effects to their causes, and to remove those causes in the most efficient manner. I am well aware that by many the sole origin of puerperal convulsions is thought to be uræmia, as the result of excess of albumen in the urine. This, no doubt, is a very frequent, perhaps the most frequent, cause; but I am not convinced that it

is the invariable cause. I also know that practitioners of much ability and experience rely chiefly, if not entirely, upon chloroform as the remedy—perhaps I may venture to say the specific—for this affection. I have had no opportunity of personal observation in the treatment of puerperal convulsions by chloroform; but I can well understand that, in many cases, it must be most useful in rendering the nervous system less susceptible of the exciting causes of eclampsia; and, from the evidence we have in its favour, it is impossible to doubt its efficacy; nor is its judicious employment at all irreconcilable with the principles I have endeavoured to lay down as a guide to the treatment of this formidable malady.

The explanation of the apparent difficulty suggested by the author of the report from which my remarks originated—namely, that, although albuminuria exists for some time previously to the advent of labour, it is, as a rule, only when parturition sets in that convulsions occur—may, I think, be found in the fact that, for the production of eclampsia, it is necessary to have not only an exciting cause, but a special susceptibility to impression on the nervous system. This special condition appertains more or less to the female economy during the entire period of utero-gestation and throughout the puerperal state; and we occasionally meet with convulsions as a result, at various periods of pregnancy, and sometimes also after parturition. The reason why they most frequently occur during labour is, no doubt, because at that time the condition referred to acquires its greatest intensity.—*British Med. Journal, Aug.* 17, 1867, *p.* 125.

105.—CASE OF DEATH FROM ANEURISM OF THE RIGHT OVARIAN ARTERY.

By Dr. W. M. Whitmarsh, Hounslow.

On the eve of September 23rd, 1866, I was summoned to visit Mrs. B., of Lampton, who had been taken suddenly ill. Not being at home when the messenger arrived, my assistant (Mr. Samson) went in my stead, and found the patient in a state bordering on syncope, with a fluttering pulse, great prostration, and violent retching. Very little could be gleaned from her; but she complained of great pain in the abdomen. To alleviate this, he gave her some effervescing medicine, with opium, and ordered warm applications to the feet and axillæ.

Early next morning, her friends sent to inform me that she was much worse, and desired that I should call and see her immediately.

On visiting her, I found her in bed, lying on her back, her face blanched, with an expression of pain on her countenance. The surface of the body was almost cold, and the pulse hardly perceptible. The sickness had continued, and the pain in the abdomen had not been relieved; the latter returned a dull sound on percussion. She had passed very little urine during the night, but declined having the catheter used. From her sister I ascertained that she was 37 years of age; and had been married twelve years, four of which she had spent in America. She "had never been regular," but when she menstruated, the discharge was pale and scanty and accompanied with pain. She had never given birth to a live child, but had had four miscarriages. Her occupation had been that of a laundress, at which she had worked rather hard. She felt the commencement of this attack when she was lifting a basket of clothes from a cart. It began by a feeling of faintness which compelled her to sit down; after this she was removed home.

I questioned the patient whether she had lost blood in any way. She replied she had not. I felt convinced, however, that she had; and told her husband that she must have ruptured a vessel in the abdomen, and doubtless she would not survive many hours. I ordered brandy and water, with ice, and opium to relieve pain. She died six hours after my visit.

On the 26th, I made a *post mortem* examination, and found the lower part *of the abdomen* covered with clotted blood; so much so, that the viscera were

not discernible. The coagulum weighed about three pounds. On removing it I traced the extravasation to an aneurism of the right ovarian artery, large enough to contain a small pigeon's egg. The sac was still filled with clotted blood. I removed it with the uterus, intending to preserve them in spirit; but the friends of the woman would not let me carry them away.—*British Medical Journal, Aug.* 31, 1867, *p.* 177.

106.— N SOME OF THE DISEASES OF THE FEMALE URETHRA AND BLADDER.

By Dr. J. Braxton Hicks, F.R.S., Physician Accoucheur at Guy's Hospital.

[There is a feature in these diseases, common to those of the vagina, uterus, and rectum—namely, the difficulty of discovering the exact seat of the trouble, for on account of the sympathy between the different parts the patient not uncommonly refers her symptoms to some other than the part really affected.]

As is well known, the reflex tenesmus of the rectum produced by a fissured anus is often mistaken for uterine forcing; so, likewise, irritation of the urethra often produces a bearing down of the uterus, and sometimes even prolapsus. Whenever, therefore, a case of much tenesmus comes before you do not content yourself with the patient's diagnosis, but inquire into its commencement, and its original seat. Some difficulty will, doubtless, present itself to you in seeking this point, because many patients forget, in the trouble of the moment, the exact mode of its commencement; and this is particularly true if the pain have changed its seat. And you will also find, even at the moment of your inquiry, a difficulty of distinguishing, from the sensations described, the precise spot of annoyance; whether, for instance, the pain be in the urethra, or vagina, or uterus! whether, also, the bleeding which occurs in some conditions proceed from the vagina or urethra. These points are important, and for their elucidation may require strict investigation by a thorough examination of each of the passages.

I may here also remark that diseases of the bladder and urethra are not so much under the influence of internal remedies as they are of topical, and therefore you will find that I shall lay corresponding stress upon the latter form of treatment. Not but that much can be done by opiates, rest in the horizontal posture, and avoidance of certain beverages, and the correction of any condition of the system which may give rise to irritating urine. But, on the whole, local treatment in the severe forms is much more effective, though in some works treating of this subject you will find that the local remedies are considered too painful to admit of being used, or else they are in great measure passed by, so that the class of cases under consideration has been not so prolific of laurels as many others.

In bringing before you some of the diseases of the female urinary organs, I shall begin from without, and follow the passages inwards to the bladder.

Vascular growths at meatus urinarius.—And first I shall call your attention to those generally extremely sensitive red out-growths of membrane of the meatus urinarius, called vascular growths.

You will find them readily, on separating the labia, by their bright red appearance at the meatus, either in the form of a minute polypus, or as a broad-based elevation of the mucous membrane, partly or wholly surrounding it, or passing some distance within. If you touch this, even as lightly as possible, in the majority of cases the patient will suffer the most acute pain; while in some few there will be no very great suffering. The difference depends, I believe, upon whether the sensitive papillæ (Pacchionian bodies) are enlarged or not. The growth in all cases is hypertrophy of the mucous membrane. In some the growth of vascular element is in excess; in some (the majority), the nerve-tissue; in others the basement layer. Now, in those which are most sensitive you can without difficulty find the Pacchionian

bodies hypertrophied, and very distinct. If they are removed by the snare, they will be found beautifully injected.

You will easily understand that, if these growths are so sensitive, much suffering is complained of during walking, coitus, micturition, &c. But, generally speaking, the patient very seldom is able distinctly to explain the seat of the distress. Sometimes the pain from micturition continues setting up a forcing and bearing down, with a distress about the vulva; but, as a rule, the distress is not at first referred to the "water-passage." You must not therefore expect to be assisted much by the patient herself in the diagnosis. In such cases it is better to examine by sight, where you have any pain and difficulty attending micturition. Perhaps the most marked sign pointing to the urethra which the patient volunteers is, that after the completion of micturition a drop of blood follows; but in many cases this symptom does not occur. When it does, it is important to ascertain whether the blood really comes from the urethra, or from the uterus; because straining in micturition will sometimes cause a small, and indeed often a large, quantity of blood to flow from a malignant os uteri. The indefiniteness of the patient's description as to the seat of trouble was well illustrated in a young woman of sedentary habits, who had for two years complained of uneasiness in the genitals. She could scarcely describe its nature, or where exactly it was, except that it was in front, and that when she leaned forward it hurt her. As I could make out nothing more from her description, I examined, and found one of these vascular growths at the meatus. It being of a polypus form, I removed it with a wire noose. She called two weeks afterwards to say she had entirely lost the annoyance, and I heard afterwards she had had no return of it.

As an instance of the vagueness and distress to which these growths may indirectly give rise, I may instance the case of a woman about sixty, who came to me complaining of incessant desire to micturate; severe straining; urine highly loaded with pus and mucus. To add to this, she had recently taken spirits of turpentine with a view to cure, and this doubtless had much increased her troubles. However, she told me she had very intense pain upon connexion, for her husband still retained much vigour. This, of course, required examination, when I found that she had a large and unusually sensitive vascular growth at the meatus. This in a day or two I removed, directly after which she began to lose the above named symptoms, which in the course of two weeks had passed off, the urine recovering its natural condition. In this case a great amount of the distress was occasioned by reflex irritation, at first set up by the growth, but which she herself had increased.

The treatment of these cases is simple. Where the growth is of the polypus kind, it is best removed by a wire snare—as, for instance, by one which I have described in the "Obstetric Transactions," vol. iii., p. 346. For myself I prefer this mode because you can thereby secure its total removal, which you cannot always if you endeavour to seize it with the forceps and cut it off with scissors; for then not unfrequently the growth, always of a flimsy and vascular character, breaks down, so that it is impossible to lay hold of it again. If we attempt to cut it of with the scissors only, we may be more successful; but I have seen such efforts defeated by the drawing up of the parts at the slightest touch before it can be secured by this instrument. Under these circumstances I have found the most ready mode is that of dividing with the wire noose. Of course we may employ the galvano-cautery; but this apparatus is rather more troublesome to prepare. As a rule, after removal I prefer touching the part with a caustic, such as nitric acid, in the manner described below for the broader-based forms. Little or no bleeding ensues; that which does occur is readily staunched by pressure. In the more sessile forms I prefer the use of nitric acid, as a convenient agent, in the following manner:—Take a piece of soft wood, such as deal; nothing is more at command than a lucifer match. After cutting off the inflammable part, and rounding off the corners, this is dipped in nitric acid, and the superfluous quantities allowed to drain off, so that the wood should only be saturated with it. *It can then be applied* to the growth without any fear of its running over the

parts not affected. Nitrate of silver is not to be relied on to destroy the base of the growth. Either the galvano-cautery or nitric acid may be used, so far as I have found, with the greatest amount of certainty.

Now these growths have a decided tendency to return, particularly if the growth has not been thoroughly destroyed or removed in some manner. Therefore in any case it would be well not to promise a radical cure too decidedly. I have seen a girl in whom the whole meatus was surrounded by a growth of considerable exuberance, and in whom the disease returned three times, which was, however, ultimately cured. These cases are certainly not malignant, but are rather to be considered as nævoid. I may remark, in conclusion, that the intensity of the pain bears no relation to the size of the growth; but the very smallest sometimes produce as severe, if not severer, suffering as others many times larger.

Stricture of the urethra is the next morbid condition to which I wish to direct your attention. It has been described as very rare; perhaps the superlative should be omitted. I have seen many cases of distinct stricture of the urethra. The seat of stricture was near the meatus, and in two appeared to have been the result of some injury or application to the urethra. In all, the symptoms were, as might be supposed, frequent desire to micturate, attended with great difficulty and forcing; a general feeling of bearing down, referred by some to the uterus. The cause of all this was readily detected by examination per vaginam; no disease was found in the urethro-vaginal septum, nor at the meatus. The catheter being passed, great obstruction was experienced; indeed in one only a small conical bougie could be introduced through the stricture. The treatment in these cases was very effectual and permanent—by dilatation by conical bougies, requiring only a few passages of the instrument. It is possible to suppose a more obstinate form of stricture than these—as cicatrices—as the result of injury to the part; but I think it would not be difficult to cure them, like those in the vagina, by stellately scoring with a straight probe-pointed bistoury, and gentle dilatation afterwards. As a rule, we may say that simple strictures of the urethra are not difficult of cure.

Urethritis.—It is by no means a rare, I might almost call it a frequent occurrence, that the urethra becomes affected by inflammation in cases where vaginitis has resulted from either gonorrhœa, simple irritation or injury, or pressure from parturition, &c. It may arise from the irritation of acrid urine, highly charged either with lithates or with ammonia; but the most common of its causes are gonorrhœa and parturition. Sometimes the frequent passage of healthy urine alone, as in irritable bladder, will produce urethritis. In cases of urethritis, simple, chronic, or acute, the pain, by the more intelligent of the sex, is referred distinctly to the water-passage, and to that only; but in as much as occasionally in chronic, and frequently in acute urethritis, the case is complicated with vaginitis, or other abnormal conditions, it is often difficult, sometimes particularly so, to unravel the symptoms, so as to place each to its respective cause. The annoyance complained of by the patient consists generally, not always, of a frequent desire to pass urine; always much pain, scalding or burning, or soreness in the course of the urethra in the act of micturition, lasting for some little time after; sometimes pain in the course of the urethra. The urethra is frequently tender when pressed during the vaginal examination, and a drop or so of purulent mucus may be made to exude from the meatus in certain cases, especially in the acute forms. When the catheter is passed, the patient, in the severer forms, complains much. In the chronic there is always more or less tenderness. In the former, where the vagina is almost always affected, there is general redness of the vulva and vagina; but in the chronic this has generally subsided, so that only the edge of the meatus and the urethra remains red.

The treatment in the acute form must depend upon the cause, and upon the associated states of vagina, &c. It will be treated in combination with them, by warm, soothing vaginal injections, and by rendering the urine as bland as possible. Where it is the result of delivery, these plans are more particularly to be zealously employed; and should there be retention of urine

also, it will be our duty to pass the catheter, notwithstanding the pain it may cause, otherwise we may have a worse malady as the consequence. I need hardly observe that the use of opiates by the mouth is of great value in these cases.

But in the chronic form we must find other treatment, for it is very persistent, and, although not seriously affecting health, except when combined with cystitis, yet it becomes very troublesome both to patient and practitioner. I believe we seldom, if ever, find any advantage in the employment of remedies taken by the mouth. In rendering the urine less irritating according to its condition in lulling the irritability of the urethra, we of course shall find advantage, but beyond that my own experience shows none. But by local treatment the disease is as completely under our control as any disease we have to attack. We may employ either astringent solutions, such as alum, or sulphate of zinc solution, or even one of nitrate of silver; but I think you will prefer the use of these remedies in the more solid form. Nitrate of silver is very efficient, but rather painful; cast into small sticks, and introduced into the porte-caustique, it can be applied for a short or long period to the whole of the urethra, according as is desirable. It will be generally found sufficient to merely apply it on the withdrawal of the instrument. The most convenient form of porte-caustique is one I use also for the canal of the cervix uteri—a silver tube, having a small caustic-holder on the end of a stilet sliding easily within. It is so constructed that the nitrate can only protrude a very short distance beyond the open end of the tube. The tube is passed up, the nitrate being drawn within. As the tube is drawn out the nitrate is pushed out, and the mucous membrane is touched lightly throughout. This mode is efficacious, but, at the same time, there is more immediate pain than with some other preparations; this, however, soon passes off, and in a day or two much relief is obtained. The application should be repeated for about a week, and a third time at the same interval if required.

Perhaps as efficient as any is the use of bougies covered with tannic acid. The best plan is to cover a medium sized gum-elastic bougie with gum-water, and then to dip it into powdered tannic acid The superfluous quantity is then shaken off, and the film allowed to dry. Before using it, pass it gently through the fingers to remove any roughness that may be present; then dip it into gum-water and pass it up the urethra, and leave it in ten or fifteen minutes. This may be repeated once a week. There is but little irritation generally; perhaps the next day some slight increase; but by three or four days the former irritation has much subsided, and then two or three applications at intervals of a week are sufficient to complete the cure.

Sulphate of zinc cast in sticks, such as you have seen me employ in cases of cervical leucorrhœa, may be used by simply passing the stick up the urethra, or as nitrate of silver in the porte-caustique, as above. We may thus speedily rid our patients of an annoyance which it were vain to expect removed by other than local remedies. I have employed each of these remedies repeatedly, and could instance numerous cases of its satisfactory results. One may suffice.

A lady had, two years since, first of all during her pregnancy, suffered pain in the urethra, with much pain during micturition. This was not alleviated by delivery, but remained constantly annoying her to a very considerable extent; and the dread of passing urine had caused a constant nervous state. She had consulted her medical attendant, without relief. I found the urethra very tender when pressed on her vaginam, and on passing a catheter, she instantly said it touched the seat of her annoyance. I passed a stick of dried sulphate of zinc, allowing it to remain about five minutes. This caused her some pain at the time, which however subsided in two hours. From this time she had no suffering worth mentioning. A second application was made, and she was quite free from pain when I last heard of her.—*Lancet, Oct.* 12, 1867, *p.* 449.

107.—SORE NIPPLES.

Dr. BLACQUIERES says, in the *Journal des Connaissances Médicales*, that three or four applications of the following compound cure this complaint:—Cocoa butter, 150 grains; extract of rhatany, ten grains.—*Lancet, Sept.* 21, 1867, *p.* 361.

108.—UTERINE SURGERY IN PARIS.

[The following short article is taken from a report on the present state of surgery in Paris by Mr. SAMPSON GAMGEE, of Birmingham.]

For all examinations and operations on the vagina and uterus Dr. Sims uses the duck-bill speculum, and no other. He dislikes sunlight, and prefers light entering from one window directly at his back to light entering the room from different quarters. The previous evacuation of the rectum greatly facilitates the use of the speculum. A table about four feet long, covered with a blanket, is preferable to a bed or couch for the patient to lie upon. The position is on the left side, body diagonally to the table, so that the buttock rests on the left of the angle next to the operator and the window. The spine straight with the head, which may be raised very slightly if more agreeable; not so the shoulders, for the chest must lie prone, and as flat as possible on the table; neither of the arms under the chest, but well apart. The thighs to be bent at right angles to the trunk; legs at similar angle, an assistant raising the feet slightly. For this purpose a small table, a little higher than the one on which the patient lies, is handy to rest the legs upon. It is convenient to flex the right thigh a little more than the left, so as to bring the right heel just in front of the left ankle. In this position the epigastrium is on a lower level than the pelvis; and great importance is attached to the waist being free from all constriction, so that the abdominal viscera may gravitate freely forwards and downwards, and allow of the entrance of air into the vagina when the speculum is introduced. As already observed, this position is the invariable one for all inspections and operations. But one exception is made. When the patient has cancer of the womb, in order to avoid hemorrhage or other accident from striking the cervix with the speculum, this is introduced while the patient is on her knees on a table, the chest prone and the back concave.

Before using the speculum, Dr. Marion Sims invariably ascertains the size, position, and direction of the womb; and he does this, while the patient lies on her back, by left digital examination, palpation with the right hand above pubes serving to bring the uterus well within reach.

Whenever he introduces anything into the uterus, be it a sound or a tent, a syringe or a knife, he fixes the cervix with a tenaculum, after introducing the speculum, and is thereby enabled to ensure steadiness, and, when necessary, to draw down the neck of the womb into the vagina, and so direct it as at once to see into it. It is only when he so fixes and sees it clearly that he divides the cervix, thus making it an accurately surgical operation, and not merely a mechanical one. In operating for fistula, he always fixes the vaginal wall with a tenaculum before paring it or passing sutures.—*Lancet, Sep.* 7, 1867, *p.* 296.

109.—ON THE DEFECTS OF ORDINARY SPONGE TENTS; WITH AN ACCOUNT OF A NEW KIND OF CARBOLISED SPONGE TENT.

By ROBERT ELLIS, Esq., Surgeon to the Chelsea and Belgrave Dispensary.

The author, after adverting to the serious inconveniences and occasional danger incident to the use of the common sponge tents, proceeds to describe a new kind introduced by himself under the name of carbolised sponge

tent. In this invention sponge is still retained as the dilating agent, but the tent is prepared by a peculiar process which renders it incapable of putrefaction, without diminishing its value as a dilator. This is accomplished by introducing into the core of the tent several threads of cotton wick steeped in carbolic acid; and after the sponge is rolled into its proper shape, it is then immersed in cocoa butter to which a certain quantity of glacial carbolic acid is added. The disinfectant properties of this agent completely protect the tents, and they are withdrawn in an inodorous state even after a stay of twelve or eighteen hours in the cervical canal. The shape and size of these sponge tents also differ from the ordinary kind which are both clumsy and dangerous, as well as disgusting, in their use. These are spindle-shaped, and thus accurately adapt themselves to the fusiform character of the canal which they are intended to dilate. They require no support when *in situ*, but, by virtue of the immediate fusion of the enveloping material, they take to their work immediately, and are firmly kept in position. The author stated that he had a large experience of their utility and value; and that they could be procured, ready for use, from Messrs. Bradley.

Mr. Ellis also exhibited an Introducer for Sponge Tents. This instrument consists simply of a slender uterine sound tapered to a fine point, which is thrust up into the tent. A short distance from its extremity a small flat metal collar is attached, on which the sponge tent rests, so as to be firmly supported while it is pressed into its place. Mr. Ellis spoke in high terms of the great handiness of this contrivance, which may be obtained of Messrs. Meyer and Meltzer.—*Lancet, Sept.* 7, 1867, *p.* 295.

110.—A NEW MODE OF VACCINATING RAPIDLY.

Dr. Carenzi, of Turin, has had a silver finger-ring constructed, to which is fixed a little capsule of the same metal. The operator places this ring on the first phalanx of his left thumb, and the capsule being filled with lymph obtained from capillary tubes, he goes on dipping his lancet and vaccinates with great rapidity.—*Lancet, Sept.* 21, 1867, *p.* 361.

111.—ON NIGHTMARE OF CHILDREN.

By Dr. Sydney Ringer, Assistant Physician to the Hospital for Sick Children, Great Ormond Street, London.

Violent screaming, which cannot be quieted, and which may last for a few minutes to several hours, is frequently witnessed in children. This is generally produced by one of three causes—hunger, pain, or nightmare. The following is a fair example of a case of screaming from the last cause.

Charles L., 2 years old, came under my care at the out-patient department of the Children's Hospital. The child was badly nourished, and was afflicted with a frequent hacking cough, that troubled him much more at night than day. For two months he had, twice or three times each night, started from his sleep, screaming violently. Each paroxysm of screaming lasted about half an hour. Sometimes he rolled about the bed, threw his arms wildly about, and knocked his head violently against the bed; on other occasions he sat up in bed and screamed so violently that he became black in the face. While thus afflicted his eyes rolled, and he appeared to be quite unconscious, as he did not recognise his mother, and could not be brought to by her care and attention. His mother stated he did not appear to have his senses. He gave no signs of being in pain. He did not talk, he only screamed violently. After each paroxysm he fell asleep, but his sleep was disturbed, and his eyes still rolled, and he frequently moaned. His gums were neither red nor swollen. His appetite was good, but his bowels had been relaxed for three weeks, and his motions were green and slimy, but no worms were ever seen in them. He was fed judiciously. He wetted the bed at night. During the

fortnight that preceded his application for relief at the Hospital, he had suffered from two convulsive fits, when his arms worked and his face twitched. Each of these fits lasted twenty minutes.

Such screaming may occur in children of all ages; but, while it is met with in children of ten or twelve years of age, it is of more common occurrence in those of a few months to two or three years old.

These attacks may last a very variable time, for though the paroxysm may pass away in a few minutes, it sometimes continues for half an hour to one or even two hours. During this time the screaming is violent and continuous. Sometimes the children appear to be asleep throughout the paroxysm, while other children wake from their sleep, but continue to scream with unabated violence; but even when awake they often appear to be unconscious of what occurs around them. They seem to be, as their mothers state, "out of their senses;" thus they for the most part cannot be quieted. Others appear to awake thoroughly and are then terribly frightened, and often tremble all over. Such children can generally be quieted in a short time by kindness and attention paid to them, but they remain for some time much agitated, and refuse to be left alone, or, if removed from their bed, they are afraid to return to it. Some children cry only a little, but they wake up frightened and trembling.

Such screaming may continue to occur for months and even years, sometimes disappearing for a time, and then, from various causes to be immediately mentioned, it returns again. It is often repeated several times each night for several months.

Such attacks are naturally a source of much annoyance and much anxiety to the parents, and thus medical men are not unfrequently consulted for this affection. The screaming may be so violent that the child becomes "black and blue" in the face, and occasionally it even terminates in a general convulsion. This, however, is unusual, and in my experience occurs only in children who suffer from convulsions without screaming and from other causes. Such paroxysms of screaming sometimes recur only with long intervals. The child may have one attack, but this may not be repeated for some weeks, or some months. On the other hand, they may recur ten or twelve times a night.

This screaming differs from delirium, as it does not occur in those diseases accompanied by delirium. Moreover, there is no incoherent talking or muttering, while some children can be roused from this state, and are then perfectly rational, although greatly frightened; it occurs only during sleep. The mothers often call it nightmare.

The children the subject of this affection, are very generally pale, often ill-nourished, and out of health. The immediate cause of this screaming appears to be some disturbance of the stomach and intestines. The nature of this affection of the intestinal canal may be very various in different cases; for one child may suffer from constipation while another is troubled with diarrhœa. This disturbance is very generally dependent on food ill-suited to young children; for this irregularity of the bowels, and screaming which accompanies it, are especially frequent in those children who have been brought up by hand, and who, consequently, suffer on the one hand from diarrhœa, on the other from constipation. Children thus reared suffer, as is well known, very generally from constipation, and pass hard, pale, lumpy motions, sometimes like marbles. These masses may consist of fæces; they are often composed of coagulated undigested milk, of a yellowish or greenish-yellow colour outside, but are white and very cheesy within, looking like, and, indeed, being composed of, curds of milk. (It may be here mentioned, in passing, that not uncommonly children pass by the bowel, or sometimes vomit, large masses of the same composition. These are generally two or four inches long, and about an inch in diameter. They often excite much wonder and anxiety on the part of the mother. When broken, the white curdy appearance at once declares their nature.) Children who suffer from the affection now under consideration are sometimes infested by thread worms, and also show signs of the altered condition of the mucous

membrane of the stomach and intestines by itching, heat, and dryness of the inner part of the nose, with itching at the anus. This screaming is increased by anything that interferes with the general health of the child. Thus, it is observed to be worse when the teeth are making their way through the gums, although the irritation and pain which arise from teething appear to be incapable of themselves of exciting this screaming. It is also made worse by slight attacks of catarrh of the lungs, or eruptions on the body. By treatment this screaming can usually be at once arrested. Both general and local treatment are in most cases required, the former to improve the general health, the latter to remove the conditions immediately exciting the screaming.

The diet should be attended to, and any irregularity in the hours at which food is given to the child, or any unsuitability in the nature of the food, must be remedied. Attention to these points will very generally arrest any diarrhœa which may be present, but constipation, with hard shotty motions, will generally prove more obstinate, for such motions are almost invariably passed by young children under six months old when brought up by hand, and this although they may be correctly fed and take nothing but good cow's milk sufficiently diluted with water. We have seen that these hard, round, lumpy motions are partly composed of coagulated undigested milk. This coagulation in mass can sometimes be stayed by the addition of alkalies to the milk, such as limewater or bicarbonate of soda. The latter is preferable for this purpose, as limewater confines the bowels, and thus bicarbonate of soda should be preferred.

If the bowels are confined, an active purgative will, in the great majority of cases, suffice to stay the screaming, and will insure to the child calm and refreshing sleep. A powder of rhubarb and soda repeated every night, or every other night, till three powders have been given, is useful. If the child be pale, and the constipation recurs and is obstinate, the following prescription will be found very advantageous, namely: Steel wine, to which is added a few drops of tincture of rhubarb, in quantities adapted to the age of the child and to the obstinacy of the constipation. Usually six drops of tincture of rhubarb in a teaspoonful of steel wine given three times a day will open freely and comfortably the bowels of a child from six to nine months old.

In order to effect a permanent cure it is often necessary to give medicines to improve the general health of the child, as these children are frequently pale and badly nourished.

Thus, in children suffering from the affection we have just described, to effect a permanent cure, if the general health be bad, treatment must be directed to the restoration of the body to sound health. In these cases iron, cod-liver oil, with cold sponging prove most useful. Of the various preparations of iron, the tincture of the sesquichloride, in my experience, is decidedly the best. It has appeared to me that bromide of potassium is able to stay this screaming, but as its administration has been accompanied by the use of purgatives, or a regulated diet, it is difficult to determine how far the bromide was useful. It is, however, I feel sure, worthy of a trial in obstinate cases. Cold baths must be given with care; for while they may, if properly administered, do much good, if administered without certain precautions they will do great harm to children. If too great a shock be given to the child, depression of the system will be produced, and this may last even several days after the bath is administered, when the child may be languid and depressed, and may suffer from much chilliness with loss of appetite. Thus the amount of shock produced by the bath must be regulated to the age and strength of the child. In cold sponging of the body the shock caused is proportioned to the coldness of the water and the length of time the bath is continued; while the younger the child, or the weaker its health, the less able is it to bear up against the effect of the shock to which it is exposed. Hence with young children, and especially with those whose system is depressed, the bath should be continued only for a short time, and if the weather *be cold,* the water must be slightly warmed. When the child is weak, the

bath should be continued at first for a few seconds only, and its duration be gradually increased as the child becomes accustomed to its use.

If the following simple plan be adopted, the child, even if very weak, can take the cold bath with advantage, and all chance of depression is removed. The child should be placed before a good fire with its feet in warm water, while the cold water is freely poured over every part of the body except the head and face. The healthy reaction, with the agreeable sensations which follow the use of the bath, may be much increased by placing the child for a short time in the warm bed from which it had just previously been removed. The bath should be given immediately the child leaves its bed, and the breakfast should be taken soon after the sponging is completed.—*Medical Times and Gazette, May* 4, 1867, *p.* 465.

ADDENDA.

112.—DISCOVERY OF A NEW GENERAL ANÆSTHETIC—BICHLORIDE OF METHYLENE.

By Dr. B. W. Richardson, F. R. S.

[Dr. Richardson has discovered a new general anæsthetic, which he for the first time described and exhibited at the late meeting of the British Association.]

The fluid is a bichloride of methylene, its composition being on the new formula CH_2Cl_2. The substance has an odour as sweet as that of chloroform, but it boils at 88° Fahr., whereas chloroform, the terchloride of methylene, requires a temperature of 142° for boiling. The bichloride of methylene rapidly and easily narcotises animals to perfect anæsthesia. It causes scarcely any excitement, and recovery is most perfect. In action it seems to combine the properties of chloroform and of ether, but it is more readily administered than either, and its effects are more permanent.

The bichloride of methylene belongs to the class of chemical organic substances known as the "Monocarbon series." The labors of the modern chemist have enabled him to separate organic bodies, such as this, into certain divisions, greatly to the illustration and simplification of the science. In making his divisions, the chemist recognizes as preliminary facts—first, that every body of the organic type is built upon the element *carbon;* and, secondly, that this element possesses the property of combining with itself, by which property it is enabled, by the combinations it makes with other elements, to produce distinct series of compounds, which are classified, in accordance with this rule, into Monocarbons, Dicarbons, and so on. In illustration, let me write down the names of the first five series:—

The *Methyl* series. In this the carbon stands as *one*—C.

The *Ethyl* series. In this the carbon stands as *two*—C_2.

The *Propyl* series. In this the carbon stands as *three*—C_3.

The *Butyl* series. In this the carbon stands as *four*—C_4.

The *Amyl* series. In this the carbon stands as *five*—C_5.

And so on up to the melisyl series, where the carbon stands as C_{30}.

The fact of the multiplication of carbon in these series is brought strikingly before us by a simple experiment. Every one of the groups above named, as well as each of the higher groups which we have not followed, possesses an analogous compound called an alcohol. The alcohols, in fact, are those bodies which first come nearest to hand from nature herself, and from which the chemist has commonly started in the course of discovery. We have before us at this present moment the alcohol of five series. We have methylic alcohol, or, as it is commonly called, wood spirit—naphtha; we have ethylic alcohol, the ordinary alcohol of the table; we have propylic and butylic alcohols; and lastly, we have this peculiarly odorous alcohol, the amylic, or fusel oil—potato spirit. I want to show you that in every one of these there is a difference of carbon. I therefore take five ordinary spirit lamps, and trim with fresh wicks, and charge them respectively with these alcohols. Having done this, I set the lamps alight, and test the different flames. The methylic *lamp* burns with little light and no smoke. I hold over the flame a white *plate, and there* is no carbon. The ethylic lamp yields to the plate the faint-

est trace of darkness, a little carbon that has not been consumed. The propylic and butylic lamps, with their cloudier flames, yield much deposit; while the amylic lamp, burning dull and heavy, its light peering as it were through smoke, yields as much unconsumed carbon as suffices to cover the surface of the white plate in a few seconds.

In combining with other elementary bodies to produce the groups or the families of the organic series of which I have spoken, the carbon forms first an union with hydrogen, by which there is produced a new and basic substance called a *radical*. The radical once-formed compounds can be made on the bases of the inorganic world; as chlorine, for instance, in combining with the base sodium, produces the compound called chloride of sodium.

The monocarbon group of bodies with which we have to-day to deal affords a good illustration of all these facts in detail. When the carbon in this series combines with three atoms of hydrogen, it produces the radical methyl CH_3, from which various compounds may be made. For sake of clearness, I will place these compounds before you in order and in the tabular form:—

Radical methyl, CH_3.

Compounds.

Hydride of methyl (marsh gas).	$C H_3 H$
Chloride of methyl	$C H_3 Cl$
Iodide of methyl	$C H_3 I$
Bromide of methyl	$C H_3 Br$
Fluoride of methyl	$C H_3 Fl$
Cyanide of methyl	$C H_3 C N$
Nitrite of methyl	$C H_3 N O_2$
Methyl alcohol	$C H_3 \left\{ \begin{matrix} O \\ H \end{matrix} \right.$

In these compounds you will perceive that the radical methyl remains always the same, CH_3, and all the new compounds, hydride, chloride, and the rest, are made by the direct addition of an element to the radical. The compounds are, in fact, salts of the radical. If I were to describe the ethyl series or the other series, I should be able to illustrate this same rule, but I must pass now to a further point—viz., to the production of a different series of substances by what is a true change in the constructon of the radical itself. The monocarbon series will serve us for one more illustration and the illustration will bring us, by a natural and easy stage, to the particular substance which is now the object of our learning.

We start, then, with the first compound on the table I have just written—viz., the hydride of methyl, or marsh gas. This, as we have seen, contains the radical methyl in combination with one atom of hydrogen. It may be represented as CHHHH.

In this compound the carbon molecule, or smallest combining part of carbon, is said to be saturated with hydrogen—that is to say, it will combine with no more. But if marsh gas be acted upon by chlorine a change, or rather a series of changes, is induced, leading to the formation of new and distinct compounds: I mean the chlorine can be made to replace the hydrogen or become substitute for it, and, according to the character and extent of the substitution, so is the product. For example:—

(*a*) One part of the hydrogen can be replaced by one of chlorine; then we have the radical methyl CH_3 combining with chlorine, and the result is *chloride of methyl* $CH_3 Cl$.

(*b*) Two parts of the hydrogen can be replaced by chlorine. In this change there is no longer the old radical methyl CH_3, but a new radical *methylene* CH_2, and this new radical is combined with two of chlorine. The resultant compound, therefore, is the *bichloride of methylene* $CH_2 Cl_2$.

(*c*) Three parts of the hydrogen can be replaced by three of chlorine. In this change there is no longer the radical methyl CH_3, nor yet the radical methylene CH_2, but a new radical CH *formyle;* and this new radical is com-

bined with three of chlorine. The resultant compound, therefore, is the *terchloride of formyle* $CHCl_3$, common chloroform.

(*d*) The whole of the hydrogen may be replaced by chlorine when the radicals of carbon with hydrogen are expunged altogether. The resultant compound is the *tetrachloride of carbon* CCl_4.

Thus, step by step, new bodies can be constructed out of marsh gas, from the chloride of methyl to the tetrachloride of carbon; and conversely from the tetrachloride of carbon the changes may be carried back again to the reconstruction of marsh gas.

To keep to our chlorine compounds, we have, then, in view four bodies thus composed:—

C H H H Cl	Chloride of methyl.
C H H Cl Cl	Bichloride of methylene.
C H Cl Cl Cl	Terchloride of formyle.
C Cl Cl Cl Cl	Tetrachloride of carbon.

By keeping these brief facts in mind, the position of the bichloride of methylene in this organic series will be easily remembered.

Before I leave these substances as thus grouped together in their chemical place, let me say of them that they all possess the power of producing anæsthesia when they are inhaled as vapours by men and animals. That the two latter of them have this power—I mean chloroform and the tetrachloride of carbon—is well known, but that the two first on the list also exert the same influence is new as a scientific fact. I discovered that chloride of methyl was a certain and gentle anæsthetic in July last, and this led me to hope that something more stable and manageable could be obtained—something that should stand between the chloride and chloroform. That substance is now found in the second body of the group—the bichloride of methylene. That this, like the chloride, would produce rapid, safe, and easy general anæsthesia, I discovered by experiment on August 30 of the present year.

Amongst the various substances which have come before us in what has preceded, there are several other besides the bichloride of methylene which might fairly engage our attention; but as we have the bichloride for special consideration, I shall diverge only, and that but for a moment, to refer to the chloride of methyl. The chloride of methyl exists in all ordinary temperatures as a permanent gas. As I have already said, it acts as anæsthetic when given by inhalation. It is very soluble in ether, and when ether is saturated with it the perfect compound fluid is one of the most perfect of anæsthetics. Unfortunately, this compound is not very stable, and therefore it is not so promising a fluid as could be wished. But I believe it to have no equal in the way of evenness of action. The sleep produced by it is rapid, gentle, profound, prolonged, and I found in an experiment, where I may say I forced the animal to die by increasing the quantity of the vapour, that the muscular irritability was perfect one hour and five minutes after death. I notice this fact because, if the more manageable and stable bichloride of methylene should prove unfaithful to experience and to our hopes, we have still this compound remaining for our practice.

One other fact is of interest in reference to the chloride of methyl: it is soluble in water, and water charged with it will take up four volumes. Chlormethyl water properly prepared is here for your inspection, and I pass it round with a little sugar to render it palatable. You will find it rather agreeable to drink, and I may add that as a drink it is a potent intoxicator. I took half an ounce with a very decided effect, but the influence it exerts is of course very transitory. The fluid might be used with great advantage in medicine as a soothing agent, and as a cooler or refrigerator. At a push this fluid could, I doubt not, be administered to the extent of producing general anæsthesia, and an operation might thus be painlessly performed after a libation instead of an inhalation. Altogether I predict for chloride of methyl a most useful and important place in our remedial treasury; but we will, if you please, now return to the bichloride of methylene, and its physical properties.

The bichloride of methylene is a colourless fluid, having an odour much like the odour of chloroform. It is pleasant to inhale as vapour, and it produces very little irritation of the fauces and air-passages. It boils at 88° Fahr. Its sp. gr. is 1·344. The sp. gr. of its vapour is 2·937; it is, therefore, nearly three times heavier than air.

From its position physically it combines many of the properties of chloroform with those of ether, and these peculiarities must be remembered in its administration. From its easier evaporation it requires more free administration than chloroform, and from its greater density of vapor it requires less in quantity than ether.

There is another physical difference between the bichloride and chloroform to which I would particularly invite your observation. If I take chloroform and diffuse the vapour of it through air in a bell jar, thus, I find, when a taper alight is plunged into the jar, that the light is extinguished—in other words, the combustion is stopped. On further inquiry I also find that the chloroform itself though it has stopped the combustion, has itself undergone no obvious chemical change. We say, therefore, that the chloroform has acted by a catalytic process: it has stopped oxidation by its mere presence without undergoing decomposition. I take next the bichloride of methylene, diffuse that in vapour through the jar, and plunge in the lighted taper. And now see the difference: the vapour burns in a brilliant flame, filling the jar. Here I have decomposed the substance; the carbon has been turned, by union with oxygen, into carbonic acid, and the hydrogen and the chlorine have been turned, by their new union, into hydrochloric acid. The proof of this latter fact concludes a singularly pretty experiment; I pour a few drops of strong ammonia into the jar in which the bichloride of methylene has been burned, and I produce a dense cloud of chloride of ammonium in white vapour which pours out of the jar like water. I have been careful in showing these experiments with chloroform and bichloride of methylene, and the different behaviours of the latter in the presence of flame, because the experiments bear on one of the most able and ingenious theories ever put forward to explain the action of anæsthetics on living organisms. Some of you will know that I refer to the theory of Dr. Snow. Snow, observing that the vapour of chloroform extinguished flame, as we have just seen, reasoned that, as it thus stopped the combustion of a taper, so by its catalytic action it stopped the combustion of blood, from which arrest all the after phenomena of anæsthesia took their origin. "I could demonstrate all the phenomena of anæsthesia on a farthing candle," was one of his striking epigrams. But here we have a true anæsthetic, which will burn readily, giving brisk combustion. This fact, in so far as it goes, is not in accordance with the theory of my late distinguished friend, and in pointing out the fact, I do no more in correction than I should for a favoured theory of my own or than he would were he here to speak for himself.

The bichloride of methyl mixes readily and well with absolute ether, and as the two fluids have nearly the same boiling point—four degrees of temperature being the extreme of difference—when they are combined they form a compound which vaporises evenly and equally. The difference in the specific gravities of the two vapours is the only objection to the combination. The bichloride further combines with chloroform in all proportions.

One more physical matter in respect to the bichloride of methylene, and this part of the subject may be concluded. The fluid should have at all times a neutral reaction to test-paper. If it show any acidity, there is present a trace of hydrochloric acid, and the vapour, which under such circumstances would also contain the acid, would be irritating to the throat, and perhaps dangerous to life. The presence of acid in the fluid is not a very probable accident—not more probable than the presence of chlorine in chloroform—but it might occur, and it ought therefore to be known. To prevent decomposition, the bichloride of methylene should be kept like chloroform, well guarded from the action of sunlight.

To show the relative effects of bichloride of methylene, chloroform, and tetrachloride of carbon, Dr Richardson placed three pigeons in glass jars,

and caused each of them to inhale the vapour of one of the above-named anæsthetics. The pigeon to which chloroform was supplied was the first to become insensible; on being removed, and placed on the table, it gradually recovered in a few minutes. The bichloride of methylene was longer in producing anæsthesia; but the duration of the insensibility was longer, and the animal recovered more suddenly than the other. The production of anæsthesia with the bichloride of methylene appeared to be attended with less acceleration of the respiration than when chloroform was used. The animal treated with tetrachloride of carbon remained unaffected for a much longer time than the others, owing to the high vaporizing point of the compound and the coldness of the atmosphere. The vapour, when it at last began to act, produced some sickness, which was followed by anæsthesia.]

There is one effect connected with the bichloride which I regret to have to record; the bichloride of carbon in some instances produces vomiting; whether in the human subject that very unpleasant symptom will be less frequent than after chloroform, experience only can decide. Owing to the readiness with which the bichloride finds its way out of the organism, it is probable that the act of vomiting, when excited by it, will be less severe and less prolonged, but beyond this it would be unsafe to speak. The fact that vomiting is produced is all that is positive, and it is a misfortune that must not be concealed.

On animals the bichloride of methylene acts more evenly on the respiration and the circulation than any other anæsthetic with which I am acquainted. When the breathing is quickened the pulse also is quickened in proportion, and when the breathing is slow and tranquil the pulse is the same. This is a good point, because there is no condition more perilous than disturbed balance of the circulating and respirating systems. The equality of action depends on equality of diffusion of the agent through the nervous centres, an equality evidenced also in the general case of the transition of the subject from the first to the third degree of narcotism, and in the suddenness and completeness of the recovery when that once commences.

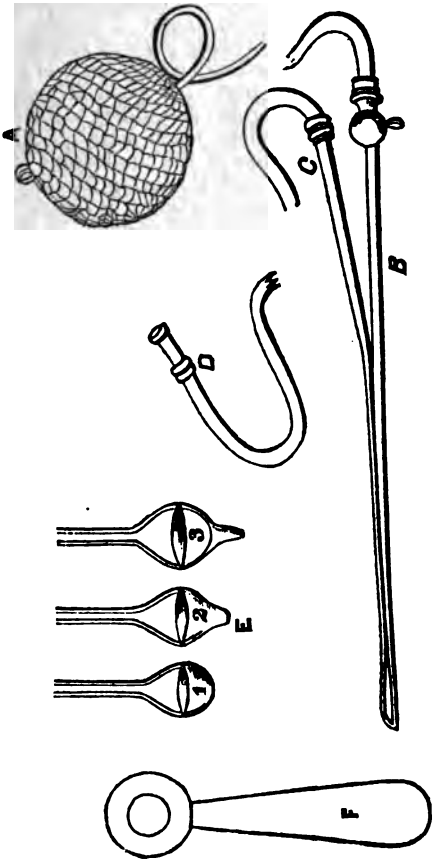
On administering the Bichloride of Methylene to the Human Subject.—When I had learned by repeated experiments, that the bichloride of methylene could be safely administered to inferior animals, I inhaled it myself until it produced insensibility. I found the vapour very pleasant to breathe, and little irritating, while drowsiness came on and unconsciousness without any noise in the head or oppression. I recovered also as the animals seemed to recover—at once and completely. I felt, in fact, as though I had merely shut my eyes and had opened them again. In the meantime, however, I had performed certain acts of a motor kind unconsciously; for I inhaled the vapour in the laboratory, and there went into sleep, but I awoke in the yard adjoining. This was on September 28th, last. I inhaled on the occasion from a cup-shaped sponge. Since then, I have inhaled the vapour in smaller quantities from several instruments with the effect of proving that there is little difference required for administration between the bichloride and chloroform. The bichloride may be inhaled from a moist sponge, from a balloon or Clover's bag, or from a funnel such as this, made by stretching a few layers of thin cloth over a small wicker work such as is used for surrounding a flower-pot, and which opens or closes to any size that may be required. A metallic inhaler such as Snow's chloroform inhaler presents too small a surface for evaporation. In the course of administration, a little more bichloride of methylene is required in the earlier stages than would be required if chloroform were being used, the fluid being more easily vaporisable, and the loss greater. One drachm of bichloride to forty grains (or, in common phrase, minims) of chloroform is a fair statement of the difference required; but when the narcotism is well set up, less of the bichloride is needed, in animals, to sustain the effect, the repetition of the administration not being so frequently demanded.

The Destructive Power of Bichloride of Methylene.—Like all anæsthetics *given* by inhalation, the bichloride of methylene has the power to destroy *life. Its safety* as a general anæsthetic must, therefore, be accepted as rela-

tive rather than absolute. I have tried to ascertain its relative value with as much care and candour as I could summon, and the result of my work leads me to hope that the balance of safety is on the side of the bichloride. Three observations bring me to this reasoning. First, I find that if two animals of the same age and kind, say pigeons, be placed in chambers of the same size, and exposed at the same temperature, and under other conditions the same, to equal values of chloroform, tetrachloride of carbon, and bichloride of methylene, the resistance to death will be as fourteen to five in favour of the bichloride of methylene against the tetrachloride of carbon, and as fourteen to nine against the chloroform.—*Medical Times and Gazette, Sept. 7, Oct. 19, and Nov. 2, 1867, pp. 259, 423, 479.*

113.—THE BLOW-PIPE GAS CAUTERY.

At the last meeting of the Royal Medical and Chirurgical Society, Mr. Alex. Bruce exhibited an instrument lately invented by himself (of which we give a woodcut) in which the intense heat of the blow-pipe flame is made available as a cauterising agent. The instrument essentially consists of two tubes, about six inches in length, one of which serves as a burner, and is capable of being attached to an india-rubber ball containing the requisite supply



Mr. Bruce's Blow-Pipe Gas Cautery.

A, India-rubber ball to receive the supply of gas. Represented proportionally much smaller than natural.
B, Tube connected with the receiver: the burner. D, The mouth-piece.
C, The blow-pipe tube.
E, The platinum cups of three different shapes. Represented proportionally larger than natural.
F, One form of shield.

of gas, this receiver being easily filled from any ordinary gas-jet by means of one of Dr. Andrew Clark's pump-balls; the other tube is attached to the former somewhat obliquely, so that a stream of air blown through it by the operator impinges upon the flame, and converts it from one of rounded form and low intensity into a sharply-pointed conical jet, possessing sufficient heat to fuse the ordinary metals. This may be applied directly to the part, or it may be employed to heat a small platinum cup, which is so attached to the extremity of the tube as to receive the flame, and which becomes almost instantaneously of a bright red heat. The cups are made of various shapes, according to the uses to which they are to be applied, the three most useful forms being the hemispherical, the conical, and the pointed. Shields of various sizes may also be used to protect the surrounding parts during the application of the naked flame or of the platinum points in cases in which it is desirable to limit the action as much as possible. The whole apparatus is packed in a small box, and has the advantage over the galvanic battery both in portability and cheapness. The instrument was made from Mr. Bruce's design by Mr. Baker, of Holborn, who, we are informed, will shortly be prepared to supply them to the profession.—*Medical Times and Gazette, June* 1, 1867, *p.* 598.

114.—AN ANODYNE FORMULA.

The following formula is recommended for combining chloroform and morphia for internal administration. One part, by weight, of morphia is dissolved in two parts of rectified wine-vinegar and twenty parts of rectified spirit of wine; and the solution, when cold, is mixed with eighty parts of chloroform. One drop contains the three-hundredth part of a grain of morphia. The dose for a child is two to fifteen drops; for an adult, thirty to forty drops. It is said to give relief in most painful affections much more quickly and certainly than morphia alone, and to leave none of the unpleasant after-effects of opium. The subcutaneous injection of morphia during the chloroform narcosis, is strongly advocated in all those cases where it is desirable to maintain the state of unconsciousness for a lengthened period.—*British Medical Journal, April* 13, 1867, *p.* 422.

SYNOPSIS.

(ARRANGED ALPHABETICALLY,) CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF YEAR; INCLUDING SEVERAL PAPERS NOT INSERTED IN THE BODY OF THE WORK.

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCEROUS AND OTHER TUMOURS.—*Internal Use of Lime.*—Dr. Peter Hood, of London, gives an account of a case of cancerous tumour of the breast, which entirely separated, leaving a raw granulating surface, this result being attributed by the patient to the internal use of small quantities of lime from the inner surface of oyster shells. This she had taken in consequence of hearing of a case which had been cured by the same treatment. The *modus operandi* suggested is, that ossification of the arteries of the tumour occurs, whereby its nutrition is cut off, and its death ensues. This mode of treatment we would suggest is most likely to be of use in the slowly-growing tumours of persons of advanced age. Previously to scraping out the "small white" part of the interior, the shells require baking for three nights in a slow oven, and the dose recommended is as much as will lie on a shilling once or twice a-day. (Lancet, Oct. 12, 1867, p. 454.)

CHOLERAIC AND ZYMOTIC POISONS.—*Nature of.*—Dr. Halford, of Melbourne, has discovered that by the bite of a cobra-dicapella molecules (cellular) of germinal matter are thrown into the blood, and by their rapid multiplication destroy life. This is not theory, for they can be detected without much difficulty, and examined microscopically. They are circular cells, one seventeen-hundredth of an inch in diameter, and containing a round nucleus and other still more minute spherules of living germinal matter. He says, "I have many reasons for believing that the materies morbi of cholera is a nearly allied animal poison." If so, may we not hope to know something definite of the poisons of hydrophobia, small-pox, scarlet-fever, and, indeed, of all zymotic diseases? (British Medical Journal, July 20, 1867, p. 43.)

FEVER.—*Only one Fever Poison.*—There is only one fever poison, and the forms of fever known as typhus, typhoid, and relapsing, are only varieties of one disease. The author has had extensive opportunities of observation, and his arguments in support of his opinion are well worth perusal. (Dr. H. Kennedy, p. 18.)

STIMULANTS.—*Abuse of.*—Of course stimulants are often needed in cases of fever, but young persons with typhus and typhoid do far better, I believe, without them. That they make good recoveries on simple milk diet is a fact which no argument can gainsay; and on the other hand a marked improvement takes place in some cases where a stimulus has been left off. In bronchitis I have repeatedly seen improvement after stimulants have been omitted. As regards heart disease, I am convinced that the amount of mischief done by stimulants is immense, and that the utmost discrimination is required in their use. There are cases where an undoubted benefit is conferred by them; and there are others where alcohol has induced palpitation, fluttering, great distress, and constant sleepless nights, and where, on the

other hand, the withdrawal of the spirit, and the substitution of a dose of digitalis or henbane, has been of the most essential service. (Dr. S. Wilks, p. 37.)

Typhoid Fever.—*Enlargement of the Spleen.*—Typhoid fever often commits the greatest ravages among young people in a guise not at all times easily detected, for the eruption is far from constant. There is, however, one clue of the greatest importance in the diagnosis of such doubtful cases. This is enlargement of the spleen, which, if it is found, may enable us to decide that typhoid fever is present. (Dr. Jenner, p. 17.)

Headache in Typhoid Fever.—If headache prevail throughout this disease it is due to cerebral mischief, and not to the fever itself. (Dr. Jenner, p. 17.)

Opium in Typhoid Fever.—When certain symptoms are present in a case of typhoid fever, we must not be afraid of administering opium. It is required when there has been insomnolency for a long period, combined with exhaustion and a peculiar pulse. That character of pulse is hardly to be described, but it is extremely rapid, and difficult to be counted because of its weak, fluttering, and indistinct character. The beats appear to run into one another, without that appreciable interval present in all other variations in the pulse, whether in health or disease. An interesting case is related, in which a profound slumber resulted from the administration of half a grain of opium, and recovery from an almost hopeless state ensued. (Dr. N. G. Mercer, p. 27.)

Typhus Fever.—*Stimulants.*—Every case of fever must be treated on its own merits, but as a rule old people require stimulants, whereas young persons do not; the danger in the first being from exhaustion, in the second from the head symptoms. Every now and then a case occurs in young people in which free stimulation is required, just as in some cases in elderly people it will do harm. It is folly to compare one plan of treatment statistically with the other, no general rule can be laid down. (Dr. T. J. Maclagan, p. 25.)

The action of alcohol in fever is two-fold, stimulant in small doses, and narcotic in larger. A fever patient is much more sensitive to both these actions than a person in health. When along with a frequent pulse and a weakly acting heart, there is sleeplessness or delirium, alcohol may be employed as a soothing agent with more benefit and safety than opium. Two ounces of whiskey made into toddy, may, under these circumstances, be administered, with the effect of inducing a comfortable sleep, after opiates have completely failed. The relief to the delirium found by Dr. Todd to result from the continuous administration of brandy was owing to narcosis, and this continued for days cannot but be injurious. (Dr. J. B. Russell, Glasgow Medical Journal, Oct. 1867, p. 205.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Delirium Tremens.—*Capsicum.*—Give a dose of twenty grains of capsicum in a bolus, as early as possible, in cases of delirium tremens. It produces a most powerful stimulant and sedative effect. A case is related in which the patient exhibited tremor in almost all the muscles of the body, chilliness, debility, sleeplessness, foul tongue, severe and general uneasiness, but a total absence of illusions, horrors, or delirium to any degree. The disease appeared to be at once cut short by the treatment here recommended. This case is an instance of that form and stage of the affection which will most probably be benefited. (Dr. Lyons, p. 68.)

Epilepsy.—*Strychnine.*—In strychnine we possess a drug which will always control the excitability of the medulla oblongata and restrain the attacks of convulsion. Large doses of the drug must however be given to produce the favourable results. In some cases it has been carried as high as a fifth of a grain twice daily, and this continued for nearly three weeks at a time not *only without its* producing the slightest sign of irritation, but on the con-

trary the most marked diminution in the frequency and violence of the attacks. The one should be commenced at one-sixteenth of a grain twice daily, and gradually increased according as it is tolerated. It is remarkable, however, that in these cases the system appears to lose its susceptibility; and the drug, even in large doses, produces some of the ordinary signs of disagreement. In no case has it yet been seen to produce mischievous excitement or irritation. (Mr. W. Tyrrell, p. 45.)

NEURALGIA.—In all cases of neuralgia there is more or less acute pain on pressure over the spinous processes of those vertebræ which correspond to the origin or point of exit of the affected nerves. When neuralgia is superficial the local application of atropine or belladonna is sufficient to relieve pain in the majority of cases. A piece of lint soaked in a solution of the salt (5 grains to three ounces) applied upon the painful part, and covered with oiled silk, is the best mode of applying the remedy. When, however, the neuralgia is more deeply seated or severe, the subcutaneous injection of morphia or atropine is necessary. (Prof. Trousseau, p. 75.)

PAIN.—*Morphia and Chloroform.*—The following combination of morphia and chloroform is said to give relief in most painful affections much more quickly and certainly than morphia alone, and to leave none of the unpleasant after-effects of opium. Dissolve one part of morphia by weight in two parts of rectified wine-vinegar, and twenty parts of rectified spirit of wine; the solution when cold being mixed with eighty parts of chloroform. One drop contains the three-hundreth part of a grain of morphia. The dose for a child is two to fifteen drops; for an adult, thirty to forty drops. (p. 276.)

PARALYSIS.—*Cold to the Spine.*—Cold applied directly to the spine is of much benefit in cases of paralysis in which there is reason to suppose that the spinal cord is in an unhealthy and excitable, though perhaps exhausted, state. The cold may be applied by means of clots dipped in iced-water, or by ice-bags. In the first case related, and this may be taken as a typical one, the disease originated by injury to the hand, and a morbid influence was thence propagated to the spine. The symptoms at the commencement of treatment consisted of paralysis involving the muscles of the tongue, throat, back, and four extremities, and associated with giddiness, headache, profuse dribbling of saliva, constipation, and scantiness of the menses. (Dr. J. Chapman, p. 59.)

RHEUMATIC PARALYSIS.—*Electricity.*—As a rule ordinary routine medical treatment has no effect upon rheumatic paralysis, and the disease progresses from bad to worse, until the use of the limb is completely lost. Yet of all the diseases which come under the treatment of the medical electrician none is more amenable to the judicious application of electricity. It will often be found that the muscles have lost their contractile power to a considerable extent. Daily, for half an hour, each muscular fibre of the paralysed muscles should be excited with the inverse current of the primary wire of the electro-magnetic battery. All the rest of the twenty-four hours a Pulvermacher band should be worn. The positive pole should be placed on the spine, and the band should then be wound round the paralysed part, and the negative pole placed on the lower part of the principal paralysed muscle and retained in position by an elastic bandage. The band should be excited twice a-day with one part vinegar and eight parts water. This treatment must be carried on without change as long as necessary; in the case from which this plan of treatment is taken three weeks was necessary. (Mr. H. Lobb, p. 66.)

Paralysis from Sciatica.—A case of severe sciatica of three months' duration is related, in which the leg was useless and the patient worn out with pain and fatigue. The positive pole of a Pulvermacher 120 element battery was applied to the lower portion of the spine by means of a wet conductor, and the foot was placed in a basin of water brought into the current by a negative pole. This current was allowed to pass for about twenty minutes, when two No. 0 bands were put on the positive pole on the lower portion of the spine, and, taking a turn round the leg, the negative pole was placed beneath the sole of the foot, the bands being excited with one part vinegar to eight parts water, and retained in position by means of an elastic bandage. This

plan of treatment was most successful: it generally is so in such cases, all that is required being the passage of a continuous galvanic current through the nerve in large quantities, (Mr. H. Lobb, p. 67.)

TETANUS.—In the present state of medical knowledge, our greatest chance of curing tetanus and tetanoid affections consists in the proper application of ice. It is of the utmost importance that the ice should not only be applied to the spine, but that a bag containing it be applied directly across the occipital region in which is contained the most important part of the cord. Everything depends upon the way in which it is applied; and the non-success of some who have tried and reported upon this plan of treatment probably depends upon their not carrying it out properly.—(Dr. J. Chapman, p. 32.)

Traumatic Tetanus.—Calabar Bean.—A case of severe tetanus is related, in which, during 36 hours, about six grains of Calabar bean were given. The patient then became half comatose; pupils considerably contracted; breathing more easy; and no tendency to opisthotonos. The muscles of the extremities became quite relaxed, and she had a feeling of sinking, as if about to die. Brandy had to be administered. Sleep followed for the first time since the commencement of the illness. Next day the patient was much depressed, pulse 120, fuller than it had been; pupils slightly contracted; breathing easier; muscles of the arms and legs quite flaccid, although unable to move them. When moved, she had still severe spasms of the muscles of the back. The medicine was continued in small doses for a short time longer, and recovery followed. "There can be no doubt that it has a wonderful effect in producing relaxation of the tetanically contracted muscles."—(Dr. A. Campbell, p. 48.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ACUPRESSURE.—Upwards of eight hundred vessels have now been stopped by acupressure by the surgeons to the Aberdeen Infirmary. These have been in all kinds of serious operations, and yet in only two has there been hemorrhage on removal of the needle. One of these was after removal of the leg in its upper third for medulary cancer; the second was the radial artery, hemorrhage from which was arrested at once by replacing the needle. This is quite conclusive as to acupressure being a perfectly reliable means of arresting hemorrhage. Dr. Pirrie says: "I am perfectly convinced that acupressure accelerates the healing of wounds; and that under its use, aided by metallic sutures, the avoidance of all dressings, and perfect rest of the wounded part, in many instances the largest wounds, after major operations will heal up without the formation of a single drop of pus." Add to these recommendations that acupressure is the easiest of performance and the quickest of all methods of arresting hemorrhage, and that the risk from pyæmia is diminished very greatly (no case having yet occurred); and impartial men will allow that the use of the ligature should be superseded almost entirely by that of the needle. The details of the various modes of applying the needle are given most fully. (Dr. W. Pirrie, Sir J. Y. Simpson, pp. 167, 173.)

Removal of the Acupressure Pins.—It is quite unnecessary to leave the pins *in situ* for any length of time. In one case the axillary artery was relieved from pressure in fifteen hours, and the spermatic arteries in two hours and a half in another case. The most remarkable case was that of a boy who removed three of the needles four hours after amputation of his thigh, one of which had compressed the femoral artery; no hemorrhage resulted, the artery being completely closed. The pins used should have glovers' points, as they are less likely to close the tissues. (Dr. Pirrie, p. 176.)

ANEURISM.—*Complete Arrest of the Current through the Sac.*—It is quite a mistake in the treatment of external aneurism to merely check the current. This plan has only succeeded in 62 per cent. of the cases in which it has been *tried, and is,* moreover, attended with much suffering to the patient. The

author has lately treated four cases by completely arresting the current in the sac; at the same time, by compressing the artery at the distal end of the aneurism, retaining the blood in the aneurismal sac so as completely to distend it. The distal pressure should precede the proximal, in order to ensure the distension of the sac. In popliteal or ante-cubital cases, distal pressure being impracticable, Mr. Hart's flexion plan may achieve the same object. He believes that cases are never cured by partial compression, and that in those cases said to be cured by that means, the pressure having been increased to the amount of complete arrest for a short time, clotting in the sac, or plugging of the artery below it, produced the cure, for the pulsation is usually reported to have suddenly ceased. In the case of popliteal aneurism the proper way of proceeding is to raise, bandage, and flex the limb, and then compress the femoral artery so as to completely arrest the circulation through it. (Dr. E. D. Mapother, p. 157.)

Aortic Aneurism.—Galvanism.—In a case of aortic aneurism threatening death by external hemorrhage, the author introduced into the sac two needles, coated the one with glass and the other with gutta-percha in the principal part of their length, and a galvanic current was by their means passed through the blood contained in the part of the sac of the aneurism external to the chest walls. No result following this operation, it was again repeated, the needles being this time coated with vulcanite, and six cells instead of four being used. In the first operation the current was continued seventeen, and in the second twenty minutes. The case ultimately proved fatal, and it was found at the post-morten examination that a considerable quantity of blood had undergone coagulation. This case seems to prove that we have a remedy by which we can at least solidify the external portions of an aneurismal sac, and that by frequent and longer-continued operations we may fill by coagulum a sac of any size. Success would depend principally upon the size of the opening into the aorta. If large, the clot formed would become free on withdrawal of the needles, and would pass on to plug up the aorta or one of its branches; but if the aperture is small, complete consolidation might be produced. (Dr. J. Duncan, p. 161.)

Cirsoid Aneurism.—Injection of Perchloride of Iron.—Cirsoid, aneurism, even of large size, may be cured by injection of the perchloride of iron. A case treated in this way was lately exhibited before the Medical Society of Vienna. The tumour had been large, and situated on the side of the head. All the arteries leading to it were much dilated. After the injection, a clot was formed, the part became hard, suppurated at points, and the tumour disappeared. (Prof. Pitha, p. 167.)

Angina Pectoris.—*Nitrite of Amyl.*—Nitrite of amyl diminishes the contractility of mucles and lessens arterial tension. This latter property has been proved both by the sphygmograph and the hæmadynamometer. In a case of very severe angina pectoris which there was lately in the clinical wards of the Edinburgh Royal Infirmary, the only treatment which afforded relief was venesection. The relief so obtained was, unfortunately, only temporary, and apparently arose from the relief to arterial tension. Chloroform also gave relief, but the symptoms recurred immediately on its discontinuance. Under these circumstances five to ten drops of the nitrite of amyl were given by inhalation, and simultaneously with the flushing of the face the pain completely disappeared, and did not return till its wonted time next night. The same remedy gives relief in cases of aortic aneurism when the pain is not constant, but intermitting. Dr. Richardson thinks it may prove useful in tetanus, and it also seems likely, from its mode of action, to be beneficial in some cases of asthma—possibly also in cholera. (Dr. T. L. Brunton, p. 84.)

Hypertrophy and Dilatation of the Right Ventricle of the Heart.—The apex beat is an indication of the state of the left ventricle, and is the only healthy impulse perceptible to the hand placed on the chest. In cases of hypertrophy and dilatation of the right ventricle, with a suppressed or a relatively diminished left apex beat, we have a diffused impulse to the right of the usual position of the apex beat, and commonly easily distinguished from the latter (even when both are present) by its diffused and heaving

character, readily appreciated by the hand strongly pressed over the costal cartilages. Sometimes there is no difficulty in detecting a proper apex beat in its usual situation, or perhaps in such a position as to indicate hypertrophy of the left ventricle; and, in addition, there is a diffused flat impulse, heaving up the whole palm of the hands, quite distinguishable from the other, and extending from the third or fourth left cartilage almost to the xiphoid cartilage, or into the epigastrium. In such a case it is to be presumed that both ventricles are very considerably enlarged; but the relative degree of enlargement of the right or left ventricle will be determined mainly by the degree to which the apex beat retains or loses its definite character. (Dr. W. T. Gairdner, p. 76.)

Ligature of the Femoral Artery in Porter's Space.—The following is the manner of securing the femoral artery in this situation:—A transverse incision, about two inches in length, parallel with, but half an inch below, Poupart's ligament; the incision being so effected as to leave the artery in its centre. The method of performing this incision is simple in the extreme. A fold of skin is pinched up—the operator himself holding one side of the fold, his assistant the other; and it is transfixed with a sharp-pointed bistoury; and, by cutting outwards, the first incision will be completed. The most ordinary care will ensure the artery's being in some portion of the wound. A very few touches of the scalpel, with the assistance of a director, will now expose the vessel; and it can be secured in any way that the operator may select. The great advantage of this operation is that, in this situation, the artery is widely separated from its vein; and, consequently, there is no danger of wounding it. The facility with which the artery is reached is also a recommendation. When the femoral artery is tied above the profunda the current of blood finds its way through the anastomoses existing between the obturator and the internal circumflex arteries; the ilio lumbar with the circumflex ilii; and others of the gluteal, sciatic, and pudic. (Dr. R. Macnamara, p. 180.)

Moveable Forceps for Closing Bleeding Blood-Vessels.—This is a new method of closing arteries after operations, by the application to the vessel of a pair of self-closing forceps, something like a *serre-fin*, but having the toothed extremities at the end of long firm shanks, which, when closed together, are but little thicker than a stout needle. Various sizes are required ranging from three inches to three-quarters or half-an-inch long. The forceps are self-holding; to open them it is only necessary to press upon the bow at the top. The points of the forceps should just embrace the vessel, while the bowed shoulder should just project out of the wound. If venous hemorrhage be troublesome there is no reason why the forceps should not be applied to the open vein, as those to be feared from a ligature, and the forceps may be removed on the completion of the operation or very shortly afterwards. By the use of these forceps all the advantages of acupressure are gained without the risk of secondary hemorrhage, which sometimes occurs and involves the opening out of the wound, and without including in the compressed portion any other tissue than the open end of the vessel. (Mr. T. Nunneley, p. 159.)

Nævus.—Whatever plan of treatment is adopted it is essential that it destroy the whole growth completely. When the nævus is prominent and cutaneous the ligature answers the purpose the best. Pass two harelip needles at right angles to each other under the nævus, and strangulate the growth by a ligature passed around it beneath the ends of the needles. The use of the needles is of course to ensure the ligature being applied beneath the base of the tumour. Prominent cutaneous nævi irregular in shape or ill-defined in their boundaries are best attacked by nitric acid, as this can be applied with a glass brush to the very smallest spots of diseased tissue without injuring the surrounding skin. "Anhydrous" acid is the best for this purpose. Vienna paste, or caustic potash, is used by many for this kind of nævus, but it is difficult to adopt them to the irregular edges and outlying portions of these growths. (Mr. T. Smith, p. 286.)

Nævi on the Face.—The treatment of nævi on the face by the injection of

perchloride of iron has more than once been followed by instant death. A case was published by Mr. Paget, of Leicester, in 1837, in which a slight convulsion and death resulted instantaneously from the injection of a small quantity of dilute liquor ammoniæ. Nævi over the fontanelle also require treating with great caution. It is always better to wait until ossification has taken place beneath the nævus before doing anything. Death from the injection of perchloride of iron results from its introduction into the general circulation, by which untoward accident coagulation of the blood in the vein and in the venous system down to the right side of the heart is effected. (Mr. T. Smith, p. 189.)

Pulse.—*A rapid pulse* does not alone necessarily indicate mischief. There are a considerable number of children and young persons whose pulses are rapid enough to frighten one. Observe whether the respirations are in the same proportion rapid; if they are not, the respirations, and not the pulse, must be our guide in judging of the patient's state. After hemorrhage we must always apply this rule of checking the indications of the pulse with those of the breathing, for the pulse is usually hastened and the breathing retarded; so that with a pulse of 120 or more there may be not 10 respirations in the minute. (Prof. Paget, p. 152.)

Pus Globules and White Corpuscles of the Blood identical.—Dr. Cohnheim, of Berlin, has made some remarkable observations proving the identity of the white corpuscles of the blood with pus globules. White blood corpuscles have the singular property of contractility, fixing finely divided substances with which they come in contact in their contractile stroma. Dr. Cohnheim, taking advantage of this, injects anyline-blue into the blood of an animal, whereby the white corpuscles become coloured, and he finds the pus globules evolved on the surface of an artificially inflamed cornea, to present the exact appearance of the coloured white corpuscles. The white blood corpuscles are in fact the cellular elements infiltrating the inflamed part. (British Med. Journal, June 22, 1867, p. 740.)

Torsion of Blood-vessels.—Mr. Syme in many operations, especially in breast cases, does without ligatures, needles, &c., to stop the vessels. He simply trusts to torsion used as follows: He seizes the vessel with the hooked forceps, or the common artery forceps, and twists it round and round till the little bit held by the forceps separates or is twisted from the vessel. (p. 128.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

Bronchial Affections and Asthma.—*Inhalation of Ammonia*—Ammonia inhaled is a remedy of considerable value in diseases of the respiratory mucous membrane, and in asthma. In the latter disease when the vital powers are depressed, it produces a feeling of warmth and exhilaration particularly grateful to the patient. The best plan of employing it is that of Dr. Lewin. The apparatus consists mainly of three bottles, as used in chemistry for the evolution of gases, one containing some liquor ammonia, and another containing some hydro-chloric acid, according to their chemical proportions, are connected with a third, which is about half filled with pure water. From the last bottle the inhaling tube projects. The patient uses the mouth-piece of the inhaling tube exactly as a pipe is used by a smoker. With each draught nascent chloride of ammonia is inhaled and carried into the smallest ramifications of the bronchial tubes. (Dr. H. Beigel, p. 98.)

Congenital Displacement of the Septum Nasi Simulating Polypus.—Congenital displacement of the septum nasi to one side so as nearly to occlude one nostril is a very common affection, and is liable to be mistaken for polypus. A careful examination will prevent this. The nose appears quite straight outside. There is usually a fœtid discharge from the occluded nostril owing to retension of the mucus; this may be cured by means of tepid water and a syringe. (Mr. T. Bryant, Lancet, Aug. 24, 1867, p. 224.)

Diseases of the Respiratory Passages.—*Inhalation of Atomised*

Fluids.—There can be no doubt but that atomised fluids when inhaled enter the respiratory tract and penetrate into the very cells of the lungs; and that therefore by means of inhalation remedies may be most appropriately and successfully applied to the organs of respiration. Dr. Beigel, of London, in a work lately published on the subject, gives woodcuts of and describes various astonishing atomising apparatus, the acting blast power of some of which is steam. Among the medicaments which can be atomised, and which in that condition have been found beneficial in diseases of the respiratory organs, are nitrate of silver, tannin, sesquichloride of iron, common salt, tincture of opium, and cod-liver oil. (Dr. H. Beigel, p. 99.)

Ozœna.—The anatomical fact that, when the mouth is opened the velum pendulum palati closes, so as to shut off the nasal cavity, may be made use of in the treatment of Ozœna, (as was first pointed out by Dr. Thudichum, see *Retrospect*, Vol. li. p. 196). The nozzle of a tube, the other end of which is placed in a vessel of water raised considerably above the patient, should be placed in his nostril—not the one which it is desired to free from matter or mucus. The tube will of course act as a syphon if filled with water previous to use, and a constant stream of water will pass in at the healthy nostril and out at the other, carrying with it any offensive discharge or mucus which may be present. (Mr. Christopher Heath, Lancet, July 13, 1867, p. 40.)

Phthisis.—*Inoculation of Animals as a Means of Diagnosis.*—For the remarkable discovery that tubercular phthisis can be inoculated from man to animals we are indebted to Dr. Villemin. It having occurred to the author that this might be made a means of diagnosis in cases of doubtful phthisis in the human subject, many experiments were made, and the following conclusions arrived at. That the inoculation of guinea-pigs with the expectoration of phthisis will, at any rate in a certain stage of the case, produce tuberculosis in the animal, and if two or more animals be inoculated at a time, and on being killed thirty days afterwards are found to be tuberculized, we may be certain that the patient is suffering from phthisis. Blood and pus as well as the expectoration seem to have the power of causing the formation of tubercles in guinea-pigs when inoculated in these animals. The spleen is the part in the guinea-pig (the most suitable animal for the experiment) which is most liable to tuberculosis. (Dr. W. Marcet, p. 91.)

Phthisis of Zymotic Disease.—The author believes that tubercle is a true zymotic disease, and that it never originates spontaneously. He considers that the tuberculous matter contains, or is, the morbific matter of the disease, and is of the nature of an eruption, and that there is reason to hope we may eventually rid ourselves entirely of this fatal scourge by the destruction of this matter on its issue from the body. Amongst the evidence on which these conclusions are based one of the most curious points is the fact that phthisis was unknown amongst the South Sea Islanders, the American Indians, and the Negroes of Africa, until they had come into intimate contact with Europeans. According to the positive assurance of Livingstone and other African travellers, phthisis is unknown in the interior of Africa, whilst it is extremely common and fatal everywhere along the African seaboard, where the blacks have come into constant and intimate relation with the whites. (Dr. W. Budd, p. 89.)

Influence of Alpine Climates on Pulmonary Consumption.—Some startling cases are related showing the beneficial influence of Alpine climates on pulmonary consumption. One patient lost his cough and gained flesh on the Peruvian Andes, and, after a subsequent relapse at New Orleans, entirely recovered his health on the table-land of Mexico. A second patient after spending a winter at Bordeaux and Cannes without entirely losing his cough, and where he still continued to lose weight, went to the Righi in Switzerland, where he lived almost entirely in the open air and drank about two quarts of milk a-day. He recovered his health perfectly so far as his own sensations went. The third case went to Davos am Platz and recovered. The treatment adopted there by Drs. Spengler and Unger, is the use of much milk and *light* nourishing food; a moderate amount of wine, principally the red wine *of the Valteline*; and graduated exercise, first on level, later up hill. The

cold douche is likewise in many cases used with advantage. It is probable that in addition to the exhilaration and increased inclination to exercise produced by the cooling, refreshing atmosphere and the complete change in the manner of living, that the increased exercise of the respiratory organs, the fuller, deeper inspirations necessitated by the rarity of the atmosphere influences the respiratory circulation and all the processes of nutrition. (Dr. H. Weber, p. 93.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

ARTIFICIAL DIGESTION OF MEAT.—There are many cases in which it is very important that nourishment should be supplied in sufficient quantity to the system, yet in which the stomach is incapable of fulfilling its function aright. Beef-tea and milk are often the only articles of diet the stomach will bear, and even the latter frequently disagrees, whilst beef-tea alone does not supply sufficient pabulum. In such cases let the meat be digested artificially as follows: Add to one pound of meat well divided, 58 grains of hydrochloric acid, sp. gr. 1·1496, in a pint of water, 15 grains of Bounalt's pepsin, and allow the whole to simmer over a water-bath at about the temperature of the body. When the meat is sufficiently broken up, it should be strained and the acid neutralized by 81 grains of bicarbonate of soda. The product is of a most agreeable character, easily digestible, and containing much more nourishment than common beef-tea. (Dr. W. Marcet, p. 108.)

DIPHTHERIA.—Dr. Alderson, President of the College of Physicians disapproves of the plan of removing the false membrane from the throat, and the application of caustics. He says: "I should deprecate the use of every irritating application in the early stage; though when that gorged state of the parts, having an œdematous character, threatening immediate suffocation, has begun to subside, a certain amount of irritating applications may be admissible and even necessary." He considers that alcoholic stimulants or ammonia are injurious. General depletion in diphtheria would be wholly out of place, but he has observed much benefit from leeches cautiously applied, so as to produce only a local depleting effect. "It is not desirable to continue the depletion longer than is called for to check the inflammatory action. The tendency in this disease to extreme debility and exhaustion is very great, but at the same time it has been most strikingly evident, in all cases which I have treated in this manner, that the rebound towards strength and convalescence succeeded immediately on relief attained through the leeches." (Dr. J. Alderson, p. 100.)

Iodine Inhalation.—The author states that he has found benefit from the inhalation of iodine in cases of diphtheria. The formula he recommends is that formerly used by Sir Charles Scudamore in pulmonary phthisis, viz.: iodide of potassium, of each four grains; alcohol, four drachms; water, four ounces. Of this for each inhalation, commencing, he takes a drachm, adds to it a pint of vinegar, infused with a handful of dried garden sage, placed in a common inhaling jar, steadily increasing the quantity of iodine solution until he arrives at half an ounce at each inhalation. (Dr. J. Waring-Curran, p. 104.)

HEMORRHOIDS.—The part to be removed is seized with a clamp, the under surface of which, *i. e.*, that next the anus, is covered with ivory, and the instrument is prevented from slipping by a screw. The hemorrhoid, grasped by the clamp, is then cut away, and the actual cautery energetically applied to the wound, the ivory preventing the conduction of the heat, and thus saving injury to the parts behind. The screw is then gradually loosened in case of hemorrhage, which, should it occur, can be easily stopped by re-tightening the screw. (Mr. H. Smith, p. 194.)

INTESTINAL WORMS.—*Substances Liable to be Mistaken for.*—A number of cases are related in which orange pulp, undigested celery, plastic lymphy, intestinal exudation, and elastic ligamentous tissue, passed from the bowel, were severally mistaken for intestinal worms or hydatids. Care in the ex-

amination or the use of the microscope will render the nature of such substances evident. (Dr. W. Frazer, p. 193.)

Obstruction of the Bowels with Obstinate Constipation.—In cases of obstinate constipation with urgent symptoms of obstruction, nothing is so reliable as a large enema of warmed oil, if the bowels cannot be relieved by simpler means. First give an ordinary enema of warm water, to which a few drops of laudanum may be added to secure its retention; then give fully three pints of warmed oil, and cause it to be retained as long as possible by pressure on the seat with a soft linen napkin. Some most interesting cases are related illustrative of this practice. (Dr. T. Head, p. 191.)

Phosphate of Soda in Small Doses.—The action of phosphate of soda is that of a mild saline aperient when administered in large doses, but it is an important alterative in small doses. It is an important salt in the animal economy, and one often sadly deficient in the food for children. Dr. Marcet has pointed out that the fatty acids are converted into an emulsion by its agency, and that they thus may become more easily assimilated. It is of much value wherever, from an unhealthy character of the motions, the visceral secretions seem to be abnormal. The parents should be directed to put a pinch of the powder into each article of food the child receives. There appears to be a similarity between its action and that sought by the administration of gray powder "in alterative doses." It is of service in cases of chalky stools or white fluid motions in infants who are being artificially reared, and who are liable to frequent derangement of the bowels. (Dr. W. Stephenson, p. 96.)

Vomiting from Sarcinæ Ventriculi.—Sulphurous acid should be made in the ordinary way, and be passed through water until the latter has dissolved up as much as it will take. This saturated solution is to be diluted until the acid is no longer unpleasant to swallow, when an ounce should be given before each meal. The sarcinæ may be thus destroyed, but are apt to return, as will other parasites, until the conditions on which their presence depends have been removed. (Dr. Reynolds, p. 108.)

AFFECTIONS OF THE URINARY ORGANS.

Bright's Disease.—The investigations of the author show that, providing the degeneration of the kidney is not fatty, a great deal can be done towards effecting a cure. The presence of albumen in small quantities (5 or 6 grains daily) still continues in the most favourable cases observed by him—but the health being good this may be disregarded. The free action of the skin must be maintained by the occasional use of the vapour bath. Congestion of the kidneys must be removed by dry cupping. Tonics and astringents should be administered, consisting chiefly of the tincture of sesquichloride of iron, sulphate of quinine, tannic and gallic acids, and when the effusion is very great, occasional doses of hydrogogue cathartics and diuretics should be given; but above and before all, great attention should be paid to the diet. This must be highly nitrogenous; milk and eggs being freely given, and meat two and even three times a day. Rest from toil and freedom from care and anxiety are absolutely necessary. (Dr. A. H. Hassall, p. 113.)

Irritable Bladder.—In a case of extreme irritability of the bladder which lately occurred at University College Hospital, no stone existed, and the urine was perfectly natural and healthy in every respect. No cause whatever could be assigned for the complaint. The patient was a man, and he was obliged to pass urine as often as four and twenty times in the twenty-four hours. Belladonna administered as suppository and given by the mouth speedily improved his condition, but he did not get well. *Alchemilla arvensis*, or parsely-piet, a common field plant, was then given, an infusion of which (one ounce to the pint) will often succeed where more pretentious remedies *have failed*. (Sir H. Thompson, p. 203.)

Lithotomy.—Sir Wm. Fergusson lately operated on two cases of very

large stone in the bladder by a modification of the operation of Celsus, making the outer cut to form a crescent or rainbow shape, the hollow of which included the rectum. The fore-finger and middle-finger of the left hand, with their dorsal surface looking up, were then directed horizontally forwards, so as at once to push back the rectum out of harm's way, and to embrace between them the bend of the staff, furnishing thus an effective guide to its groove, which the knife then entered, and completed the operation exactly as for lateral lithotomy. (p. 200.)

Mr. Lee has recently introduced a modification of the lateral operation for stone. After passing an ordinary grooved staff a straight median incision was made, and continued from just in front of the anus outward and backward so as to embrace about one-fourth of the circumference of the bowel. The scalpel was then re-introduced in the median line, with its back towards the rectum; it was then passed forwards into the groove of the staff, and the membranous portion of the urethra having been opened it was withdrawn, and a curved bistoury with a projecting probe-point introduced. The probe-point was then made to slide along the groove into the bladder, the edge of the knife being held to the operator's right side, as in Buchanan's operation. The heel of the knife was then made to describe a portion of a circle corresponding to the external incisions, while the point, while being withdrawn, was moved little from the median plane. The operation was then complete. (Mr. H. Lee, p. 202.)

LITHOTRITY.—The size and form of the stone ought always to be determined before deciding which instrument to use. For this purpose the kind of sound to be used is by no means a matter of indifference. Many surgeons still use a sound having the form of an ordinary catheter. Such an instrument does not admit of much lateral movement in the cavity of the bladder, while rotation is impossible. Hence it often fails to detect a calculus. An exploring sound should have a short beak only, which can be moved in every direction with facility. By having the handle of this sound marked in eighths of an inch, upon which a small sliding ferrule is placed, we may with ease measure the exact size of the stone. The surgeon having struck the stone, moves the beak of the sound to the further end of it, and slightly taps its extremity. The sliding ferrule is then slipped down the stem till it touches the external meatus. He then slowly draws out the sound until it reaches the nearer end of the stone, and observes how much of the stem has issued from the urethral meatus, during the progress of the beak over the stone. The fact of the existence of renal disease is no bar to the performance of lithotrity. (Sir H. Thompson, p. 196.)

All the dangers and difficulties occurring to the patient in lithotrity may be broadly referred to one cause, viz., mechanical injury to the bladder and urethra. The sources of this injury are two: the stone itself, and the instrument employed to remove it. It is well to use a large flexible bougie on two or three occasions before determining on crushing the stone, in order to accustom the urethra to the passage of an instrument. If fever is easily aroused by so simple a procedure, the question of performing lithotrity must be carefully considered. At the first sitting the stone should be seized not more than twice (with the fenestrated instrument only when large) and merely broken into several pieces. In order that the fragments may become a little water-worn, and their edges less sharp, the patient should be kept in bed and admonished only to pass water for the next day or two when lying upon his back. The lithotrite should rarely remain at any sitting more than two minutes in the bladder. One minute usually suffices for three or four efficient crushings. Of course this is only possible with an instrument the construction of which admits of instantaneous change from screw to sliding movements, and *vice versa*; and for successful lithotrity this is absolutely necessary. We should diminish as much as possible the use of all instruments not absolutely necessary. For by far the greater number of cases a simple flat-bladed lithotrite is sufficient. Few matters are of more importance than the removal of the last fragment. There is sometimes uncertainty whether the symptoms are owing to a small fragment remaining, or to a slightly inflamed

condition of the bladder. The best plan is to carefully explore once or twice with the sound, and the small beaked lithotrite, the blades of which are easily reversed and can be made to traverse the floor of the bladder. If nothing is discovered let the patient have a week or two's freedom from instruments, and during that time he should on two or three occasions take a rough drive in an omnibus or other jolting vehicle. Nothing more certainly increases the irritation if it is due to a fragment, while the effect will probably be very slight if none is present. (Sir H. Thompson, p. 203.)

Stone in the Bladder in the Adult.—Sir Henry Thompson gives a resume of 100 cases of stone in the bladder, 84 of which were operated on by lithotrity and 16 by lithotomy. The mean age of the lithotrity cases was sixty-two and a-half and yet the mortality was only five per cent. of the whole He says, however, that lithotrity can be only thus successful when it is performed on a definite system, in accordance with certain practical rules which experience has determined, and which can be laid down. By exercising an ordinary degree of vigilance for adult patients suffering from symptoms of urinary disorder, every case of calculus may be discovered in an early stage, may be successfully treated by lithotrity, and consequently the operation of cutting for stone may be rendered obsolete, and applicable only for some very exceptional example which has been developed as the result of extreme neglect or ignorance. (Sir H. Thompson, p. 194.)

Stricture near the Urethral Orifice.—Strictures about two inches from the orifice of the urethra cannot be treated by dilatation; it is perfectly useless except early in the case. There is so much fibrous tissue and so little of the spongy texture in old strictures, that the only plan is to divide them by means of a urethrotome after dilating them, if necessary, so as to admit of the introduction of that instrument. Nearer the bulb dilatation alone is the proper plan of treatment. (Sir H. Thompson, Medical Times and Gazette, July 27, 1867, p. 92.)

Varicocele.—The following is the plan of operating lately pursued at University College Hospital. A needle threaded with silver wire was passed between the enlarged vessels and the vas deferens, which were first separated a little from each other. The needle being withdrawn the loop of wire was left. The needle with a fresh loop of wire, was again introduced from the opposite side, but instead of passing behind the swollen vessels it was passed in front of them. It was again withdrawn and the loop left. A loop and two free ends of wire now projected at each side, and the ends being passed through their corresponding loops and drawn tight, the vessels were effectually strangulated. When ready for removal, a little pushing easily separated the wires. (Mr. Marshall, p. 206.)

FRACTURES, DISLOCATIONS, AND DISEASES OF BONES AND JOINTS.

Abscess.—*Antiseptic Treatment.*—In the treatment of abscess our great aim must be to prevent the introduction of those living particles of matter contained in the atmosphere, and which cause septic changes in the interior of the pyogenic cavity. A solution of one-part of crystallised carbolic acid in four parts of boiled linseed oil having been prepared, a piece of rag from four to six inches square is dipped in the oily mixture and laid upon the skin where the incision is to be made. The abscess may then be opened beneath the raised lower edge of the limb, which must be allowed to drop immediately the knife is withdrawn. The lint forms an antiseptic curtain beneath which the pus flows out into a vessel placed to receive it. If necessary a piece of lint soaked in the antiseptic oil may be introduced into the incision to check hemorrhage or to prevent primary adhesion, which is otherwise very apt to occur. This plan, however, requires modification during the remainder of the treatment. We require an antiseptic dressing which will effectually prevent the decomposition of the stream of pus continually flowing from the wound. For this purpose a little of the above mentioned solution of carbolic

acid in linseed oil should be mixed up with common whiting (carbonate of lime) to the consistence of a firm paste. This must be spread upon a piece of sheet block tin about six inches square; or common tin-foil will answer equally well if strengthened by adhesive plaster. This should be applied over the abscess and retained in position by adhesive plaster. The dressing requires changing every twenty-four hours. The result of this treatment is that abscesses do not secrete any more pus after their original contents have been evacuated, the discharge being merely serum. (Prof. Lister, p. 115.)

Antiseptic Treatment of Wounds and Abscesses.—The necessity for an antiseptic treatment of wounds and abscesses is shown by the fact that, if iodine is put within them it is absorbed so rapidly that it is detected in the urine and saliva in a few minutes. If iodine is absorbed, why not the natural contents of the abscess? (M. Demarquay, p. 151.)

Adhesion of Cicatrices to Bone.—It occasionally happens that great pain is felt in a cicatrix following amputation or other operation on the leg. This pain, however, is not confined to the cicatrix, but is referred to the whole of the surrounding parts, and it will not be discovered that the adherent portion of cicatrix is the seat of the pain unless the part is carefully examined. The proper plan is to divide the adhesion between the cicatrix and the periosteum subcutaneously, and then, by daily moving the skin over the bone, to prevent their reunion. One case is related in which a conical stump was forming apparently owing to the constant wearing pain from this cause, and the removal of which, by the treatment recommended, was followed by proper deposition of fat in the stump, as in other parts of the body. (Mr. H. Hancock, p. 143.)

Bony Anchylosis.—To ascertain whether anchylosis of a joint is bony or not, attempt to straighten the limb, and there will be no contractions in the muscles around the joint if the parts are immoveably fixed, whereas, if the anchylosis is only fibrous the surrounding tendons are felt to spring from their sheaths. (Mr. H. Coote, p. 147.)

Diseased Bones and Joints.—In the lower classes the treatment of diseased joints by rest is in most cases a failure, disease spreading from bone to bone in the smaller articulations, and some organic affection attacking the liver or some other internal organ before the consolidation of a large joint can be effected. The author states that he has met with great success by cutting down at once upon the diseased bone, even at the commencement of the affection, and after piercing the compact tissue by means of a trochar applying potassa fusa c. calce freely to the diseased bone, and along the track of the trochar. This treatment is most commonly called for in cases of congestive inflammation of the head of a long bone (which would terminate in disease of the joint). In less acute cases a small eschar may be first made, the centre of which being incised, the caustic can be introduced, and by combining its action with the knife, the tunnel be carried deeper, from day to day, in a gradual manner. The same treatment is to be recommended in the advanced period of the case when caries is established, or when a whole joint is converted into a foul suppurating cavity, but it is not to be advised in the intermediate period, when the head of the bone is the seat of a diffused suppuration. The thing required in these cases is to put an end to the imprisonment of the diseased bone. (Dr. F. Kirkpatrick, p. 148.)

Dislocation of the Lower Jaw.—It is necessary for the reduction of this dislocation that the pterygoids, masseters, and temporals should be relaxed, as being the chief or opposing forces in its reduction. During the voluntary act of depressing the lower jaw these muscles are relaxed, therefore, just as you press downwards with the thumbs placed on the crowns of the posterior molars cause the patient to open the mouth a little. By this means the slightest pressure will be sufficient to disengage the necks of the condyles from the transverse roots of the zygomas, and will permit the temporals and masseters to at once reduce the dislocation by the reinduction of their normal action. (Mr. D. Kelly, p. 153.)

Dislocation of the Hip-Joint.—Supposing the dislocation to be one of the left side and upon the dorsum ilii, the following is the mode of procedure:

Place the patient on a mattress laid on the floor, and put him completely under the influence of chloroform until the muscular system is thoroughly relaxed. The pelvis being steadied, seize the left ankle with the right hand, at the same time grasping the corresponding knee with the left hand. Flex the leg to its complete extent upon the thigh, and the thigh upon the abdomen. With the limb in this position the knee and foot must be made to take a circular sweep outwards as far as they will go, and then the whole limb must be suddenly extended. This movement causes the head of the bone to slip into the acetabulum at once with a distinct jerk. In some of the dislocations of the hip-joint the directions of the circular sweep should be made inwards, and a little rotation of the limb often assists the head of the bone into its place. (Mr. T. Annandale, p. 152.)

Excision of the Ankle-Joint.—Hitherto, in cases of disease of the ankle-joint incurable except by removal, the whole foot has been removed either by amputation at the ankle or by Syme's or Pirogoff's operations. There is no reason why the ankle-joint should not be excised as is done in similar disease of the knee or elbow, and so the foot be preserved. In Syme's operation, independently of the entire loss of the foot, there is danger of sloughing of the flaps, of bagging of matter, and in both Syme's and Pirogoff's, of inflammation, suppuration, and sloughing in the course of the divided tendons. In excision of the ankle-joint these dangers do not exist. Mr. Hancock, of Charing Cross Hospital, has now performed this operation five times—four times successfully, once unsuccessfully, the patient dying some six months after the operation from lung disease, the result of a life of dissipation. In no instance was there sloughing. There need not be a single artery or tendon divided. (Mr. H. Hancock, p. 137.)

One semilunar incision, extending across the front of the joint to behind the malleoli on either side and penetrating only to the fascia below, is the best way of commencing the operation. When this flap of skin is reflected we have plenty of room to see what we are about, and can avoid wounding the posterior and anterior tibial arteries, a point of great importance. If we meddle with an ankle-joint it is better to remove the entire joint, no matter how limited the disease may seem to be; when this plan has not been followed, the results have generally been very unsatisfactory. When practicable the bones should always be removed by the saw, and the employment of the gouge avoided as much as possible. With the gouge we inflict an amount of violence, bruising, and crushing, which the parts already weakened by the pre-existing mischief cannot repair, and thus the operation fails. (Mr. H. Hancock, p. 139.)

Fractures.—*Elastic Extension.*—The use of elastic extension is recommended in the treatment of fractures of the lower extremities. The means by which this is accomplished will best be understood by referring to the author's description of his splint. This plan seems to promise advantages in certain cases, especially when much spasmodic muscular contraction has to be overcome. In cases of fracture of the thigh with shortening, the limb will be found to have gained its proper length in two or three days. The extension may be increased or diminished according to circumstances. (Mr. H. Worthington, p. 129.)

Ununited Fractures.—Dr. Bigelow, of Harvard University, has been very successful in the treatment of ununited fracture. His plan of treatment is as follows: The false joint having been fairly exposed by a free incision in the part most free from important nerves and muscles, the ends must be turned out by flexing the joint and dividing any connecting material. The callus over-lying the periosteum at the tips, having been then incised by a crucial or other regular incision, should then be seized by a strong-toothed forceps, and efforts made to tear it out of the rugous inequalities of the formerly inflamed bone. When the bone is reached, the periosteum must be stripped from both ends as far up as the intended section. The ends of the bones must now be sawn off. For the purpose of inserting the wire, holes *must be* bored in each extremity with a good bone drill, at a little more than *half an inch from* the end, and through one wall only. The wire must then

be inserted from without inwards in one end, and inversely in the other, and the ends drawn accurately together, and the wire twisted and allowed to protrude at the external wound. The parts must be put up in properly-made splints so as to preclude all motion. When union has taken place, the wire must be removed by division of the loop by cutting-pliers, and be forcibly drawn out. It should generally be left in position several months. (p. 135.)

Fractures of the Femur.—We are all familiar with the advantages of suspension in cases of fracture of the leg, but not with the same principle of treatment in cases of fracture of the femur. Mr. Paget has almost done away with the use of the long splint in fractures of the thigh at St. Bartholomew's Hospital, and by a simple and effectual plan both suspends the limb and applies extension at the same time, thus getting rid of the irksomeness of the restraint and the objectionable perineal band incident to the older plan of treatment. The frame-work of the apparatus, or we may call it the splint itself, is two pieces of No. 2 iron wire passing down on either side of the thigh and leg, from the trochanter externally and the pubes internally to about six inches beyond the foot at each side of it. These are bent at the knee, and the nearer the fracture to that joint, the smaller the angle should be. The limb however should in no case be much flexed. The leg and foot are pretty firmly bound to these side splints, but from two inches below the knee to the upper end of the iron wire, short pieces of bandage are applied on the under side of the limb only, running from one wire to the other, and forming a cradle for the limb carefully adapted to its shape. The upper surface of the thigh is therefore uncovered and open to inspection. By means of four hooks on the side wires or splints, two about the centres of the thigh pieces, and two about the centres of the leg pieces, and by three small pullies, the whole limb is suspended by one piece of whipcord which is attached about seven feet above the bed to a point a little in advance of the foot of the patient. By this means there is a constant extension kept up. If there is much tendency for the patient to slip down in bed, by placing small pieces of wood underneath the bottom legs we can slightly elevate the lower portion of the bed. This plan has been found very successful, and productive of great comfort to the patient. (St. Bartholomew's Hospital Reports, Vol. iii., p. 387.)

Fractures of the Neck of the Thigh-Bone.—The late Mr. J. R. Hodgson, of Chesham, Bucks, was very successful in his treatment of cases of fracture of the neck of the thigh bone, as is attested by the specimens in Guy's Hospital Museum. His success may be attributed to his not disturbing the impacted fragments. The treatment in these cases was the use of the double-inclined plane. The plane was made of pillows sewed together; the foot was fixed to the sacking at the foot of the bed by a handkerchief folded round the ankle and over the instep, and tied to the sacking. The thigh and leg were fixed by shawls folded obliquely round the limb, above and below the ham, and secured to the under pillow of the inclined plane. This was maintained for fourteen or sixteen weeks. (Mr. S. Solly, p. 132.)

Fractures of the Leg.—Division of the Tendo Achillis.—Mr. Paget almost invariably divides the tendo achillis in cases of oblique fractures of the tibia and fibula, and in those near the ankle-joint with much displacement, and *always* in compound fractures. (St. Bartholomew's Hospital Reports, Vol. iii., p. 392.)

Pathology of Joint Disease.—There are but two structures in a joint which admit of primary inflammation—namely, the synovial membrane, and the articular extremity of the head of the bone. Ulceration of the articular cartilage is always a secondary affection, and depends upon a morbid condition of the cancellous tissue of the bone. In this inflammatory condition there is a layer of exquisitely sensitive granulations interposed between the painful and tender bone and the loosened cartilage. The joint is necessarily bent in order to relieve the pressure of the surfaces against each other. During the acute stage, no attempt at extension of the joint is admissible. (Mr. H. Coote, p. 147.)

Sleep after Operations.—Capacity for sleep after an operation is an

excellent sign; such a patient will get through where another would not. (Mr. J. Paget, Lancet, Aug. 24, 1867, p. 220.)

Sub-Periosteal Resection of Bone.—In resection of bones, especially of the foot, it is a good plan to separate the periosteum along with the soft parts, 1st. Because the bone to be removed can in this way be thoroughly exposed; 2nd. Because the blood-vessels of the reflected periosteum, and flap or flaps of soft textures, are not in any way injured, except along the line of the incisions. In separating the periosteum from the bone, it is of great consequence to save as much as possible of its inner layer, as the experiments and researches of Ollier have shown that this is the chief bone-producing portion. (Mr. T. Annandale, p. 145.)

Sub-Periosteal Resection of the Os Calcis.—The following were the steps of an operation lately performed by Mr. Annandale, of Edinburgh, who has introduced this plan of resection of bones. An incision was first made along the outer aspect of the bone, extending to the articulation between the os calcis and cuboid in front. Two short incisions were also made at right angles to and at each end of the first incision, the anterior one of which corresponded to the direction of the joint between the os calcis and cuboid, and the posterior one to the course of the tendo achillis. The incisions were made quite down to the bone; and then, with one of Langenbeck's instruments the periosteum was scraped off and separated together with the flap of soft textures from the entire surface of the bone. By extending the anterior short incision a little the flap was easily turned downwards and inwards, and the bone was then detached and removed. The flap being re-adjusted, a gutta-percha splint was applied to the anterior aspect of the leg and foot, so as to keep the parts at rest. During the operation no artery required to be ligatured. (Mr. T. Annandale, p. 145.)

Union of Wounds by the First Intention.—It having been proved conclusively that the non-union of incised wounds by the first intention is owing to the putrefactive changes in the blood and other discharges, it follows that by the prevention of such change by exclusion of atmospheric air we procure rapid healing of wounds and sores. A mixture of one part of carbolic acid with five of linseed oil by means of a piece of lint soaked in the liquid, is a good application to a wound. The success which is met with in the treatment of deep wounds by this plan renders it admissible to arrest arterial hemorrhage by torsion of the vessels. This mode of arresting hemorrhage is quite safe in the case of all arteries except the very largest. (Prof. Syme, p. 128.)

Wounds.—*Carbolic Acid.*—All the local inflammatory mischief and general febrile disturbance which follow severe injuries, are due to the irritating and poisoning influence of decomposing blood or sloughs. This decomposition depends upon the presence in the atmosphere of minute floating particles of organic matter, which owe their energy to their vitality. Carbolic acid has such a peculiarly destructive influence upon low forms of life, that by its application to a wound in a suitable manner, any decomposition is entirely prevented, and even the most dangerous wounds, as compound fractures, will heal without formation of pus. In treating any wound, first introduce the acid in full strength into all accessible recesses, by means of a piece of rag held in dressing forceps and dipped in the liquid. The compound formed by the action of the acid on the tissues need occasion no anxiety, as it is disposed of by absorption and organization. The next step is to lay on the wound, overwrapping it well on each side, a piece of lint soaked in a mixture of one part of carbolic acid and four of boiled linseed oil. This must be maintained permanently in contact with the wound, and never raised when the dressing above it (to be described) is changed. A paste made of common whitening and the carbolic acid and linseed oil, and of the consistence of putty, must be rolled out between two pieces of thin calico to a layer a quarter of an inch thick, and laid on the wound over the lint. This dressing must be changed daily so long as any discharge continues. It serves as a reservoir of the carbolic acid, and constantly maintains the subjacent lint in an antiseptic condition. It is a good plan to cover the dressing with a piece of thin sheet-lead,

or tinfoil strengthened with adhesive plaster, in order to prevent rapid evaporation of the acid. The results obtained are frequently surprising, serious wounds healing without the formation of any pus, and compound fractures uniting and the wound healing without any unfavourable symptoms whatever. (Prof. J. Lister, p. 118.)

Dr. Wolfe, of Aberdeen, whose experiments and published article on the subject of carbolic acid preceded those of Professor Lister, does not believe in the pan-spermatism hypothesis of M. Pasteur. He considers that the action of carbolic acid is due entirely to its antiseptic properties, that is, of preserving organizable matter from passing into a putrescent condition. (Dr. J. R. Wolfe, p. 124.)

Chloride of Zinc.—Chloride of zinc is constantly applied to recent wounds at King's College Hospital. It is kept in solution of moderate strength, and all the deep parts of a wound, as well as the more superficial, are well mopped out with it. This plan was first suggested by Mr. De Morgan, and especially recommended by him to be employed after removal of cancerous tumours, in order to thoroughly destroy any germs of the disease remaining in the wound. It is found to prevent much suppuration, and promote early healing of the parts. (Mr. De Morgan, Mr. J. Wood, p. 126.)

Sulphurous Acid as Applied to Wounds and Sores.—Sulphurous acid is superior in efficiency to carbolic acid, and is entirely free from the objection applicable to the latter, viz., of being a powerful irritant and having a disgusting smell. A case is related by Dr. Dewar, of Kirkaldy, in which the raw surface left after the amputation of the breast was carefully sponged with sulphurous acid, and the edges adjusted by four silver sutures. A piece of lint soaked in the liquid was laid over the wound, and this covered with gutta-percha, the dressing being changed every six hours. There was never any pain in the wound, union of which was complete in twelve hours. (Dr. J. Dewar, p. 127.)

AFFECTIONS OF THE SKIN, ETC.

Carbuncle.—*Permanganate of Potash.*—In cases of carbuncle, great benefit will be found to result from dressing the swelling with lint saturated with a strong solution of permanganate of potash (3 ss. to f. ℥j.), and covered with oiled silk. A free incision is necessary first. (Dr. T. L. Leavitt, p. 210.)

Eczema.—*Iodide of Lead.*—Iodide of lead is a remedy of great value in eczema. It should be applied in the form of an ointment, twelve grains to the ounce, with one drachm of glycerine, and forty minims of chloroform, to relieve the itching. Another formula is the following: Iodide of lead, twenty grains; simple ointment, seven drachms; glycerine, one drachm. The ointment of iodide of lead of the present pharmacopœia is too strong for cases of chronic eczema or psoriasis; it contains sixty-two grains to the ounce; whereas, from a fifth to a fourth of that quantity is sufficient, and more useful than the pharmacopœial strength. The use of constitutional treatment must be combined with this. (Dr. W. T. Belcher, p. 206.)

Favus and Tinea Circinata.—*Identity of the Fungi of.*—The opinion that the fungi of these diseases are identical is that of Dr. Tilbury Fox. It is confirmed strongly by some observations of Mr. John M. Purser, of Dublin. A cat having a patch of apparently favus on the paw, communicated tinea circinata to a female. The fungus of the cat was achorion, that of the woman trichophyton. (p. 207.)

Hypertrophied Cervical Glands.—*Injection of Iodine.*—Dr. Marston, of Devizes, relates a case of hypertrophied cervical gland, the integuments covering which had ulcerated, leaving the diseased growth projecting, and of course of most unsightly appearance. By means of a Wood's subcutaneous injection syringe, he injected tincture of iodine into the centre of the tumour until it exuded from all parts of its surface. Neither pain nor inflammation followed, and in a week the tumour had diminished to nearly half its original size. The injection was repeated, and after the third operation the growth

entirely disappeared, and the wound healed, leaving an almost imperceptible scar. This treatment seems deserving of trial in that numerous class of cases in which the skin remains unbroken, and which yet resist ordinary treatment. (Medical Times and Gazette, July 27, 1867, p. 87.)

Scabies.—*Sulphuret of Calcium.*—The following is the best way of using the sulphuret of calcium. Quick lime, one pound; spring water, a sufficiency; sulphur, two pounds; water, twenty pounds. Boil to twelve pounds. The patient is placed in a tepid bath for half an hour; then, on leaving it, all the parts affected are rubbed with a piece of flannel dipped in this solution. The patient is then placed in a second tepid bath for another half hour. The same measures are repeated next day. (M. Vleminckx, p. 211.)

Tumours.—*Electricity.*—If electricity or galvanism is to be useful in the dispersion of tumours, it must not be applied with the object of decomposing them into their elements, but of modifying their nutrition. For this purpose a simple voltaic pile composed of from twelve to sixteen pairs of zinc and copper plates, one and a half inch square, with intervening pieces of felt, is sufficient. This battery can be rendered sufficiently active by immersion for a moment in salt and water. It is necessary that the negative pole should be placed over the tumour, never the positive, and the enlargement must be covered with a plate of copper exactly fitting it. The positive pole may be connected in a similar manner with some neighbouring portion of the body. A case is related in which a gland enlarged from secondary cancer to the size of a walnut, was reduced "level with the surface" of the neck. Other cases equally encouraging are related. (Mr. M. H. Collis, Medical Press and Circular, July 24, 1867, p. 80.)

Vascular Growths.—*Electricity.*—Vascular growths, such as nævus, papillary growths, and molluscum, may be destroyed by passing through them an electric current for a length of time varying with the size of the growth and the strength of the current. The negative pole (that connected with the zinc end of the battery) is the one to be applied to the growth, whilst the positive pole may be applied to some neighbouring part. In a case of nævus a needle was passed into the right half of the tumour and connected with the negative pole of ten cells of the battery, the current was then allowed to pass for two minutes, after which the needle was withdrawn. The right half of the tumour appeared shrunk and shrivelled up. Subsequently the other half was similarly treated, and the whole tumour was completely obliterated. (Dr. Althaus, British Medical Journal, May 11, 1867, p. 538.)

VENEREAL AFFECTIONS.

Gleet.—*Oil of Sandal-Wood.*—Nineteen cases of gleet were treated with oil of sandal-wood. Thirteen cases received marked benefit, six none, and in four of these unpleasant symptoms, such as nausea, were produced by the smallest dose. Some stomachs tolerate oil of sandal-wood which cannot bear copaiba. The average dose given was fifteen drops three times a day, with a little liq. potassæ in peppermint water. (Mr. B. Hill, p. 213.)

Gonorrhœa.—*Starch Injections.*—Finely powdered starch mixed with lukewarm water, so as to obtain a fluid of the substance of cream, but thin enough to allow of injection, forms a most successful injection in cases of gonorrhœa, especially after the inflammatory stage is over. (M. Luc, p. 214.)

Syphilitic Ulceration of the Throat.—*Sulphurous Acid Spray.*—A spray of sulphurous acid ejected upon the throat will give great relief in cases of syphilitic ulceration of that part. In a case in which it was so employed at the Royal Free Hospital, the smell and bad taste which had been a source of great annoyance ceased immediately, and the unhealthy aspect of the sore gave place quickly to signs of healing. (Dr. Murchison, p. 214.)

Syphilization.—Syphilization has been placed well and impartially on trial at the Lock Hospital in London, and the conclusion come to is that it is not a plan of treatment which should be recommended for adoption.

Even admitting that it has the advantages claimed for it by its advocates (which it has not), the surgeons of the above hospital are of opinion that these are quite insufficient to compensate for the loss of time, personal discomfort, and indelible traces which it entails upon the patient. (Medical Times and Gazette, July 27, 1867, p. 105.)

Venereal Disease.—There is but one venereal virus, the action of which is usually chronic, and always ulcerative. The character of the sore depends upon the tissue upon which it is situated. The Venereal Commission appointed by the Admiralty has come to a different conclusion to the above, which is the opinion of Mr. Holmes Coote. (p. 212.)

AFFECTIONS OF THE EYE AND EAR.

Artificial Pupil.—The question often arises as to whether an artificial pupil should be made in one eye while the other is sound. This may be answered decidedly in the negative, except the pupil can be made in the centre of the iris, the lens being present or absent. If otherwise, there will be confusion of sight, double vision, or squint. It is singular, however, and unaccountable that a dissimilarity in the positions of the pupil is not invariably followed by this disturbance of vision. Supposing however that a central pupil can be made, the lens is always absent, and minute sight must consequently always depend on the use of glasses of a high power, commonly called cataract glasses. The adjusting power of the eye is gone, and for seeing at different distances glasses of different foci are required. But, practically, an eye cannot be fitted with glasses and brought up to a state to match the other. Therefore arises this important question, on which the whole matter hinges: Will this kind of sight be really of material advantage? The answer is, decidedly, Yes. We should only be deterred from operating by the probability of the eye being too much damaged to afford that amount of sight known as "useful." (Mr. H. Walton, p. 223.)

Divergent Squint.—The author operates for divergent squint, not by dividing the insertion of the external rectus, but by bringing forward the insertion of the internal rectus. The insertion of the latter muscle being reached in the usual way, a hook carrying a ligature is passed under it, and the ligature tightened on the end of the muscle. The succeeding steps are—a vertical incision in conjunctiva; traction on and loosening of the muscle by means of the ligature; passage of two ligatures through the muscle perpendicular to its plane, and situated at a variable distance from the ligature according to the amount of previous divergence; removal of the first ligature by cutting off the end of the muscle; fixation of the shortened muscle, by passing the ligatures one above and one below the cornea, to a point corresponding to the insertion of the superior rectus, and between the conjunctiva and sclerotic. (Dr. C. R. Agnew, p. 228.)

Membrana Tympani.—*New Mode of Examining.*—The membrana tympani may be easily illuminated and examined by means of a biconvex lens of 3¼ inches focal length, which must be held at such a distance that a cone of light is directed on the surface of the membrane. This has the effect of strongly illuminating the membrane itself, and at the same time keeping the surrounding parts in comparative darkness. For this purpose the lamp or other artificial light should be four feet from the head of the patient, and behind the head of the observer. When the meatus is sufficiently large in calibre, the speculum can of course be dispensed with. (Mr. W. S. Watson, p. 232.)

Opacity of the Cornea.—Drop a little sulphate of soda in fine powder into the eye of the patient. It has the effect of causing absorption of the opaque material. This plan of treatment was suggested by the fact that sulphate of soda has the property of retaining the fibrine of blood in solution. (M. De Luca, p. 229.)

Soft Cataract.—Mr. Greenway, of Plymouth, who has had considerable

experience in the extraction of soft cataract by suction, describes a suction curette which he considers an improvement on that generally employed. It is important that within the mouthpiece there should be a valve made of extremely thin india-rubber (oiled-silk will not answer), which is opened with the slightest amount of suction, and is self-acting in closing. This prevents an accidental back current of air, a very important point when the eye is the point being operated on. It also serves the purpose of a maintaining power for holding whatever has been sucked into, or laid hold of, by the canula. (Mr. H. Greenway, p. 221.)

MIDWIFERY, AND THE DISEASES OF WOMEN, ETC.

CANCER OF BREAST.—*The Acetic Acid Cure.*—A case of cancer of the breast is related in which Dr. Broadbent's acetic acid treatment was tried. The tumour was hard and rather deeply seated, and rapdly increasing in size. The acid employed was of the strength of one in four. It was most energetically used both by means of a Wood's syringe, and a small trochar. In three weeks' time, the report is "The disease spread rapidly, soon involving the whole breast, and ulcerating through the skin at one or two points of it, so as to leave me much too little skin to cover the wound, when, after the failure of the injection I felt compelled to remove the tumour." The wound did not heal satisfactorily, a small ulcerated surface being left, and this rapidly relapsing into a diseased state. A dressing of lint soaked in dilute acetic acid was applied constantly, but the disease spread rapidly till the whole cicatrix was involved in it. The patient ultimately sank with cancerous disease of the liver and intestines. (Dr. M'Gregor, p. 35.)

CEPHALOTRIBE.—The cephalotribe should be used in all cases of embryotomy in preference to all crotchets, hooks, and craniotomy-forceps. The use of it would reduce the dangers of embryotomy to a minimum, and allow its performance in cases where delivery could not otherwise be accomplished except by Cæsarian section. In one instance in which it was used there was but one inch and three quarters in the antero-posterior diameter of the pelvis, and yet the mother was delivered safely and made a good recovery. For the information of those who have not seen one of these instruments, we may describe it as a most powerful two-bladed forceps, the blades nearly two inches in width, solid, and toothed. By means of a screw in the handle, they may be made to crush bone, or simply to act as prehensile forceps for the purpose of extraction. Simpson's cephalotribe has a pelvic curve. Dr. Kidd, who has had much experience in the use of this instrument, has adopted a straight blade. He believes this has the advantage of permitting the head to be rotated in a smaller space than with a curved blade. This is of great importance in removing the crushed head of the child. It is also easier to guide a straight instrument than a curved one. (Dr. J. B. Hicks, Dr. G. H. Kidd, pp. 254, 255.)

Sir James Simpson prefers a curved cephalotribe, and states that the advantage is that it will not slip where a straight pair of blades will. The cephalotribe with teeth for fixing the scalp of the child, he regards as a great improvement. (p. 255.)

CHLOROFORM IN LABOUR.—In a small work "On Safe Delivery from the Pains of Labour," the author recommends, for the purpose of procuring "painless parturition with full mental consciousness," one part of eau-de-Cologne with two of chloroform. (Mr. I. Brown, p. 238.)

EMBOLISM AND THROMBOSIS.—*Death from Pulmonary Obstruction following Labour.*—A case at King's College Hospital is related in which, on the fifth day after labour, sudden collapse came on, with urgent dyspnœa. Death rapidly ensued, and the pulmonary arteries were found to contain dark-red, soft, unadherent clots. Both cavities of the heart were empty. No clots were found in the uterine, ovarian, or femoral veins. The uterus itself presented nothing unusual. When death occurs from primary coagulation of the *blood in the* vessels of the lungs it is generally within a moderate time after

delivery. There are other cases in which the fatal issue is preceded by well marked symptoms of phlegmasia dolens, or crural phlebitis, or coagulation of the blood in some of the veins of the lower extremities. No sooner is the fibrin deposited than it begins to undergo certain changes consisting chiefly in a retrograde metamorphosis, which is generally either an amylaceous or fatty degeneration. The result is that the coagulum becomes softened; and thus some accidental cause gives rise to the detachment of a portion, which being carried to the right side of the heart and thence to the pulmonary artery, at once produces the phenomena of embolism. Death from this cause rarely occurs before three weeks from the time of delivery. When thrombosis occurs after labour, in the large majority of instances the patient has been in a very feeble and exhausted state from flooding, severe labour, or other causes. (Dr. W. S. Playfair, p. 234.)

Midwifery Forceps.—The rules laid down by most writers on the use of the midwifery forceps, as Denman, Robert Lee, Churchill, Meigs, and Ramsbotham, are far too restricted. It is not necessary to wait six, four, or even one hour, so long as we are satisfied that the labour if left to nature will be tedious and protracted: a point which it must be admitted requires much judgment and experience to determine. The rule should be discarded that before the forceps are applied the ear should be felt, neither is it always necessary to wait until the os uteri is fully dilated; for with ordinary care the blades may be passed beyond the os uteri without inflicting the least injury, and with properly constructed forceps the os uteri is not stretched until the head itself is brought down to assist in the dilatation in the natural way. Even where the fœtus is dead, extraction by the forceps is preferable to craniotomy, as being safer to the mother and less repulsive to her feelings and those of her friends. (Mr. A. B. Steele, p. 238.)

It is generally supposed that the hold of the forceps upon the head depends upon the grasp of the accoucheur on the handles of the instrument. This is not the case. The hold is nearly as firm when the handles do not cross at all. Mattei, of Paris, has made an instrument whose parallel blades are set in a cross-bar of wood to serve for traction. In the case of the short handled forceps there is little or no compressive power. The hold is really due to the curvature of the blades, which fit more or less accurately upon the globular head, and the compression of the bows of the blades against the soft parts of the mother, supported by the bony ring of the pelvis. The greater the curve of the forceps blades and the wider the fenestræ the better they hold. Those blades which, when in apposition, approach parallelism are more liable to slip. In the French and Continental forceps the blades when in apposition form nearly a circle, and the points approach nearer than in the English patterns. (Dr. R. Barnes, p. 242.)

Application of the Long Forceps.—There is this difference between the long and short midwifery forceps, that the former is invariably applied in relation to the pelvic diameters whatever the position of the head, whereas the latter is applied in relation to the fœtal head. When the long forceps are used the position of the head may be practically disregarded. The pelvic curve of the blades indicates that these must be adapted to the curve of the sacrum in order to reach the brim. They must therefore be passed as nearly as may be in the transverse diameter of the pelvis. One blade will be in each ilium, and the head, whatever its position in relation to the pelvic diameter, will be grasped between them. The universal force of this rule much simplifies and facilitates the use of the instrument. (Dr. Barnes, p. 244.)

If the blades lock when applied, it may generally be taken as evidence that they are properly adjusted to the head. If they do not, either they are improperly applied, or the pelvis is not adapted to their use. If the blades cannot be got to lock, that is, the second blade to pass exactly opposite to the first, and both in the transverse diameter of the pelvis, the attempt must be given up and the hand passed into the pelvis, if necessary, to explore its dimensions and form carefully, and to determine between turning and craniotomy. (Dr. Barnes, p. 245.)

The following manœuvre is occasionally serviceable, and in certain cases

may enable us to deliver without resorting to the forceps or lever. Pass a finger into the rectum, so as to get a point of pressure upon the forehead. In this way it is sometimes possible to bring the face downwards, to start the extension movement, and thus to extricate the head delayed at the outlet. And if at the same time firm downward pressure be made upon the breech through the fundus, the force propagated through the spine will aid materially in giving the extension movement. (Dr. R. Barnes, p. 247.)

Dr. Beatty's Midwifery Forceps.—These forceps were first described by the inventor twenty-five years ago, since which time he has constantly employed them and used no other instruments. The old short forceps have been totally abandoned in Dublin in their favour. They are designed as intermediate between the long clumsy French forceps and the old short ones of Smellie. Their entire length is twelve and a half inches; of the blade to the lock, eight inches. It will be seen that the blades, which have no lateral curve, are unusually long, they are also at the greatest breadth only an inch and three-eighths. The curve of the blades is not great, in order to facilitate their introduction. In using this instrument, the point should never be urged in the direction of the long axis of the blade. Such a proceeding is calculated to inflict injury on the mother. The blade should first be laid with its concave surface flat on the child's head, and the handle projecting between the thighs of the mother. It must then be gradually introduced by a series of lateral sweeping movements into its proper place, no force whatever being used. (Dr. T. E. Beatty, p. 248.)

The forceps may be used with all the adroitness possible, and all the force allowable as extractors, but without success in many cases of occipito-posterior position of the fœtal head; whereas, a mere tithe of the same force will answer if the head be rotated at the same time, and the forehead be turned backwards and to the left, and the occiput forwards and to the right. If this manœuvre be attempted with curved forceps, on its accomplishment we shall have the points projecting backwards, and liable to inflict great injury on the mother. (Sir J. Y. Simpson, p. 254.)

From the preceding observation of Sir James Simpson, it will be seen that Dr. Beatty's forceps are applicable in an important class of cases, in which the curved instrument of Dr. Barnes would not answer the purpose. (Dr. H. E. Eastlake, p. 253.)

New Needle for Fissure Operations.—Dr. P. H. Maclaren, of Lasswade, Edinburgh, recommends a needle for facilitating the introduction of metallic sutures in operations for the cure of vesico and recto-vaginal fistulæ, and congenital palatine fissure. This needle is 3½ inches long, perforated throughout and fixed in a handle. Its essential peculiarity is that its free extremity for an inch and a half describes the half of a wide spiral. It may be obtained from Mr. Young, of Edinburgh. (Glasgow Medical Journal, July, 1867, p. 102.)

Speculum.—*The Duck-bill.*—The duck-bill speculum is preferable to all others for examinations and operations on the vagina and uterus. Sunlight is not the most suitable kind of light; and it is preferable that the light should enter the room from one window at the back of the operator rather than from different quarters. A table about four feet long covered with a blanket, is preferable to a bed or couch for the patient to lie upon. The position is on the left side, body diagonally to the table, so that the buttock rests on the left of the angle next to the operator and the window, the thighs being bent at right angles to the trunk. (Dr. J. M. Sims, p. 265.)

Sponge Tents.—*Carbolised.*—Sponge tents may be rendered incapable of putrefaction by introducing into the core of the tent several threads of cotton wick steeped in carbolic acid; and after the sponge is rolled into its proper shape, by immersing it into cocoa butter to which a certain quantity of glacial carbolic acid is added. The disinfectant properties of this agent completely protect the tents, and they are withdrawn in an inodorous state even after a stay of eighteen or twenty hours in the cervical canal. (Mr. R. Ellis, p. 266.)

Urethritis in the Female.—The two principal causes of this affection are

gonorrhœa and parturition. After the acute stage has passed off we can only rely upon local remedies. The most reliable are the following—1. Nitrate of silver. This should be held in a small caustic holder on the end of a stilet and sliding easily within a small silver tube, from which the caustic can only be made to project a short distance. The nitrate should be sheathed in the tube when passed to the vesical end of the urethra, and then being projected beyond the silver tube, the whole should be withdrawn. By this means the mucous membrane is lightly touched throughout its whole extent. 2. Dip a moderate sized gum elastic tube in gum water and then in powdered tannic acid. After it has dried it should be passed gently through the fingers to remove any roughness, and when wanted for use, is just passed gently into the urethra, and allowed to remain about fifteen minutes. 3. A stick of dried sulphate of zinc may be passed up the passage and immediately withdrawn. Any of the above plans of treatment is effectual. The mode of using the nitrate of silver is also suitable for the cervix uteri in chronic inflammation of that part. (Dr. J. B. Hicks, p. 261.)

Uterus.—*Fixation of by a Tenaculum during Operations on it.*—Whenever anything is introduced into the uterus, be it a sound or a tent, a syringe or a knife, the cervix should be fixed with a tenaculum, after introducing the duck-bill speculum. By this means steadiness is ensured, and, when necessary, the neck of the womb may be drawn down into the vagina, and so directed that the surgeon can at once see into it. (Dr. J. M. Sims, p. 265.)

Vascular Growths at the Meatus Urinarius.—These are of two kinds, polypoid and sessile. The first should be removed by means of a wire snare, and the base touched with nitric acid. The second should be destroyed with nitric acid. For this purpose nothing is more convenient than to take a common lucifer match, cut off the inflammable portion and round the tip, which should then be dipped in nitric acid, the superfluous liquid wiped off, and the saturated wood applied to the part to be destroyed. No caustic acts so well as this. (Dr. J. B. Hicks, p. 261.)

MISCELLANEA.

Bichloride of Methylene as an Anæsthetic.—Dr. Richardson proposes the bichloride of methylene as a substitute for chloroform. It has one physical property of great importance and value, viz., that it volatilizes at a temperature lower than that of the body. It is hence thrown off from the system with great ease, and the patient recovers from the effects of the vapour almost instantly. At the same time the sleep produced by the bichloride is more prolonged than that produced by either chloroform or tetrachloride of carbon, and is not preceded by any excitement as in the former, nor followed by any drowsiness or dulness as in the latter. On animals, the bichloride acts more evenly on the respiration and circulation than any other anæsthetic. When the breathing is quickened the pulse also is quickened in proportion, and when the breathing is slow and tranquil the pulse also is the same. This is a good point, because there is no condition more perilous than the disturbed balance of the respiratory and circulatory systems. The bichloride dissolves in water, and the solution possesses intoxicating properties. This solution may probably be found to be useful as a soothing medicine, or anodyne. (Dr. B. W. Richardson, p. 270.)

Blow-Pipe Gas Cautery.—This is an instrument by means of which the intense heat of the blow-pipe flame is made available as a cauterising agent. The supply of gas is contained in an india-rubber ball filled from an ordinary gas-jet, and as the lighted gas is forced from this it is converted, by a current of air, which impinges upon the flame, into a sharply-pointed conical jet, possessing sufficient heat to fuse the ordinary metals. The whole apparatus is packed in a small box, and has the advantage over the galvanic cautery both in portability and cheapness. It is made by Mr. Baker, of Holborn, from the design of the inventor, Mr. Alex. Bruce. (See woodcut, p. 275.)

Casts.—*An Easy Method of Taking.*—Paint the part of which it is desired to take the cast with melted paraffin by means of a large brush, and as each layer dries put on another until the paraffin is a quarter of an inch thick or more, according to the size of the cast. During the few minutes in which the paraffin remains plastic, a sharp pen-knife is to be run over the lines requisite for the removal of the matrix. A cold water cloth should then be placed over all for five minutes, when the pieces may be removed and reunited by a hot wire. Plaster is then to be run into the matrix in the usual way, and after it has set the paraffin is to be carefully broken off, and the cast dressed. (Mr. Lawson Tait, Medical Times and Gazette, Aug. 17, 1867, p. 168.)

Chloroform.—On the whole we have no safer anæsthetic than chloroform, if it be administered with steadiness, boldness, and attention. Much of the mischief attending its use arises from handling it too cautiously, in a too fearful way, prolonging thus the alarm of the patient and the chances of spasm of the glottis. The latest deaths have been from mixtures or "mixed vapours." (Dr. C. Kidd, Glasgow Medical Journal, May, 1867, p. 16.)

Disinfection by Chlorine.—Mr. Stone, Professor of Chemistry at the Manchester School of Medicine, has constructed an apparatus which will, continuously and with graduated flow, supply chlorine in such a way as to be available for use in the sick chamber or the wards of an hospital. As a substitute for disinfection by chloride of lime, it is infinitely preferable for obvious reasons. By Mr. Stone's arrangement, a mechanism is afforded by which the amount of chlorine generated can be exactly regulated. (Lancet, Aug. 31, 1867, p. 272.)

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THE

RETROSPECT

OF

RACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

ONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M. D.,

E LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.,

AND

JAMES BRAITHWAITE, M. D., LOND.

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PRACTICAL MEDICINE.

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

-THE EPIDEMICS OF TYPHUS AND CHOLERA IN LIVERPOOL.

By Robert Hamilton, Esq., Surgeon to the Southern Hospital, Liverpool.

uestion as to the fungoid origin of typhus and cholera is becoming the
n of the day, and is gradually exciting amongst us an interest still
mperfectly proportionate to its immense importance. Modern inves-
n is fast leading us to the belief in the cryptogamic origin of many
s; and though we may be staggered by the suggestion that such a
as rheumatism has its cause in the sporules of a fungus, we are not
n it is attempted to prove that the germs of other fungi have to do
e development of cholera and fevers.]

; we are on the eve of arriving at the conclusion that the malarious
of hot climates, that dysentery, yellow fever, and cholera, are all
by the action of different species of fungus or of infusoria, seems ex-
r probable. And if so, the time is not far distant when further inves-
n will prove that enteric and typhus fevers, as well as scarlet fever,
s, and small-pox, have a like origin.

epidemic of pestilential intermittent fever at the Mauritius is perhaps
est, as it has been, in proportion to the population, the most fatal of
courges of mankind; and the facts already published concerning it
t malarious influences with its outburst, whilst its afterspread was ag-
ed by contagion, by contaminated drinking-water, and by exposure to
anations from the dejecta of infected persons. And such, too, is the
of the yellow fever in its rise and progress, in whatever country it
; of epidemic dysentery in its home in India; and of cholera. Each
ndent upon its own peculiar fungus for the train of symptoms which
and this, again, belongs to that genera or species which the animal
getable life of the country where it grows tends to produce.

fever at the Mauritius has led to further discoveries. The investiga-
f Dr. Schmidt have proved the presence along the whole of the in-
l canal, of a patient who had died from the fever, of minute plants of
ıs the counter-types of similar growth found by the aid of the micro-
in the water of the Grand River. Can we resist the belief that the
wed its existence to the presence of these growths?

difficulties attendant upon all microscopical examinations of the
h, intestines, and their contents, are so great, and the conclusions de-
rom the same so liable to be erroneous, when the search is for organic
ıls, that it requires the other links in the chain of evidence to be many
ong. We may at least aver that external facts do point very forcibly
cryptogamic origin of all the most virulent of the infectious diseases,
ow fever, cholera, and the marsh fevers. In these instances, it seems,
ere, that a close scrutiny and examination of the tissues and organs of
fferer who has succumbed to one of them must lead to the discovery
presence of organisms, vegetable or infusorial, which have given rise
particular malady: and doubtless it will do so. The very fact of Dr.

Sehmidt having detected them in his fever patients, and Hallier in the evacuations of a cholera patient, will stimulate other microscopists to examine and test the truth of the discovery.

The blood in several types of fever, as well as in cholera, has hitherto failed to exhibit, under the most carefully repeated microscopical examination, the presence of these growths, or of anything abnormal. And this has proved a stumbling-block to many. Preoccupied with the belief that the majority of these diseases are blood diseases—that continued fever, for example, is the very type of a disease which consists in the poisoning of the blood by some deleterious agent from without, and failing to find its presence in what seems its chief seat, they have abandoned, and even opposed, the doctrine of the fungoid origin of fever. It is remarkable that neither Hallier nor Schmidt found in the blood of their patients any of the spores which they yet discovered so abundant in their stomachs.

I must not weary the readers of the Lancet by detailing facts connected with the yellow fever in the West Indies and Africa, or with the rise and progress of the cholera, or with the various intermittent fevers and dysenteries of tropical climates. An examination of the past volumes of the Lancet alone, where particulars of a great number of these outbreaks are given, would suffice to show that the general condition of the country or district where these diseases prevail is the same in all. Rank vegetation, stagnant waters, a polluted atmosphere, or one damp and reeking with the products of decaying animal and vegetable matter, contaminated water, with heaps of human ordure lying near it, are more or less characteristic of them all. Dr. Livingstone, in writing to a friend, stated that around every village on the Zambesi there was a very large collection of human ordure during the dry season, which was swept away into the rivers by the heavy rains; but he adds, "What must be the effect of this filth from hundreds of thousands of villages upon the stream? And though they do not take their drinking water from it, but dig holes in the sand close by, yet we can understand how readily by percolation the water in the sand-holes has become impregnated with noxious matter. All the rivers on the West Coast of Africa—the Niger, Old Calabar, the Congo—are in the same way polluted."

It is very important to bear in mind that these streams, and likewise the sewers, drains, and cesspools of towns, which hold in suspense the germs of many a poison, may, and doubtless do, contain other ingredients which destroy the vitality of the germs, and so we account for their limited spread. Countless myriads of them are rendered innocuous, or their organisation destroyed, and new combinations formed out of their elements, by the action of other ingredients held in suspense in the same medium. This thought is a very suggestive one; and, if we but follow it up, we see at once how large and counteracting a cause to the spread of infectious diseases is ever at work for our preservation. Not all, or probably one-thousandth part, of the spores given off by one seed-vessel fall to the ground, or there meet with favourable conditions for their fructification and development. The birds may devour them, frost or cold may destroy their vitality; the presence of a noxious gas or emanation in the air, or an atmosphere charged with deleterious influences, may neutralise their power of germinating; and, then, what becomes of them? They do not remain unchanged; but when the subtle essence of their life is gone, they break up at once by the action of surrounding matter, and their constituents enter into new combinations. But allow that they do commence an independent existence,—let the seed fall to the ground, and under the favouring conditions of heat and moisture burst its capsule—even then it is but to meet with, in the majority of instances, new causes inimical to its existence; for, on the one hand, whilst it has overcome many obstacles, on the other it requires certain fresh conditions without which it cannot grow. To mention but one of these, as perhaps the most important: a congenial soil, one not charged with matters that would be destructive to it—and we can understand how, though the seeds and *spores and germs* of vegetable life may be as the sands of the sea-shore in *number*, yet the germs and spores of animal and infusorial life are equally

nd the one is so often inimical to the coexistence of the other that bar-
ss, death, dissolution, recombination into new forms, is the almost uni-
l fate of both. If they are not always destructive to one another, there
t other agencies to bring about their destruction; and, in the mineral
om, we have forces at work in the evolution of gases and acids, the
of which upon microscopical animal and vegetable life, though but
nderstood at present, may be safely affirmed to be opposed, as a rule
ir further growth or development.
hese things we find an answer to those who say that, if infectious dis-
had their origin in the sporules of a fungus, each disease would be
more wide-spread and universal than it is. Again, we have no reason
nk that the fever or the cholera-breeding fungus is ever so abundant
t of most vegetable fungi; the very rarity of its appearance, its com-
ively limited spread, and its disappearance after a time, are no argu-
against its existence, but simply demonstrate its delicate organisation,
sy destruction, and the rarity of the combination of conditions neces-
to produce it. The history of infusorial life is, in many of its aspects,
r to what has here been said of the vegetable world; and, as the two
loms approximate so closely in their most primitive forms, so are they
rly acted upon by external influences. It may be that some diseases
their rise in the entrance of the spores of certain species of the infuso-
nd some others in that of the germs of certain genera of fungi. The
of either, whilst floating in the air, must be inconceivably minute; but
introduced into the stomach by the food, or perhaps (as they most com-
r are) by the saliva, they there find material congenial to their germi-
; or, on the other hand, destructive matters may be present. In the
case, they are resolved into their ultimate elements, and, entering into
ombinations, cease to be noxious, if they originally were so. In the
r, a development probably of the most rudimentary form is produced,
his, though germinating within the stomach, may be as harmless as
nds of its species are, or as poisonous in its degree as its larger brother
ng in the field or buzzing in the air. Speedily developed thus far,
w requires other conditions than are to be found in its present habitat,
t as speedily dies; but if possessed of noxious properties, it and its
's who entered with it have, during their short but active existence,
eded in poisoning the nutrition of the body. The exact mode in which
have done this remains a still unsolved problem.—*Lancet, Nov.* 23, 1867,
).

NTERMITTENT FEVER SUCCESSFULLY TREATED WITH STRYCHNIA.

By Dr. John Pearson Nash, Surgeon in H. M. Madras Army.

Nash was induced to try strychnia in certain cases of ague, from the
e of quinine in effecting a cure. Mercara is the head-quarters of the
ct of Coorg, and is situate at an elevation of 4000 feet above the level
sea. The climate is cool and pleasant though extremely unhealthy
tain seasons of the year.]
se 1.—Mr. J. H——, a coffee-planter, residing on his estate, eight miles
Mercara, had been suffering from intermittent fever for three months,
aroxysms recurring every third day with great severity, and lasting
ten hours. After having taken large doses of quinine for several weeks
ut any benefit, he sought my advice, and presented the pale and emaci-
appearance of a person who had suffered severely from ague. I pre-
d the one-sixteenth of a grain of strychnia, to be taken four times a
which immediately checked the paroxysms, and the patient was able to
to his estate after having been only five days under treatment, and
a fortnight afterwards to intimate that he still continued free from

Cases 2 and 3.—Two daughters of the chaplain of the station had suffered from repeated attacks of intermittent fever, and having failed to derive any benefit from either quinine or the arsenical solution, were at last recommended a change of climate. After two months' absence, they returned to Mercara, and again contracted intermittent fever, the paroxysms of which commenced every other morning at six o'clock, and continued until noon. As the ages of the young ladies were 9 and 11 respectively, only one-thirtieth of a grain of strychnia was ordered to be taken four times a day in some strong infusion of quassia. In six days the younger girl was free from attacks of fever, and in eleven days the elder one had also recovered.

Case 4.—Captain P. J——, the proprietor of a coffee estate in a very ma-larious portion of the district, who is continually suffering from fever, an-d over which quinine has long since ceased to exert any influence, was ordere-d the one-twentieth of a grain of strychnia four times a day. On the eigh-th day of treatment the patient expressed himself as being perfectly well, a-nd free from fever.

Case 5.—J. C——, a carpenter in the Mercara gaol, has suffered severe-ly from repeated attacks of intermittent fever, for which he has taken an en-or-mous quantity of quinine, and has also been treated with the arsenical so-lu-tion, but without any benefit from either mode of treatment. He was the-re-fore ordered the one-twelfth of a grain of strychnia four times a day, un-der which treatment he rapidly recovered, and has continued in good health e-ver since.

I find from some tables prepared by my medical subordinates that in the Regimental Hospital, Gaol, and civil Dispensary at this station, thirty-se-ven cases of intermittent fever have been lately and successfully treated with strychnia, with an expenditure of a little less than one drachm of the alkal-oid, and that the average period of recovery was on the eighth day of treatme-nt; whereas in the thirty-seven preceding cases treated with sulphate of qui-nia, with an expenditure of fourteen ounces of this salt, recovery did not re-sult until the twelfth day of treatment.—*Lancet, March* 14, 1868, *p.* 344.

3.—ON THE USE OF SEPTICIDAL AGENTS IN DISEASE.

By Dr. Arthur Ernest Sansom, Physician to the Royal Hospital for Diseases of the Chest, City-road.

[The use of sulphites in zymotic diseases was first advocated by Polli *in* 1857.]

It was found that animals could, without any apparent ill effects, swallow and absorb large doses of the sulphites. It was then observed that when the animals were killed they long resisted the putrefactive process—that whilst an animal killed under ordinary circumstances exhibited rapid and marked signs of putrefaction, an animal which had previously absorbed doses of the sulphites betrayed no sign of putrefaction whatever. Another series of experiments, and in this series three hundred dogs were the bases of the deductions, showed that the sulphites exerted a prophylactic and curative power when septic poisons were introduced into the economy.

Then, as regards the human subject. It was found that the stomach would tolerate large doses of the sulphites of soda or magnesia. They were tried in the various eruptive fevers, intermittents, typhus, typhoid, pyœmia, puerperal fever, dissection wounds, malarial infection, &c. The records of cases treated in this way show an extraordinary amount of success. Dr. Polli states—"I published my first memoir on this subject in 1861, and since that time one hundred and fifty-eight papers have been published in answer to my call; and, with the exception of five or six containing some criticisms on my labours, all the remainder confirm, in the strongest terms, by many *hundreds of* detailed observations, the value of these remedies."

I can endorse this statement from my own personal experience. I have

employed the sulphite of soda in the eruptive fevers, in malignant sore throat, in diphtheria, as well as in cholera and choleraic diarrhœa. In measles I have used it in a number of cases, always with apparent success. In scarlatina I have employed it, both internally administered and as a gargle; its efficacy in anginoid symptoms has been marked. In diphtheria I have employed it in like manner in three cases. These were all extremely severe; all but one recovered, the fatal case being a child of 2 years, to whom none of the sulphite was for twenty-four hours, by a mistake in the nursing, administered. In a case of small-pox the effect seemed to be wonderful. The face was completely covered with pustules, the drug not being administered till the pustular stage had arrived. There was extreme prostration, the pulse was, on November 7th, 1866, 104; the sulphite of soda, in scruple doses, was assiduously administered every four hours, and in two days the signs of danger passed, the pulse falling to 76 on November 9th. I notice that Dr. Nichol, in the *Nashville Journal of Medicine and Surgery* for August, 1866, testifies to the efficacy of the sulphites in small-pox. For cases illustrating the value of the sulphites in cholera and choleraic diarrhœa, I can refer not only to my own volume, but to the narrative of cases in my first communication to your columns. The cases there mentioned were all treated by the sulphite of soda method, and all recovered.

If it be granted that the sulphites are useful in the infecting diseases, what is their *modus operandi?* In my answer to this question I am forced to differ from Professor Polli, who says, "these salts do not act as poisons towards the several morbific ferments. . . . They do not kill the catalytic germs of the organic poisons; but they re-act on the material components of our own organism, rendering it, by their presence, incapable of being acted on by these catalytic germs." The mode in which Polli supposes they affect the "fermentable stuff" of the organism is a process of deoxidation. I have before said that sulphurous acid and the sulphites are not mere deoxidizers, but that they have a special influence in arresting the vitality of low organisms. Moreover, there are many arguments against Polli's hypothesis that the sulphites act not on the ferment but on the pabulum, wherein the ferment undergoes development. In the first place what does physiological chemistry tell us of concerning the action of the sulphites in the economy? Dr. de Ricci says, "Animals after having been saturated with sulphites were killed, and every portion of their body, solids and fluids, gave, on examination, unquestionable proof of the presence of the sulphites employed. These experiments seemed to prove that when sulphites, whether alkaline or earthy matters or not, are introduced into the stomach of a living animal, they may be absorbed and circulated *as such* through the organism." Furthermore, "Dr. Polli found in men as in dogs, that they are not ordinarily decomposed in the stomach but pass unchanged into the blood, or, at least, appear in a brief period unchanged in the urine, and only after a while as sulphates." From these facts alone it may be concluded that the deoxidizing power of the sulphites must be limited indeed, for that power could be strictly determined by their transformation into sulphates. Furthermore, if the efficacy of the sulphites depended on their deoxidizing power over the fluids of the organism, consider what an enormous quantity would be required to effect such an object! What does analogy teach us? A trace of sulphurous acid or an alkaline sulphite will arrest the fermentation of a large quantity of saccharine material. The same quantity would not suffice to deoxidize the smallest fraction of this material; therefore, its efficacy must lie in some other quality than its power of deoxidation. Analogy tells us that it has a peculiar power, as I before stated, of arresting vitality; this power is catalytic not chemical, and finds its chief exemplum in carbolic acid. There is nothing antagonistic to reason in the conclusion that substances which are perfectly harmless in the case of a high organism should be fatal to a lower organism. Common salt, among a multitude of others, presents an instance in point. I think, therefore, we cannot fail to come to the conclusion that the sulphites are readily absorbed by the higher organisms—that they circulate in the economy for the most part unchanged—that

they exert a peculiar power, not upon the pabulum of the body but upon organized particles which may exist within it; and that in virtue of this septicidal power they manifest those prophylactic and curative effects which both experiment and experience have proved them to possess. We now arrive at

Proposition D.—That the germs of cholera are capable of destruction within the living body.

It may seem that this proposition admits of an easy discussion, for, according to our former conclusions, the argument might stand thus: Diseases which depend on morbid germs are to be successfully treated by sulphites or such-like antiseptics. Cholera is a disease which depends on morbid germs; therefore cholera is to be successfully treated by sulphites, &c. But this conclusion is not at this time justifiable; it can only be said that an *á priori* case has been made out.

Many circumstances may militate against the success of a treatment, though its principle may have been satisfactorily enunciated. The virus of cholera may induce such rapid effects that remedial agents of sufficient power may not be capable of withstanding it in time; or the poison may have a long period of latency, which no symptom may betray until its hold on the body has been fatally strong; or it may have a more intense power of proliferation and diffusion throughout the body than the poisons of those zymotic diseases heretofore mentioned. Or, perhaps, the sulphites which have been shown to be efficacious in other diseases, may not be powerful enough, or administrable in sufficient quantities, to kill the germs of cholera. It may be, therefore, that though antiseptics can destroy the virus of cholera outside the organism, internally administered they may be powerless. It was greatly thus in the case of cattle-plague: a mass of evidence proved the antiseptic power of carbolic acid; but the internal administration, though apparently in some cases successful, was not attended with an advantage proportionate to the value of its external employment.

Professor Polli did not claim for the sulphites the probability of a success in cholera. "Dr. Polli does not venture to assert that they will prove available in cholera or the plague; but he thinks there is reasonable ground for concluding that they will be highly useful in prophylaxis, in destroying the fermentable material on which the malignancy and fatality of these pestilences chiefly depend, and thus render an inevitable attack mild. This opinion, however, is founded on the hypothesis that the sulphites act, not upon the ferments, but upon the "fermentable material"—a hypothesis which I have considered unsound. The antiseptics act directly on the virus. It behoves us, as we have done in the case of other zymotic diseases, to take the evidence afforded by actual cases.

I have related in my book on cholera, cases in which the treatment by sulphite of soda was successful. All those cases related in my former communication to these columns on the Cholera Poison, were treated by the sulphites, and they seemed to show unmistakably the advantage of the mode of treatment.

Carbolic acid, internally administered, has been testified, by many observers, to have been successful in cholera during the recent epidemic. I find that so far back as December 17th, 1862, Dr. Godfrey advocated the administration of carbolic acid in cases of gastric irritability, especially when produced by miasmata or sewerage exhalations; he anticipated much good from it in the treatment of cholera.

The difficulty which has been urged against the antiseptic treatment of cholera, especially by those reviewers who have noticed my book, has been that the agents administered, could not be sufficiently rapid in their action to neutralize the cholera poison. We know, however, that the sulphites are like other salts of alkalies and alkaloids very rapidly absorbed and circulated throughout the organism—a few minutes sufficing for their diffusion throughout the economy. The argument, therefore, against the sulphites would lie *not against the rapidity*, but the *sufficiency* of their action.

On the other hand, carbolic acid is, I think, proved to be the most power-

fully septicidal agent known. The difficulty in this case is its deficient absorbability. It is undoubtedly an irritant, as may be at once proved by applying a little of it to the skin—the stomach will not tolerate doses of more than two to five minims, and of this amount it is probable that only a small proportion is absorbed into the blood.

The great desideratum, in my opinion, has been an agent which should supply the qualities of the rapid diffusibility of the sulphites, with the potential efficacy of carbolic acid. This desideratum I believe now to have been supplied. Some months ago I saw, at the house of my friend, Mr. Crookes, a specimen of a compound salt of sulphuric and carbolic acids with potass. I determined to pursue the method of investigation which thus opened out to me, and I have now succeeded in producing the compound salts of potass, soda, ammonia, and magnesia. I have found them to possess every quality which could be required of them, and a record of my practical experience with them I only defer to a future time.—*Medical Press and Circular, Jan.* 22, 1868, *p.* 61.

4.—ZYMOSIS WITH SPECIAL REFERENCE TO CHOLERA.

By Dr. Sansom.

[The following is an abstract of a paper read before the Medical Society of London.]

The author commenced by giving an outline of the theory of zymosis. In tracing the origin of infecting particles, we may, he said, divide them into two classes—First, those arising from the animal world, such as variola, vaccine, pyæmia; and secondly, those arising from the vegetable world, as favus, thrush, and, if we are to believe a large mass of scientific evidence, diphtheria, ague, &c. But whether animal or vegetable, it cannot be determined with accuracy whether the materies morbi is, at the period of infection, one or the other. It is best, under such circumstances, to call it "germinal matter." Dr. Sansom then related a series of cases which had occurred in his practice, all of which were united by close relations of time, place, and circumstances, and in one of which the "oidium albicans" was discovered as a prime factor in the disease. The author then discussed the operation of disinfectants. He divided them into three classes—First, those which alter the chemical constitution of the materies morbi, such as chlorine and iodine; secondly, those which act partly chemically and partly vitally, such as the sulphites; and thirdly, those which act only on organised material, arresting vitality, such as carbolic acid. The treatment of zymotic disease by the internal administration of the sulphites was then considered, and forty-one cases were brought forward in which they had been employed, and in which one death only occurred. The facts seemed to be that the sulphites are the most easily absorbed of our internal antiseptics, but that carbolic acid is the most powerful. The author concluded by saying that the great desideratum was a salt which combine the two. This desideratum Dr. Sansom had succeeded in fulfilling, and specimens of compound salts, the sulpho-carbolates, were exhibited to the Society.—*Medical Times and Gazette, Feb.* 22, 1868, *p.* 216.

5.—CLINICAL EXPERIENCES WITH DR. RICHARDSON'S "STYPTIC COLLOID."

By Dr. John Lowe, Surgeon to the West Norfolk and Lynn Hospital.

In ordinary operations performed on healthy tissues, Nature is, as Hippocrates said, "sufficient of herself to the cure of every evil." She pours out a protective fluid for the closure of the wound, and prevents those untoward results which follow the production and absorption of noxious products. But in less healthy structures, or where closure of the wound is impractica-

ble, these results are certain to follow in a greater or less degree, and it is in such cases that we need assistance in controlling the tendency to decomposition in the fluids, and thus preventing the risks from pyæmia, while at the same time fostering and protecting the delicate cell growth on which we depend for the completion of healing.

Owing to the antiseptic principles (gallic acid and benzoin) contained in the styptic colloid, we find this indication completely fulfilled. The healthy character which the pus assumes after its use in necrosis operations, contrasts wonderfully with the usual sanious and offensive secretions when other dressings are used.

Case 3, which is narrated below, was one of the most striking I have ever seen, both as regards the speedy production of healthy pus and the rapid cure effected. Many conservative operations depend mainly for their success on the degree of immobility which it is possible to maintain. Frequency of dressing is in these cases the *bête noire* which we most dread. With the most delicate handling, and with the Surgeon's constant personal care, how difficult it is to avoid doing mischief! Without them how often failure results! But if these be no longer necessary, except in a very minor degree, the gain is great indeed. The case of amputation of the foot shows well how little of this kind of supervision is required. Those who have had to undergo severe operations would soon tell us the worth of a process which would diminish the number of dressings to two or three.

But there are other cases in which we find equal advantage from the use of the "colloid." Burns, which are both troublesome, offensive, and painful in the dressing, are deprived of half their terrors. When profuse suppuration sets in, the "colloid," if painted freely over the surface, at once removes the fœtor, protects the granulations, and the wound speedily heals. No doubt the first dressing is painful, but not so the others. With moderately sized wounds, the first dressing is also the last. Nothing further is required but daily painting with the "colloid." In cancer the comfort obtained is very great. I have given a case in which the colloid was used with most perfect success, and strangely enough, with a marvellous diminution in the amount of pain.

Taking into consideration all the advantages which we obtain from the use of this preparation, and without departing from a sober estimate of the value of the facts observed, I cannot but conclude that we of the Profession, and above all the public, are under great obligations to Dr. Richardson for his simple but most elegant and useful discovery.

[Dr. Lowe then gives a case of amputation of the foot, by Pirogoff's operation.]

On May 13, the patient being put under the influence of chloroform, I proceeded to perform Pirogoff's operation, with the modification which I had previously adopted—viz., the preservation of the periosteum of the tuberosity of the calcaneum, the bony structure being enucleated by means of the gouge. The ends of the tibia and fibula were found to be sound, but a large portion of the anterior flap was gelatinous and very unhealthy. Owing to the contraction of the Achilles tendon, it was found needful to divide it. No vessel required ligaturing; the flaps were brought together by silver sutures. About an hour afterwards the wound was closed up by a free application of styptic colloid and lint, in the manner described by Dr. Richardson. Very little blood had been lost during the operation, and, with the exception of the anterior flap, concerning which grave doubts existed, the case promised a successful termination.

2nd day. Doing well; not much pain.

4th day. As before. No discharge.

5th day. Slight discharge; no smell whatever. Colloid applied freely over the wound.

8th day. The colloid has been applied daily. There has been pretty free *discharge*, but no odour. On dressing the stump for the first time, it was *found* that a large piece of the anterior flap had, as was feared, sloughed. *The slough* was not, however, detached. The sutures were removed; a rough

of pieces of strapping applied to support the heel; colloid poured freely into the wound; lint and a light bandage applied.

16th day. There has been rather free discharge, but, thanks to the colloid, which has been applied daily over the bandage, no unpleasant odour. The stump was again dressed to-day, when the slough came away, leaving a healthy rapidly granulating surface. The end of the tibia was quite exposed, but showed no signs either of repair or decay. The heel was brought forward by strapping, and colloid applied as before.

On the third dressing the wound had nearly filled up by granulation; the tibia was no longer exposed; a thin shell of bone had separated from the outer margin of the fibula. There is now but little discharge.

June 19. Up to this time the limb has been kept perfectly quiet. The dressing has been applied daily without moving the stump. The wound is as nearly as possible healed, and there is only a trace of discharge. General health much improved.

July 10th. The stump has daily become firmer. Only one small spot about the size of a sixpence remains uncicatrised, owing to the traction of the reunited tendo Achillis, which had drawn the heel backwards.

20th. By the application of a splint to the back of the leg, this has been counteracted, and the sore is now all but healed. The stump is a well-formed one, with the heel nicely in the centre, and bids fair to render the limb a very serviceable one.

It is impossible to overestimate the value of such a dressing as the "styptic colloid" in a case of this kind. The perfect immunity from fœtor, the quiet state of the limb which is allowed, and the diminished risk of purulent absorption, together with the healthy granulation which is permitted to progress unretarded by the presence of a septic poison, are facts which create a new era in Surgery.

The perfect state of rest which was obtained in this case seems to have been the main element in its success. Three dressings only were required in the first three weeks—that is, the limb was only moved so many times. Judging from what I have seen of the action of the colloid, I am disposed to think that erysipelas will prove of much less frequent occurrence under its use, if, indeed, it be not altogether abolished in Surgical practice.

Case 2.—*Cancer of Mamma—Beneficial action of "Colloid."*—[Mrs. F., had first noticed a small hard tumour, near the right nipple four years ago She was advised to have it removed, but she refused her consent.]

The tumour continued to enlarge, and in July last ulceration commenced. The sore was five inches in length, extending from the nipple across the anterior border of the axilla. The remains of the breast closely adherent to the muscle. The axilla was filled with a mass of enlarged glands. The wound was sloughy, and intensely offensive. Continued lancinating pains deprived the patient of her sleep and appetite. She had lost flesh rapidly in consequence, and her face bore the appearance of cancerous cachexy in a high degree.

She was at this time quite willing to undergo the operation, and begged me to remove the breast. This, I told her was impossible, as the disease had advanced too far. I could hold out to her no hope save of mitigating her pain and making her last days more easy. To remove the horrible effluvium which made the poor creature a nuisance to herself and family, I painted the wound over with colloid, and gave her a bottle of it to take home with her. I also gave her a morphia pill to be taken at night. She came again the following week, when I was surprised to find that the wound had become quite clean, and was healthily granulating, every trace of smell having of course disappeared. But more than this, the pain had also gone; she had enjoyed comfortable nights, her appetite had improved, and, with hope in her countenance, she looked years younger than she had done the week before. She felt certain she would get well, but on this head I could give her but little encouragement, so extensive was the amount of disease. However, she continued to improve; *the wound*, which was constantly painted with the styptic, *gradually filled up*. *No* pain was experienced, except when she ran short

of the dressing. The enlarged mass of axillary glands diminished *pari passu* with the wound, so that now they can scarcely be felt. The wound itself is not larger than a fourpenny piece (July 15), and would, I think, have been quite healed ere now but for the difficulty I have in inducing her to rest the arm, which, owing to her having a large family of small children, she has not been able to do.

Case 3.—*Caries of Tibia.*—W. A., aged 14, a delicate, strumous boy, residing at Snettisham, was admitted into the West Norfolk and Lynn Hospital with disease of the ankle-joint, March 13, 1867. Several sinuses communicated with the joint, but the probe did not come in contact with any diseased bone. The limb was put up in a gutta-percha splint. Patient placed on a generous diet.

April 22. The sinuses continue to discharge, and now denuded bone is felt at the extremity of the tibia. Patient having been put under chloroform, an incision was made over this point. The articular end and about three inches of the shaft of the tibia were found to be soft and carious. By means of the gouge and pliers, the whole thickness of the bone was removed over this extent, care being taken to avoid the joint. The wound was filled with colloid, and closed by a piece of lint soaked in the same, the limb being placed again on a splint.

On the fourth day, when the limb was dressed, the wound was granulating very freely, and was filled with thick creamy pus instead of the usual sanious discharge one meets with in similar cases. The same dressing was continued, the wound rapidly closed, and on

May 13 he was made out-patient, only a small sinus, caused by a fragment of undetached bone, remaining. The splint was still retained, and patient was ordered to come up in a few weeks to have the fragment of bone removed. General health greatly improved.

The styptic has been largely used in our hospital in a variety of cases, and invariably with the best results. I have as yet never seen it fail to answer the ends for which it was designed. In old chronic ulcers, foul-smelling and filthy to a degree, the odour is at once destroyed, and healthy granulation established. Most markedly was this the result in a case of my colleague's. The patient, a young sailor, who had been long at sea, was admitted with a number of very large syphilitic ulcers on both legs, the stench from which made the ward unbearable. They were freely painted over with the "colloid," and in a very short space of time the odour was all but gone. They healed most rapidly.

In cancer of the penis, too, it is most serviceable in removing the odour and in mitigating pain. Nothing could be more striking than its action in this respect in the case of cancer of the breast which I have reported above.

In conclusion I may state that erysipelas is, with us, to all appearance a thing of the past. Some time ago, owing to imperfect sanitary arrangements, almost every surgical case in the hospital was attacked with erysipelas, and several died of pyæmia. Some improvement was effected in the hygienic condition of the hospital, but it is still imperfect. Latterly, however, since using the tincture of iodine to the surface of the wound and styptic colloid externally, there has not been a single case of either erysipelas or pyæmia, and the cases generally remain a very much shorter time on the books. There is no doubt that the colloid is an expensive application, and that it causes a serious expense to the hospital; but I consider that the gain from the rapid cure of the cases under treatment largely overbalances the original outlay; and not only is the expense diminished by the shorter duration of the cases, but, owing to the absence of septicæmia, there is a greatly reduced need of stimulants and extras.—*Medical Times and Gazette, Jan.* 25, 1868, *p.* 88.

6.—ALCOHOLIC RHEUMATISM FROM DAILY IMBIBITION OF FERMENTED ALCOHOLIC LIQUORS; AND ITS REMEDY.

By JOHN HIGGINBOTTOM, Esq., F. R. S., Nottingham.

About thirty years since, I first noticed that a form of (so-called) rheumatism was cured by abstaining for some time from the use of fermented alcoholic fluids. I said at that time, that the complaint should not be called rheumatism, but alcoholism, as *alcohol* produced the disease, and abstinence from *alcohol* was the remedy. Rheumatic gout and "the poor man's gout" may perhaps, for the most part, be that form of rheumatism; and gout, in some cases, may be superadded. I find Mr. N., a clergyman, has also noticed the fact in a work (*His Clerical Experience*, by the Rev. Thomas Rooke, M.A.) lately published. He says, referring to the effects of alcohol, "I may instance three men in our society—men advanced in life, who while drunkards were, as they express it, '*eat up with rheumatism*,' now, that they are tried abstainers, are entirely free from it, and look fresher and younger by several years."

This form of rheumatism is produced by fermented alcoholic beverages, and has not been distinguished from rheumatism or gout. It appears usually about the middle stage of life, after the person has for some years daily imbibed fermented alcoholic liquors, such as beer, ale, porter, cyder, &c., even in moderate quantities. The disease advances insidiously, and the accumulating effect of the beverages produces great changes in the person, both physical and mental. There is an expression of stupidity or dulness in the countenance, a weakness of the intellectual powers, a stiffness and sluggishness of the body; the limbs lose their suppleness, causing a slight clumsiness of gait, followed by hobbling and ultimate lameness, which increases with age, and becomes permanent, if the drink be continued. This state probably arises from a change of structure in the synovial membrane, sheaves of the tendons, and the joints, when the complaint has the appearance of chronic rheumatism. This complaint does not appear to affect the general health.

Ordinary rheumatism arises from a far different source, and is caused by exposure to cold and wet, cold air, and by the variableness of temperature, insufficient clothing and food; attacks mostly the young and strong; is uncertain in the times of its attack, and characterised by quickly changing its seat from one part to another; affects most of the limbs, the joints and the course of the muscles being successively affected; and is attended by fever of an inflammatory type. Chronic rheumatism is a frequent result of the acute.

The cause of alcoholic rheumatism in this locality, I believe, is chiefly the effect of malt liquors; but a writer observes that cyder-drinkers have a full share of the complaint. He says: "There must be something deleterious in cyder, as it is the precursor of rheumatism in the labouring man. Witness the number suffering from the effects of it, hobbling about at forty years of age; the cyder countries are full of them." I have no doubt the complaint is alcoholic rheumatism, produced by the fermented alcoholic beverage from the apple as from malt, just as like results attend the daily use of malt liquor beverages.

Alcoholic rheumatism may be modified or prevented in a great measure, in those persons who do take fermented alcholic drinks, by living on proper nutritive food and taking much exercise in the open air, which quickly throws off the alcoholic poison from the system, and prevents the evil. On the contrary, if the persons lead a sedentary indolent life, and indulge in taking abundance of food and fermented alcoholic drinks, they will probably inflict a double evil upon themselves—obesity, and alcoholic rheumatism or gout. If they are of a gouty diathesis, this will tend to increase their lameness and diminish their locomotive powers, as we see in landlords of inns, gentlemen's coachmen and butlers, and others of the same class. A farmer, who rides on his nag to overlook his labourers, and spends the afternoon and

evening in his corner chair with his pipe and his ale, is a subject for cor pu-
lency and alcoholic rheumatism or gout, which may be, indeed, the precur sor
of paralysis, apoplexy, disease of the liver or kidneys, and also of calcul li of
the kidneys or bladder. The labouring men, with daily out-door exerc ise,
although they may take their regular allowance of ale, and probably sp end
evenings at the alehouse, will probably have their share of alcoholic rheu ma-
tism, although free from obesity. Their drinking habits, hereafter cont tinu-
ed, will bring on other diseases, and premature old age and death. Such men
are frequently old at sixty years of age.

A remedy for alcoholic rheumatism is absolutely called for, as so lar ge a
class of men are suffering from it. I believe most cases may be treated suc-
cessfully by abandoning the use of fermented alcoholic beverages altoge ther.
I have a firm conviction that abstaining will cure most cases; and, even
where structural changes have taken place, the *pain* may be relieved, if not
removed.—*British Medical Journal, Jan.* 11, 1868, *p.* 26.

7.—ON DISINFECTANTS.

By Dr. William Procter, F. C. S.

Dead organic matter passes rapidly into a state of change or decomposition, the nature of the resulting products being dependent upon numerous collateral circumstances. Amongst these changes putrefaction is specially distinguished by the fœtid character of the matter evolved. The more complex the composition of the bodies, the more unstable is their equilibrium, the more readily do their constituents enter into a state of change, and the more offensive are the emanations. Even the final products of putrefaction (especially when air is excluded or imperfectly supplied) are of this character, comprising, as they do, sulphuretted hydrogen, sulphide of ammonium, phosphides, ammoniacal, and other like compounds. It would not seem that the ultimate products of putrefactive decomposition directly generate zymotic disease, although they may produce their individual special effects, but that indirectly they promote its invasion by the exclusion of pure air, by forming a nidus for the nourishment of morbific matter, and, by depressing the powers of the system, render the body more predisposed and less resisting, and cause a more rapid spread of specific disease. It is rather to some intermediate product of the putrefactive process, and especially to the organic effluvia still in a state of change, that the power of setting up zymosis is to be referred. But the resulting gases may themselves either destroy life rapidly by acute poisoning, with symptoms well recognised, or produce a certain class of endemic diseases not transmissible under ordinary circumstances. Dr. H. Barker has submitted this matter to the test of experiment by conducting the air of a cesspool, containing carbonic acid, sulphuretted hydrogen, and sulphide of ammonium, into a box in which animals were confined; the symptoms which resulted resembled the milder forms of continued fevers common to the dirty and ill-ventilated homes of the lower classes of the community, and Dr. Barker attributes the results not to the organic but to the gaseous matter. The effects of these gases were then tried separately on confined animals, and he found that of sulphuretted hydrogen 0·5 per 100 may produce serious symptoms, and 4·2 per 1000 was rapidly fatal, but not with the symptoms or pathological conditions of cesspool fever, while on the other hand, a volatile alkaline body, such as sulphide of ammonium, persistently administered, produced both the symptoms and pathology of fever. Dr. Dundas Thompson was one of the first to recognise the importance of organic matter as a constituent of the air of towns, and to enunciate the proposition that the gases evolved during putrefaction are not the main source of danger The existence of a large amount of foreign matter in the atmosphere has been satisfactorily demonstrated. If pure distilled water in an open vessel *is* exposed to the air, the loss from evaporation being made up by the addition *of fresh* liquid, on the careful microscopic examination of the sediment

which is formed, monads, amœbæ, and other of the lowest forms of life, will be detected. Pasteur placed soluble gun-cotton in a glass tube, and by means of an aspirator caused a current of air to pass through it for several hours. The cotton being dissolved, and the residue examined, was found invariably to contain organic growths, and he proved that the air of inhabited places contains a greater relative number of germs than the air of uninhabited regions. Over the mouth of an impure cesspool a glass globe was placed filled with ice; on its surface the aqueous vapour, with its soluble suspended matter, was condensed. This liquid was turbid, had an offensive odour and alkaline reaction, and contained small flakes of matter which under the microscope had the appearance of organic *débris*, and with it there were vibriones, monads, and other low forms of life, with confervoid and fungoid filaments. Somewhat similar results are obtained from the vapour condensed by placing a bell-jar over putrid meat. Experiment has shown that in such an impure atmosphere milk is rapidly changed, and meat speedily putrifies, It is eminently a suggestive fact that these germs are not everywhere present in all forms and equal numbers. They exist numerously in the lower strata of the air and in densely populated towns, becoming fewer as we rise higher, and at such elevations as the summit of the Alps they are almost absent.

These conditions, then, are general; but in certain localities special germs are detectible. M. Chalvet has collected putrescent organic matter from the walls of hospital wards; and when watery vapour near a suppurating surface was collected, it was found to be charged with irregular corpuscles resembling dried pus. Eiselt, of Prague, found small cells like pus cells, in the air of a ward in which epidemic ophthalmia was raging.

These facts afford just reasons for concluding that the air is a medium through which diseases may be transmitted either by specific germs or by the presence of organic matter in a state of change which may set up certain so-called endemic diseases. In every instance of communicated morbid condition, there is material cause, however subtle its nature and mode of transmission, which must be concerned in its propagation. The power to communicate certain diseases is a material substance, in all probability made up of solid non-volatile particles, certainly not gaseous. Probably, then, it is a germinal cell, of which vaccine lymph may be taken as the type, of peculiar organisation, capable of being transmitted from one locality to another, of preserving its vitality for a time at least, outside the organism, and within of reproducing itself.

We can imagine two ways in which poisons may act on the system.

1. When virus is introduced into the system, it multiplies rapidly, and seems to have the power of transforming healthy matter into matter of its own nature, as small-pox. 2. By catalysis, a minute quantity of virus being sufficient to set up the septic changes in the blood to which it has gained access. Carefully conducted experiments by numerous observers have shown that the composition of the blood during disease undergoes alterations and variations. Purkinje says that the blood in cholera contains pure urea and an extractive substance by which the urea is rapidly converted into carbonate of ammonia. Diseases closely resembling those which occur naturally may be set up artificially, by introducing into the circulating fluid substances capable of acting catalytically. Abscesses have been produced by injecting pus into the veins of dogs, septic affections by the injection of putrid purulent matter into the veins, and diseases with all the characters of typhoid fever, by the introduction of putrid blood into the circulation. It must be admitted that countless germs of vegetables, infusoria, &c., exist in the air, and grow and multiply whenever they find an appropriate nidus. This condition of the atmosphere has been called by Dr. Sanderson septic, and derives importance from the possibility of being concerned in the production of zymotic disease. That these low forms of life may seriously affect the blood of the higher order of animals is clearly proved by the recent researches of Davaine, who has furnished the first well-established example of a disease of the blood due to the presence of inferior beings. A low form of bacteria was discovered in the

blood of sheep suffering from splenic apoplexy, and considered by the observer to be the cause of that disease. Dr. Salisbury, of Ohio, states that the prevalence of measles in the Federal army arose from fungi (a Penicillum). He was led to this examination by observing that a large number of men rose one morning with symptoms of measles, after sleeping on straw which was mouldy, and had a peculiar odour, and that, by inoculation with this fungus, he set up in many persons, in from twenty-four to ninety-six hours, a disease closely resembling measles. It is nevertheless proper to state that these experiments have been repeated by Dr. Woodward of the United States Army, and that he does not confirm them.

Strong evidence has lately been brought forward to show that fungoid vegetations, if not the cause, are intimately associated with malarial fever (Medical Times and Gazette, November 9, 1867). Although it is true, as remarked by Dr. Snow in speaking of his opinion that cholera is disseminated by an especial cell, "that it is no objection to this view that the structure of the cholera poison cannot be recognised by the microscope, for the matter of small-pox and chancre can only be recognised by their effects, and not by their physical properties," yet special poisons have in many cases been isolated. Dr. Richardson has separated the poison of pyæmia; it may, he say[illegible] be evaporated to the form of syrup or extract. It produces when dried [illegible] substance closely resembling the snake poison. It admits of being pulv[illegible]ised, and when it is introduced into the wound of a healthy animal produ[illegible] precisely the same symptoms as those of the patient from which the poi[illegible] was taken. Panum's experiments on putrid infection are of high inter[illegible] but he will not attempt to decide whether the putrid poison acts "directly [illegible] the system, or as a ferment to the blood." All the phenomena of dise[illegible] would indicate that blood-poisoning is the result, and, as Dr. Richard[illegible] says, "that each particle of any one of these poisons, brought into contact either with the blood of the living animal or with certain secretions of the living animal, possess the property of turning the albuminous part of that same blood or secretion into substance like itself." Dr. Halford made some experiments on the poison of the cobra. He states that when a person is actually bitten by that animal, molecules of living germinal matter are thrown into the blood, which speedily grow into cells and as rapidly multiply, so that in a few hours millions upon millions are produced. Do their number render the blood unfit to support life, or, with some analogy to a process of fermentation, do they impoverish the blood by growing at the expense of some element of it, and at the same time excrete a something, a poison to which the symptoms of the disease may be directly due?

If, then, it be true that the presence of organic matter or the products of its decomposition are important elements in the causation of disease, the question arises—Is it within our power to remove those causes or to diminish or to prevent their injurious effects? The organic contaminations found in the atmosphere possess, for the most part, a more or less complex constitution analogous to that of organic matter in general, and, being of an oxidisable nature, are therefore extremely disposed to enter into combination with active oxygen, and form with that substance new arrangements with their elements, of a permanent character. And, again, from the little stability of composition of contagious poisons, evinced by the fact of their decomposition at about 212°, it appears that their constituents are weakly held together. Here, then, one method is opened to us by which their action may be limited, by rapidly hastening their decay by a process of oxidation; or, on the other hand, some substance may be employed which prevents the compound from changing its original composition. In this manner two classes of disinfectants are obtained:—1. Oxidising disinfectants, which hasten the decomposition of the compound, and at the same time render the resulting products harmless by reducing them to their simplest forms. 2. Antiseptics or colytics, which place the matter in a state unfavourable for its destruction by oxidation or otherwise. To the preceding classes another division may be practically added, to which the term fixative has been applied—

[illegible], which entering into combination with the offensive

volatile products, prevent the pollution of the air by their escape.—*Medical Times and Gazette, Feb.* 29, 1868, *p.* 228.

8.—ON THE PRINCIPLES OF TREATMENT OF DROPSY.

By Dr. George Johnson, Physician to King's College Hospital.

There are two objects to be aimed at in the treatment of dropsy; these are, 1st, to remove if possible, the original and exciting cause of the dropsy; and, 2nd, to remove the dropsical accumulation. If we can accomplish the first of these objects the second is generally attained with it. The dropsy will soon disappear with the removal of its exciting cause. For instance the slight anasarca which occurs in chlorotic young women is a result mainly of the poor and watery condition of the blood; and the dropsy quickly passes away when the quality of the blood is improved by nutritious diet, fresh air, and exercise, with the use of iron as a tonic, and perhaps an occasional aperient.

In the treatment of acute renal dropsy, it is important to bear in mind the relation in which the dropsy stands to the renal disease; and it is especially interesting to observe the phenomena which occur during the progress of recovery. In particular, there is one phenomenon which deserves notice in connexion with the general pathology as well as the treatment of dropsy. I mean the copious flow of urine which occurs spontaneously during convalescence.

In cases of acute renal dropsy the urine is at first scanty and of morbid quality, being often high-coloured from admixture with blood, always albuminous, and usually containing numerous casts of the kidney-tubes. The scanty secretion of urine is the cause of the dropsy, and the secretion of urine is scanty because the flow of blood through the kidney is obstructed and the structure of the gland changed, the tubes being filled with desquamated epithelium, and with blood and fibrin which have escaped from the gorged Malpighian vessels. Now observe what happens during the progress of cure in a case of this kind. The patient we will suppose to be placed in circumstances favourable for recovery: he is confined to bed; has a scanty diet; the loins are dry-cupped, or mustard and linseed poultices are applied there; and means are taken to excite the secretory actions of the skin and bowels, and thus to lessen the work of the kidneys. Soon the secretion of urine begins to increase, until, in the course of four or five days, perhaps, the quantity of urine, which at first had been less than half the natural amount, becomes three times as great as the standard quantity, no diuretic medicine of any kind having been given.

The explanation of this spontaneous diuresis appears to be this. During the acute stage of the renal disease, the constituents of the urine, both solids and liquids, have accumulated in the blood, and have thence been effused into the areolar tissue and serous cavities. Now, urea itself is a most powerful diuretic; and no sooner is the inflammatory congestion of the kidney removed, and the freedom of the renal circulation restored, than the urea exerts its natural diuretic action on the kidney. The copious diuresis thus induced speedily removes the accumulated urinary solids and liquids from the blood, the areolar tissue, and the serous cavities into which they had been effused, and so the dropsy is cured.

This abundant flow of urine occurs without aid from diuretics or drugs of any kind. I have seen it happen while bread-pills alone were given as a *placebo*. Stimulating diuretics, such as squills, or cantharides, or turpentine, are injurious, by increasing congestion of the kidney. The best diuretics in such cases are means which tend to lessen the congestion of the kidneys; counter-irritation over the loins, especially by dry-cupping, hot-air baths and diaphoretics, purgatives, and a scanty diet.

In some cases of chronic renal dropsy, diuretics may be given without risk, but too often without much benefit in the way of removing or lessening

the dropsy. A pleasant and efficacious diuretic is the imperial drink made with lemon, cream of tartar and sugar, with the addition of gin, in the proportion of a wine-glass to a pint. Hot-air baths often distress the sufferers from chronic Bright's disease; for the reasons which I have before given, the skin does not readily perspire, and the body consequently becomes painfully heated. Of late, therefore, I have been in the habit of prescribing for these patients a daily packing for two or three hours in a wet sheet and blankets. In this way I succeeded in obtaining a more prolonged and copious diaphoresis, and that too, with less distress, if not with actual comfort to the patient.

In the treatment of cardiac dropsy, while we endeavour to remove the fluid by diuretics and by purgatives which excite copious watery discharges from the bowels, it is desirable to do what we can to sustain the power of the heart by nutritious food, stimulants, and tonics. Little or nothing can be done to repair a damaged valve, but much may be done to strengthen the muscular walls of the heart, and thus enable it to overcome the impediment to the flow of blood and the consequent tendency to dropsy which a diseased valve occasions. A combination of tincture of the perchloride of iron with tincture of digitalis is particularly useful in some cases of cardiac dropsy.

When other means fail to remove a dropsical accumulation, we may often afford great temporary relief, and prolong life sometimes for a considerable period by mechanical means—by tapping the abdomen, for instance, in case of ascites; by acupunctures or incision through the skin of the legs for the removal of anasarca.

It is very interesting to note the phenomena which follow upon puncturing or incising the legs in cases of anasarca. There is first a copious drain of liquid from the punctures. Secondly, there is a further exudation of liquid from the over-distended blood-vessels; this liquid also escapes from the punctures or incisions, and its escape is often associated with temporary symptoms of exhaustion, such as a rapid and feeble pulse and pallor of countenance. Thirdly, there occurs often a copious secretion of urine, in consequence of a more free circulation of blood through the kidneys.

Dropsical accumulation tends to cause a secondary impediment to the circulation by the pressure of the effused liquid from without upon the blood vessels. And, again, the capillary circulation becomes more and more impeded in proportion to the increasing distension of the veins, which results from cardiac or renal disease. The drain of liquid from the areolar tissue, allowing of a further exudation from the capillaries, thus removes or lessens the obstruction which results from over-fulness of the veins. The general circulation therefore becomes more free, and the greater freedom of the circulation through the kidney, is attended, as we have before seen, by a more copious secretion of urine. The greater freedom of the circulation through the kidneys is shown not only by the more copious secretion of urine but also by the diminished amount of albumen, and not unfrequently when albuminuria has been caused by passive congestion, the result of cardiac disease, the albumen quite disappears for a time after a copious drain of dropsical fluid through the skin.

The free of action of a hydragogue—elaterium for instance—is often followed by a copious secretion of urine. The gorged vessels are partly unloaded by the drain of liquid from the bowels; the circulation through the kidneys, as through other organs, consequently becomes more free; and hence a copious secretion of urine, and a rapid diminution or even a complete removal of the dropsy.—*British Medical Journal, March* 7, 1868, . 215.

DISEASES OF THE NERVOUS SYSTEM.

9.—ON STRYCHNIA HYPODERMICALLY ADMINISTERED IN PARALYTIC AFFECTIONS.

By CHARLES HUNTER, Esq., late Surgeon to the Royal Pimlico Dispensary.

[If morphia be proved to have greater rapidity and certainty of action when introduced into the cellular tissue than when given in the ordinary way, and that certain unpleasant symptoms can be avoided by given it by this means, it is reasonable to anticipate certain advantages from the administration of strychnia also by the cellular tissue.]

Pereira tells us that, "of all diseases for which nux vomica has been employed, in none has it been so successful as in paralysis, and it is deserving of notice that this is one of the few remedies whose discovery is not the result of mere chance, since Fouquier was led to its use by legitimate induction from observations of its physiological effects.

One has to study the dose which the constitution of the patient requires more carefully with strychnia than some other alkaloids, and to begin with a minimum rather than a maximum dose, for the effects from a large dose may be almost too rapidly generated to be subsequently beneficial. Some patients, moreover, are far more susceptible of strychnia thus administered than others, but such is also the case with morphia and opium. In one patient, a lady w ho was suffering from injury to the spine and severe nervous prostration, after a fall more than a year previously, I found the one ninetieth of a grain of strychnia as large a dose as she could bear; on the other hand most patients bear well the one thirty-sixth of a grain, and some even the one twenty-fourth.

That this *mode* of administering strychnia is more powerful than others is shown by the experiments of the Hypodermic Committee upon rabbits, for they found that the one sixteenth of a grain of strychnine was the smallest dose that killed by the mouth, the one twenty-fifth of a grain by the rectum, and the 1-120th of a grain "by the skin" as they call it, but more correctly by the *sub*cutaneous cellular tissue. That committee give no experiments or observations of the therapeutic effects in man, but allude to the experience of Dr. Biegel, "who found one case yield to this method," and to my own observations in favour of its usefulness in certain cases.

I will now shortly detail a few cases of paralysis in which I have injected strychnine with benefit; at the same time I would remark that one must not look for the astonishing or suddenly beneficial results which seem to and do at times attend the injection of anodynes in cases of *pain*, of delirium, &c.

Strychnine should be employed as a nerve tonic or nerve-excitant in cases in which active irritation is supposed no longer to be going on about the clot or presumed cause of the lesion.

What we may fairly hope to expect when we inject strychnine is to find a *beneficial* result produced in a *shorter time* and from much *smaller doses* than when it is given by the mouth, and benefit may be expected even in cases which have, as in Case 1, been frequently treated, for weeks together, by its stomachic administration without benefit.

Case 1.—Hemiplegia of six years' standing. John C—. aged 52 years. Had always enjoyed good health, and followed his occupation in the brewery until six years ago, when, whilst at work he was suddenly seized with a loss of power on the left side of the body. He has never since sufficiently recovered to permit of his returning to his occupation, and the utmost he can do is slowly, by the aid of two sticks, to walk about, dragging after him the paralysed limb. In addition to the want of power, he nearly always has *pain*, chiefly in the loins after walking a few yards, numbness and coldness, and a sense of weight in the limb, and fatigue from very trivial efforts, so that when out he has been accustomed to stand still frequently to rest, and to walk much bent forwards, chiefly to avoid *pain* in the loins.

This man had been a patient of mine at the Royal Pimlico Dispensary [illegible] and off for several years with these symptoms and loss of power over the bl[illegible]der, and generally after a few months of stomachic administration of strych[illegible] and other tonics, the symptoms have been moderated for a time. On the 2[illegible] of July, 1866, I put him on the one twentieth of a grain of strychnia—[illegible] acetate—with sulphate of magnesia, and some carminatives. This he c[illegible]tinued for five weeks twice a day without improvement, except as rega[illegible] the loss of power over the bladder.

On the 31st of August I began the subcutaneous administration of [illegible] alkaloid, expecting little or no more effect, as so little amelioration had [illegible] tended the stomachic doses during three or four years.

To sum up briefly, this man, from August 31st to December 1st, th[illegible] during three months, had twenty-two strychnine injections; seventeen [illegible] these were in quantity the one thirty-sixth of a grain, the remainder wer[illegible] as strong as one twenty-fourth of a grain. During the five weeks which *pre*[illegible]ceded the hypodermic treatment, he had taken as much as *three grains* [illegible] strychnine by the mouth without any apparent or palpable effects of the dr[illegible] upon the paralysed limbs; *but direct strychnine* effects manifested themselv[illegible] with the *first* as well as with *every* succeeding puncture.

Ten injections were given during the first three weeks. The result[illegible] were:—1. Rapid improvement in walking power, and increased steadines[illegible] of gait. 2. The previous sensation of cold was replaced by a sense o[illegible] warmth of both legs, chiefly of the paralysed one. This *induced* warmth ha[illegible] never quite subsided, and was always well marked for some hours after th[illegible] puncture, in fact he almost *complained of* the heat the injections produced[illegible] throughout his system. 3. The "heavy-weight" sensation in the leg was[illegible] replaced by a sense of lightness of the limb. 4. The pain both of the loins[illegible] and leg were removed.

These good results have continued, and he can now walk several miles, [illegible] more upright, with comparative ease, with little or no pain, and by the aid, [illegible] for a time, of only one stick.

The question might be put whether the three grains of strychnia, previ[illegible]ously given by the mouth did not materially assist in these results, seeing[illegible] that by the cellular tissue he only had altogether about two-thirds of [illegible] grain?

My reply is simply that he had more than once taken strychnia for two or[illegible] three months by the mouth without such good results. 2ndly. That the ef[illegible]fects from the injection were direct and comparatively immediate. 3rdly They were such as have been described spontaneously by other patients.

Case 2.—William J—, aged 60, of Belgrave Buildings. A tall, thin, ac[illegible]tive, intelligent engine-driver, was seized in Sept., 1863, with right hemiple[illegible]gia. He was at engine-work when the fit came on. He became insensible and had much fever, he says, for some days. He gradually improved so [illegible] to be able to get about with a stick, but the leg has very little power, feel heavy, and drags; he feels that there is no dependence upon it. The ar[illegible] is weak, especially the muscles of the upper arm and shoulder, but he c[illegible] grasp with his fingers.

December 9th. Omitted medicines by the mouth. I injected the one nin[illegible]tieth of a grain of strychnine into the arm.

10th. He felt momentary pains in the course of the evening about [illegible] muscles of the forearm and heel; no muscular twitchings, but less "stret[illegible]ings" of the muscles. Repeated the injection.

11th. Felt a lightness and freedom in the muscles of the limb. Repe[illegible] injection of one ninetieth of a grain.

12th. Had a few slight muscular twitches in the lumbo-dorsal regior[illegible] both sides of body. Injected the one sixtieth of a grain.

13th. The paralysed limbs feel stronger and lighter; has had a few tw[illegible]es in the right side of the face, right arm, and both legs.

21st. Has had three more punctures from one seventy-fifth to one si[illegible] *of a grain.* He had two or three punctures after this date, and by th[illegible] *of the month* the power both in the arm and the leg had greatly incr[illegible]

he could turn his wife's mangle with the arm without pain or fatigue; he could stand steadily and strongly upon the leg, and stamp the foot on the ground with some vigour. On the other hand, during the ten weeks from the time of the fit to the time of the first puncture, *no* progress, of any note, had been made; whereas each puncture (save two) seem to add fresh vigour to the muscles. More jerking of the muscles occurred after the last few than after the first puncture.

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April 24th, 1864. This man again came under treatment, not for paralysis, but for *cramp* of the *same* leg. For six weeks he had hardly passed a night free from four or five attacks of cramp which *came only* in the leg that had been paralysed.

I gave him a quarter of a grain of morphine subcutaneously; no cramp in the night; a slight twinge in the morning.

26th. Three attacks of cramp in the night. Repeated morphia injection.

May 24th. No cramp since.

October, 1866. Keeps well; able to turn the mangle, and carry parcels about, the leg remaining as strong as when the puncture was omitted.

Case 3.—James N., a sailor, aged 28. Came under my care on the 25th of May, 1866.

He had received a bullet fourteen months before in the dorsal region when at Shanghai. The bullet had entered close to the posterior border of the right scapula, and is somewhere deeply imbedded close to the spinal marrow. Paralysis came on gradually from that time; but it became much worse after he had been shipwrecked off Hong Kong eight months ago. He was two months in the hospital at the latter place. Strychnine was there given him by the mouth for some weeks; it caused jumping in the leg which is paralysed, but did not increase the power of the muscles.

He now walks very slowly and carefully with two sticks, and drags the left leg almost like a log; sensation is much impaired; leg still jumps occasionally, and would give way at once if he were to try his weight on it; has pain in the back and loins after walking many minutes.

It was doubtful to what extent power could be restored in this case if, as seemed probable, the bullet was mechanically interfering with the transmission of nerve power, the more especially as strychnine, as above mentioned, had already been given by the mouth *without effect*.

On May 25th, 1866, I injected the one twenty-fourth of a grain of strychnia into the cellular tissue of the upper arm, and on the 27th, the same amount into that of the leg.

28th. Can put the foot more firmly on the ground. Some jumping of the leg after both punctures; no jumping of the right leg.

28th and 30th. Repeated the same dose. Leg decidedly stronger and feels as if it now belonged to him, no longer like a heavy weight; can lift the foot higher, and he can now (after five punctures) just momentarily bear his weight on the foot without pressing on his sticks.

June 19th. I diminished the dose to the one thirty-sixth of a grain still injected every second, third, or fourth day.

29th. Can now walk two hours without feeling fatigue, and without that pain in the leg and loins which used always to occur after very small efforts.

The leg now feels quite supple and "sensible."

July 23rd. Up to this date this man has had twenty-six injections from the 25th of May, just over two months, and altogether has had less than *one* grain injected during that time. The following are the chief results from the last few punctures. The one thirty-sixth of a grain suits him better than the one twenty-fourth of a grain; it causes much less jumping, at the same time it always causes the leg to feel lighter and looser, and as if life had come into the heavy useless painful weight it formerly was to him.

October 28th. He comes up once a week from Barnes to report progress. He can now easily walk three miles without stopping to rest, and without pain in the loins, and can walk across the room without his sticks. These

good effects still remained when I saw him nearly one year after I had ceased to attend him.

Case 4.—Several injections of strychnia were given to a young woman who for some years had suffered from incessant muscular jactitations, frequent attacks of sickness, pain, and debility of the spine. The doses injected were from the one forty-eighth to the one twenty-fourth of a grain. This patient had received great benefit from the atropia injection, and was not told that the agent injected was different.

The chief effects observed by her were a sensation of warmth in the spine, of greater strength and less pain in the muscles of the neck; the sickness of stomach was also removed by it.

Case 5.—Mr. W., a publican, came to me on the 23rd of March, 1866. For the last three weeks he had complained of numbness of the legs and feet, of the sensation of pins and needles in the latter, and a fear of walking without holding or looking to see his feet. Is a sallow, puffy, unhealthy looking man, temperate, bowels regular, urine often loaded with lithates, and difficult to pass at times, no albumen in it. Has the tight pain or band across the stomach. I put this patient upon alkalies, and the one twenty-fourth of a grain of strychnia twice a day by the mouth. He improved good deal, sold his business, and went into the country.

On October the 23rd, he came back to London. He had been going pretty well until three weeks ago, when his legs again stumbled and he could now only walk slowly and with difficulty. Urine again loaded with lithates, more loss of power over bladder, bowels, &c. I ordered him alkaline saline by mouth for the state of the kidneys, &c., and this time gave him the strychnia subcutaneously, with the following results:—Decided increase of power of muscles of the legs and hips; he could, after five punctures, get up from sitting on a low chair, without supporting himself as before with his hands. Usually feels a warmth of both legs, increased diaphoresis, "live-blood" as it were circulating in his thighs, generally also a stiffness, but no jerking thirty minutes after each puncture, less fear of falling when walking, diminution of the numbness and trembling of the legs, and increased walking power.

He had ten punctures altogether, some in the arm, and some in the thigh. The doses were from one forty-eighth to one thirty-sixth of a grain.

Case 6.—John B., aged 31. Came to me in March, 1866, with loss of power of the muscles of the left arm and forearm, consequent upon bruising and dislocation of the shoulder-joint some weeks before. Most of the bruise had disappeared, but the power did not return, as indeed it often will not for months; he could not grasp anything, nor pick up things. I ordered an iodine and camphor liniment for some days without benefit resulting, and then had recourse to the injection of strychnia.

This man had seven punctures; three of which were into the muscles themselves, and four into the cellular tissue of one arm or the other. The chief effects observed by this patient were, 1st. He felt the influence usually in ten to fifteen minutes, viz., tingling in the fingers of the affected limb, some pains in, and increased temporary power of the muscles of the arm.

The power returned quicker in the muscles of the arm than did the activity to the finger.

He did not note any difference in the time in which the effect was produced, whether the injection was made into one arm or the other; but thought the fingers felt its influence most when the strychnia was injected into the pronator muscles. The dose in this case varied from the one forty-eighth to the one twenty-fourth of a grain.

Case 7.—William B., left hemiplegia. Came to me November 30th, *1867*. Has had left hemiplegia for two years and a quarter, since a fit, which came on suddenly after two or three previous threatenings in August, at Madras; he was a soldier at the time. The left leg now drags, and he cannot walk [illegible] stick) more than fifteen minutes without the leg becoming stiff [illegible] has cramp in it at night, usually three or four times. [illegible] mouth is still partially drawn: he seems

very nervous, and frequently laughs when he should not; sleeps well, bowels regular, tongue clean, frequent micturition, urine healthy.

January 30th. Has had eighteen punctures in two months, with the following results:—Feels generally stronger, not only in the limb which is paralysed, but "in himself." Has much more control over his muscles. He looks less foolish, and hardly laughs now as before without occasion. Always feels "warmer all over" after each puncture; feels more power and warmth for some hours in the leg after the injections, which were all made into cellular tissue of one of the arms.

In this man it is the arm, contrary to my experience in the majority of cases of hemiplegia, which has received the most benefit; he can grasp much firmer with it, and hold weights which he could not when he first came; he is less nervous, and looks as if he now had some energy and will of his own; it is also interesting that the cramps in his leg which used, for many months, to wake him up at night, have never returned the last six weeks. This patient is still under treatment.

Remarks.—The effects obtained from the hypodermic use of strychnia are not so remarkable as are those which are constantly to be observed from the injection of atropia or of morphia. This is easy of explanation. Anodynes and narcotics are agents acting especially on nerves of sensation, and on nerve-centres, and are most valuable thus employed in acute affections, such as delirium tremens, acute mania, of intense pain, &c. Strychnia is a nerve-tonic, and nerve excitant, and its employment is chiefly valuable in cases of palsy and nerve atony, after all acute symptoms or causes have subsided. But because the case is one in which time is required to assist the action of the remedy, there can be no reason against trying to shorten the period required for the cure, or to limit the amount of the drug from which we expect benefit to accrue.

The chief advantages to be derived from the hypodermic use of strychnia are,

1st. *As a therapeutic test*, for three or four injections will almost always show if strychnia has any power or not over that particular palsy. If they have no effect it is almost useless to continue their employment.

2nd. *It is economical, both as regards amount of* (*a*) *alkaloid to be used, and* (*b*) *time required for it to affect the system.*

(*a*) There is a great saving of the *amount of strychnia* required by this means over the stomachic employment of the same drug. The saving may be put at five-sixths of the amount which would be given by the mouth. For, as far as my observation goes, the one-thirtieth of a grain, injected every other day for thirty times, will consume just one grain of strychnia in sixty days: now if the somewhat ordinary doses of one-twentieth of a grain is given twice a day—and some physicians order the one-sixteenth and even the one-twelfth of a grain for a dose—it is evident that, in the sixty days, as much as six grains of strychnia are taken into the stomach. But, in cases of paralysis, where its continuance seem necessary, I do not, as a rule, inject the strychnia more often than twice a week, so that the saving is nine rather than six times less than the amount which experience shows us has to be given by the mouth for a reasonable amount of benefit to ensue.

Nor need this saving of material be considered remarkable nor the results singular. The experience of many medical men who have largely employed quinine injections in ague and fevers in India, Italy, and the West Indies, is quite in accordance with my results with strychnia. Dr. Moore, of the Bombay Medical Service, thinks four or five grains of quinine under the skin are equal to five or six times that amount taken by the mouth; and this opinion is confirmed by Drs. Chasseaud and McCraith, of Smyrna, by Dr. Desvigne in France; and more recently by Dr. Huggins and his colleague at San Fernando in Trinidad.

(*b*) In addition to the ultimate tonic effects of the strychnia being more quickly gained, and from a smaller amount of material, we have also manifested, by its introduction into the cellular tissue, certain phenomena indicative of the *immediate or direct* primary action of the agent upon the

system. These are, 1stly, a general warmth or glow of the cutaneous surface. This warmth is experienced by nearly every patient, especially those benefited by the injection. It is most felt in the paralysed limbs and down the spine, and at times making the patient "quite hot all over." This sensation lasts a few hours, according to the strength of the dose. 2ndly, a sensation of lightness and freedom in, and a removal of the "heavy-weight" sensation of the paralysed part. The leg often feels "light as a cork," and 3rdly, increased temporary power, which also generally begins in about twenty minutes, and lasts for perhaps some hours.

As regards the jerking of the muscles. This symptom of the action of strychnia appears to me to be of less frequent occurrence than when strychnia is given with a similar object in view for a lengthened period by the stomach. It seems practically to be the case, as might theoretically be conceived, that the primary tonic effects—namely, greater power and improved nervous sensibility—should follow the small strychnine injections, sooner and more effectually, than after the system has, by slow degrees, absorbed enough strychnia which has been administered stomachically, and the quantity of which so given has been perhaps three or four times as much as has really been necessary. When, on the other hand, muscular "twitchings," spasms, or cramp, have accompanied the palsy or numbness previous to the employment of the strychnine injection, those irregular muscular movements seem very quickly to yield to the tonic influence of the puncture, as in case 7, a in a bricklayer now under treatment.

Thus far, then, in favour of the hypodermic injection of this alkaloid. What, on the other hand, are the disadvantages of this method?

That great care is necessary lest too large a quantity should be injected of this agent, I need not point out: far better, therefore, always is it to inject a minimum than a maximum dose, say from the one-sixtieth to the one-eightieth rather than from the one-thirtieth to the one-twentieth. As a rule, however, the one thirty-sixth of a grain will suit most cases in which a tonic effect is desired upon a paralysed muscle or nerve.

I think it as well to mention that in two out of about twenty-five patients in whom I have injected strychnia a carbuncular state of the nose, or rather a collection of small boils arose. It is questionable to what extent, or whether or not, the strychnia injection is to be blamed. We know that belladonna will, at times, produce a rose-rash, and this is especially the case with atropia, its alkaloid, when injected.

Both of the patients who had the boils produced about the nose, had had a good many injections; one was the sailor with the gun-shot wound in the spine whose case is mentioned above; and the other was an Indian officer who had for years suffered from neuralgia and muscular prostration, consequent on—according to Brown-Sequard—"congestion of the spinal marrow."

I have seen no ill results from the injection of strychnia such as have been described as following the injection of the acetate or sulphate of quinine. Those salts seem, in many cases, to have set up severe irritation, and subsequent sloughing of the cellular tissue, and even tetanus has been attributed in two cases to the injection of that alkaloid. Strychnia is, however, innocuous to the cellular tissue, and scarcely ever causes the least smarting, burning, or other local sign of irritation, when injected. Nor would quinine, as far as my experience goes, if some more basic salt than those above alluded to were used.—*British and For. Med.-Chir. Review, April*, 1868, *p.* 446.

10.—NEURALGIA: CASES TREATED SUCCESSFULLY BY THE SPINE-BAG.*

By Dr. JOHN CHAPMAN, Physician to the Farringdon Dispensary.

The cure of neuralgia, whether the disease be treated by drugs given internally, or by applications of various kinds at the seat of pain, or by the two methods conjointly, is confessedly almost always difficult, and in a large proportion of cases impossible. The cases reported below have been treated by a method altogether new. By stating each case with extreme brevity, I am enabled to present at one view within a small compass the results of several experiments; and I do this in the hope that they may produce such an impression on the minds of professional readers as may impel them to acquaint themselves with the pathological and therapeutical principles of which these results are an expression. I shall hereafter publish an exposition of those principles, illustrated by reports of cases *in extenso*, and shall then give a full description of the treatment adopted.

1. *Facial Neuralgia.*—T. H., a gentleman, aged 35, who had been suffering during the previous fortnight, requested my advice March 18, 1865. He was in great pain, which had been continuous from the previous day, and which had wholly deprived him of sleep. The pain was chiefly on the right side of the face and head; but during the morning preceding my visit the left side had become invaded. The affected parts were very tender, and somewhat swollen. The head was rather hot, the face flushed, the tongue thinly coated with whitish fur; pulse 92, full and strong. Several medicines prescribed by two physicians in succession had proved of no avail.

I applied a ten-inch spinal water-bag, containing water at 130° F. to the cervico-dorsal region, and shortly afterwards left the patient's room. Within half an hour I returned, when I found him asleep. The treatment was continued for two days by means of heat; afterwards I used ice (at first in the lumbar region), and from the time he first fell asleep he continued free of pain, which has not since returned.

2. *Facial Neuralgia.*—Fräulein S., aged about 25, consulted me February 3, 1867, on account of neuralgia affecting the infra-orbital and dental branches of the trifacial nerve. The pain was not confined to one side of the face, but was sometimes most acute on one side, sometimes on the other; it increased at night and kept her awake the greater part of each night. She had been suffering in this way for about three weeks before I saw her. Her general health was good. The affected part presented no trace of hyperæmia.

She was treated by means of ice, and experienced almost immediate relief. After three days of treatment she felt, and slept, very much better; and before the end of the fifth day the pain had wholly ceased. Nearly a year afterwards she told me that it had never returned.

3. *Facial Neuralgia.*—Mademoiselle M., aged 20, consulted me in August, 1867, when she was suffering from acute facial neuralgia, the chief foci of which were the infra-orbital foramen and the mental foramen of the right side. The extreme pain came on in fits, sometimes at 8 a. m., sometimes at 2 p. m., but between the paroxysms the face continued to ache, and at times the patient had pain at the back of the head. She had suffered in this way about a fortnight before coming to me, and had had several similar attacks during the preceding year.

The treatment consisted in the application of the double columned hot water-bag. The malady was immediately subdued: no distinct paroxysm occurred after the first application of heat; all pain rapidly and completely subsided, and since that date has not returned.

4. *Dental Neuralgia.*—A. W. B., a Russian gentleman, suffering from

* The Spine-bags are supplied by Messrs. Maw and Son, 11, Aldersgate Street, London, and can be obtained from them through the agency of any druggist.

dental neuralgia, consulted me in September, 1867. The malady was chiefly confined to the teeth of both upper and lower jaw, but no particular tooth or teeth seemed to be especially affected. The pain was intermittent, and so severe as to interfere, seriously with the patient's daily occupation. No cause of the disorder, which had continued some weeks, could be discovered, and the face so far from showing any sign of hyperæmia over the seat of pain, seemed cooler than normal. In the course of the first day of treatment by means of the spinal ice-bag, the pain was completely subdued; the cold was persisted in for some time, and during the remainder of the patient's stay in England he continued free from suffering.

5. *Dental Neuralgia.*—H. E., female, aged 21, suffering from violent and continuous pain spreading over the teeth and gums of both the upper and lower jaw, consulted me, January 17, 1868. The pain was most intense in the lower jaw and on the left side; she had intense headache also. The forehead and cheeks were notably hotter than normal, and she complained of great heat in the roof of the mouth as well as in the gums—which were swollen and sore. During the previous week she had had several teeth stopped with gold; one of them became most especially painful; and there was threatening of an abscess at its root.

The treatment consisted in the application of cold across the occiput, and of heat over the cilio-spinal region—in the first instance separately, and afterwards simultaneously. The pain was speedily and completely annulled; it recurred, and was again annulled by the same method on several occasions. The patient volunteered the statement that during the application of the heat, her mouth became perceptibly cooler.

6. *Facial and Brachial Neuralgia.*—Mary A. T., aged 44, first consulted me at the Farringdon Dispensary, December 28th, 1867, when she was suffering from neuralgia of the right side of the head, face, and neck, and along the right shoulder and arm—extending to the fingers. The right half of the tongue was also affected. The pain, which was exactly limited to the median line, was described by the patient—"like as if something is pulling the flesh off the bone, it's so dreadful, and sometimes as if the parts were screwed up in a vice."

She was treated by means of ice applied along the whole spine. She improved immediately and rapidly, and as early as January 15th, informed me that she had not had "a bit of neuralgia" during the whole of the preceding week. Up to this date (February 22d), the pains have not returned.

7. *Intercostal and Hypogastric Neuralgia.*—Miss E., who first consulted me, January 14th, 1865, complained of extreme pain on the right side of the lower dorsal and upper lumbar vertebræ, extending outwards and thence forward to the right hypogastric region. The pain had continued every day and night, with occasional intervals of three or four hours release from suffering, and had lasted about 20 years. The pain was so distressing and wearing that the patient's strength had become much impaired—especially by loss of sleep, and she was unable to undertake any regular occupation. The only sleep she could get was induced by brandy and narcotics. She suffered also nearly every morning, and frequently throughout the day from nausea, and occasionally vomited "water."

I directed the patient to leave off brandy, opium, and morphia at once, and entirely; and treated her chiefly by means of ice along the lower half of the spine. As early as February 14th, the patient was enabled to say to me,—"During the last few nights I have slept throughout the night without being disturbed at all—an experience quite new to me." On the first of the following April she informed me that she had already been many days absolutely free from pain. The nausea and vomiting had ceased; and the bowels had become "open every day—quite a new feature." I have since heard from time to time, that this patient continues well.

8. *Intercostal Neuralgia.*—A man came to the Farringdon Dispensary, October 21st, 1867, and complained to Dr. Drysdale, whose patients I was seeing with him, of acute pain along one side of the chest; no assignable cause for [illegible] and we regarded it as a case of intercostal

neuralgia. I recommended the application of the spinal ice-bag along the dorsal spine twice a-day. "Sometime afterwards," as Dr. Drysdale related to the Harveian Society, "the man returned to the dispensary looking so delighted, that the doctor asked him what was giving him so much pleasure, when he replied that the 'ice-bag had done him a world of good: it had taken away all his pain with wonderful quickness.'"

9. *Intercostal Neuralgia.*—Mary B., aged 51, first seen by me November 27, 1867, complained of intense pain over the right shoulder, between the scapulæ, and along the right side of the chest. During the attacks of pain which came on and went away suddenly, the flesh, she said, quivered and tingled. She felt her body bent down as if she could not move it for fear of the pain.

She was treated by the application of the spinal ice-bag along the lower half of the spine during an hour twice a-day. The patient was surprised to find herself already quite free from pain during November 29th and 30th. Early in December, it recurred very slightly, but on the 18th, she said she remained free from neuralgia, except slight pain in the evening when fatigued by her work; she became much stronger, and was not so "nervous"; by December 28th all pain had gone, and February 26th, when I last saw her, it had not returned.

10. *Chest-Ache.*—October 21st, 1867, I was asked at the Farringdon Dispensary by Dr. Drysdale to prescribe for one of his patients, Maria Williams, aged 22, who was then suffering from continuous aching beneath the right clavicle, together with "dreadful pains" in the head, dizziness, heaviness in the morning, and profuse leucorrhœa—which had lasted several months.

I prescribed the application of a narrow 16-inch ice-bag along the spine during 30′ twice a day—the bottom of the bag being placed on a level with the 4th lumbar vertebra.

October 26th. The patient reported that the ice-bag had done her "a wonderful deal of good"; that the leucorrhœa had stopped, and that *all* her head symptoms had vanished. I requested her to persist in the treatment previously prescribed. By November 2nd the pain beneath the right clavicle had quite ceased, and no one of all her other troubles previously got rid of, had recurred.

11. *Mammary, Ovarian, and Uterine Neuralgia.*—H. M., a girl aged 17, who was first seen by me Feb. 24th, 1863, and who was suffering from daily attacks of epilepsy (*petit mal*), complained of extreme pain in the region of the left ovary, which was tender on pressure, and increasingly so immediately before the patient's menstrual periods; also of pain in the right mamma —which was considerably larger than the left, and of "fearful cutting pains" in the womb during menstruation.

I treated this case chiefly by means of iced-water applied along the spine. The result was complete cessation of the mammary, ovarian, and uterine pains; the mammæ became of equal size; and the epileptic vertigo, which had continued many years, ceased entirely a month after the treatment began, and has never recurred.

12. *Neuralgia of the Legs.*—I was consulted June 25th, 1867, by Mr.——, M. P., aged 49, who complained of excruciating neuralgia in the legs. The pain was of the ordinary tearing, shooting, and stabbing kind, sometimes in one limb, sometimes in the other, and affecting one heel almost continuously. This was often so acutely tender that he could not bear to touch the ground with it. As a rule the patient suffered most at night, and could rarely get any refreshing sleep. The malady came on about sixteen years ago, and had recurred at intervals, ever afterwards. Until the morning he came to me he had never been free of intense pain for nineteen days. The patient was in the habit of drinking daily about a pint of sherry which I advised him to give up.

The treatment consisted in the application of a spinal ice-bag along the lumbar and the lower half of the dorsal vertebræ. The patient informed me, July 19th, that "practically speaking" he had had no pain since he began the treatment, and that he had slept well. I advised perseverance with the ice,

and abstinence from wine. The patient wrote to me, August 21st, "I continue to receive great benefit from the ice application, and shall continue it." The pain afterwards recurred in a mitigated form, but was greatly subdued by the ice, and, I incline to believe, would be quite cured if the patient would wholly abstain from wine.

13. *General Neura'gia.*—June 22nd, 1855, I was consulted by Lord——, on account of neuralgia affecting different parts of the body; the walls of the chest, and the lower extremities were chiefly involved. The attacks generally came on suddenly, and lasted about fifteen hours, sometimes longer. The pains were of an acute, stabbing, and seemingly spasmodic character, and were often brought on by vigorous muscular exercise, especially deer-stalking, which his lordship is very fond of, but which his attacks either cut short or prevented him from indulging in.

I prescribed the application of ice along the spine, and five grains of citrate of iron and quinine during a few days at a time occasionally. When I saw the patient again, April 27th, 1866, he informed me that he had applied the spinal ice-bag as directed during several weeks at a time at three successive periods since he consulted me. He declared himself so greatly improved that he had been quite free from his malady for long periods together, that when it had recurred the pains had been comparatively slight, that he could resume violent exercises, including deer-stalking, without bringing on an attack, and that in his opinion, had he continued the treatment regularly for a longer time he should have been completely cured.

14. *General Neuralgia.*—Hannah E. complained to me January 4th, 1868, at the Farringdon Dispensary, of great pain in all the four limbs, but most especially in the fingers and toes, and in the left side over a spot about the size of half-a-crown; of headache each morning; of "dreadful" back-ache; and of excessive irritability of the bladder, involving the necessity of urinating about every five minutes.

She was treated by the application of the lumbar ice-bag 90′, twice a day. Within a week the pain in the limbs was almost wholly gone. By the 15th of January it, as well as her headaches, had quite ceased: her back was much better; and she was obliged to micturate only about every hour. On the 5th of February she reported herself free from every pain on account of which she had consulted me, and the irritability of the bladder was so much lessened, that she was only troubled with it at intervals of about 90′.—*Medical Press and Circular, March* 4, 1868, *p.* 201.

11.—THE INFLUENCE OF THE NERVOUS SYSTEM IN DISEASE.

By Dr. SAMUEL WILKS, Physician to, and Lecturer on the Practice of Medicine at, Guy's Hospital.

The cases which we may style nervous are without number—in fact, a very large part of all diseases which are called functional are indeed nervous. You have only to remember that every function of the body is regulated by nerve force, and you will perceive that the study of nervous diseases must embrace affections of every organ and tissue of the body. If this be so, their study must be considered to be still in its infancy. For example, a heart is beating irregularly, palpitating, or its force is being reduced; this, we know positively from experience and experiment, may arise from a nervous influence. But suppose that the heart should altogether cease to beat, and on post-mortem examination the dissector should pronounce it healthy—by means only as regards its muscular tissue and valves; but how about its nerves? Just think of those beautifully dissected hearts at the College of Surgeons, which look more like bundles of thread, so closely are they covered with nerves; and then remember the experiments which you have had shown to you of the movements of the heart when out of the body, as well as of the contraction of its segments which contain the ganglia; you will perceive the importance of the nerve supply to the heart, and the consequences of derangement of its nervous elements. How many cases of dys-

pepsia, constipation, liver disorders, are due purely to a nervous cause! Think of the sympathetic nerve surrounding the blood-vessels! the vaso-motor nerves, and how they influence necessarily the supply of blood to the tissues, and thus exert a power over the processes of nutrition. Think of those large semilunar ganglia and the solar plexus with their supply to the vessels and viscera, and you cannot but suppose that their influence is all-potent. We get some kind of hint of the importance of these structures when, as we see in Addison's disease, all these nerves matted together and the patient die out from sheer exhaustion, while all the organs of the body are perfectly healthy, In peritonitis and strangulated intestine, also, we perceive a similar prostration. No wonder it has been suggested that in these ganglia lies the *fons et origo mali* known as cholera, since, in a few short minutes, a healthy body may be reduced to almost the condition of a corpse. I have very little doubt that the influence exerted by this solar plexus has much to do with the fact of the frequency of cancer in the neighborhood of the stomach. Surely, then, we have much to learn about this important centre of our bodily solar system. Considerations of this kind prompt me to the belief here yet may be that wide field of therapeutics open to us, in which we may employ remedies which may alter the nutritive process through the agency of the vaso-motor nerves.

No doubt such considerations have led to the belief that in the application of heat and cold might at last be found the universal medicine, the dilatation or contraction of the blood-vessels being thought sufficient to rule all other changes, and, these vessels being altered in calibre by the influence of the nerves, a depressant or stimulant to the ganglionic cells would be the means of regulating all nutrient processes. Speaking generally, there can be no doubt of the power for good or for evil of heat and cold. Consider the value of drugs from this point of view. Look at opium, which has so striking an influence on the nervous system, and then go into the surgical wards and see a large sloughing sore heal under its use; this will give some idea of the value of such classes of remedies, and how nutrition is modified under their use. I am beginning to use aconite more largely than I have hitherto done, seeing no *à priori* objection to much of the power which is attributed to it. I do not see my way to the arrest of a disease which is passing through its several stages, as pneumonia, but I see no objection to the belief in remedies of the opiate class having the power to arrest morbid processes. That aconite is used largely by homœopathists should not allow us to be prejudiced against its adoption. There is a story related of the late Rev. Rowland Hill, to the effect that he once demanded that a hymn should be sung to the tune of a then popular air. On a remonstrance by some of his hearers, he said he did not know why the devil should have all the good tunes. I must leave you to make the application.—*Medical Times and Gazette, Jan.* 4, 1868, *p.* 1.

12.—A CASE OF TRAUMATIC TETANUS TREATED WITH NICOTINE.

By Reginald Harrison, Esq., Surgeon to the Liverpool Northern Hospital, and Lecturer on Anatomy at the Liverpool Royal Infirmary School of Medicine.

[An additional interest has been given to this plan since the publication of the Rev. Prof. Haughton's paper on the counteractive effects of strychnia and nicotine. The following case of tetanus was in a boy 11 years of age, and resulted from an irregular scalp wound.]

Jan. 9th, 1857.—General rigidity of muscles increased. He was constantly crying out, being in great pain. He had not slept during the night. The muscles in the perineum were extremely rigid, and it was necessary to remove the urine with the catheter. Though the muscular spasm was continuous, it was greatly increased in intensity on any one touching him, or even approaching him to alter his position. There was great pain about the diaphragm, with hiccough. Skin hot and dry; bowels only acting after an ene-

ma; pulse 120, and thready. There was a small collection of matter near the wound, which was opened, and about a drachm of pus evacuated.—Quarter to five p. m.: One-twelfth of a drop of nicotine was administered, the pulse being 120.—Quarter past five p. m.: the dose was repeated; pulse 108.—Quarter past seven: pulse 112. One-eighth of a drop of nicotine was given.—Nine p. m.: The pulse was 68, and slightly intermittent. He was sweating profusely. There was much less muscular rigidity, and he was enabled to pass his urine naturally, which he had not done for thirty hours. The nicotine was discontinued for the night.

10th. Nine a. m.: The pulse had risen to 110. He had slept comfortably for several hours. Muscular rigidity certainly less than on the previous day. Skin moist and perspiring. No nicotine was administered during the day; but in the evening one-twelfth of a drop was given, the pulse being noted at 132.

11th. Passed a tolerably good night, and remained comfortable during the day. Pulse 110. One-eighth of a drop of nicotine to be given at night.

12th. Had slept for several hours. Pulse 92, and regular. Towards the evening of this day he did not appear so well. The general muscular rigidity was much increased, and it was necessary again to make use of the catheter. Pulse 115. The nicotine was repeated (one-eighth of a drop) twice during the night.

13th. Half-past 11 a. m.: He had slept for several hours, and was decidedly better. Pulse 100. He was again able to pass his water naturally.

Without going more into detail, I may say that from this date his improvement was uninterrupted, and he left the hospital quite well in six weeks. The nicotine was given occasionally, being invariably followed with relief. During the whole course of the disease a generous fluid diet was given, without stimulants.

In this case, although symptoms did not show themselves till the thirty-second day after the injury, yet they were of a sufficiently aggravated character to lead me to apprehend a fatal result. The morphia in the first instance appeared to give relief; but on its failing, I fell back on the tobacco treatment as a last resource. In administering the nicotine (which was procured for me with great promptness by Messrs. Clay and Abraham), I found that one-twelfth of a drop was quite sufficient to produce a decided effect, and at no time was it increased to more than one-eighth, dissolved in alcohol and water. An hour always intervened between the doses. I mention these points for the guidance of those who feel disposed to try the plan of treatment. The effect of nicotine on the system is of so depressing a nature, especially in the case of those unused to tobacco, that it is most important not to give the least quantity more than is absolutely necessary.

The effects of the nicotine, as observed in this case, may be summed up as follows:—

1st. A diminution in the frequency of the heart's action. On the 9th of January, in four hours and three quarters, the pulse fell from 120 to 68. This was most marked on the first day; but the same effect was more or less invariably observed consequent upon its administration.

2ndly. The muscular spasm was invariably diminished. This was generally evident, but more especially by the voluntary discharge of urine, and by the breathing becoming easy and unaccompanied with diaphragmatic pain.

3rdly. A tranquillising effect on the nervous system generally.

4thly. It promoted the action of the skin, though I never detected the "snuffy odour" of the excretion as mentioned by others.

Such are the more prominent particulars in this case. I find there are many recorded cases where a recovery followed the use of tobacco; and some speak highly of its beneficial effects as an external application, in the form of a poultice to the wound by which the tetanic symptoms were produced. From the observation of many cases of this distressing disease, *should* certainly feel more confidence in again adopting the treatment that *have recorded* than any other I have seen tried.—*Lancet, Nov.* 9, 1867 *p. 578.*

13.—ON CASES OF TRAUMATIC TETANUS TREATED WITH THE ORDEAL BEAN OF CALABAR.

By Dr. Eben. Watson, Surgeon to the Royal Infirmary, and Lecturer on Physiology in Anderson's University, Glasgow.

[Dr. Watson has now treated six cases of tetanus with the Calabar bean—all successfully. The first case related in the following paper is that of a boy, 9 years of age, who had a contused wound on the sole of the right foot. Two days after admission the report is,]

At 7.45 p. m., of the 8th of June, the patient had a distinct fit of opisthotonos, which lasted for about five minutes. At 9.45 he had another similar fit, and during it he rolled over on his side. At 10.30 the treatment began with four drops of the tincture of Calabar bean, to be repeated every hour. He had several fits of opisthotonos during the night, and next day the dose was increased, first to five drops, and then to ten drops, every hour. He also had a drop of croton oil in the evening, by which time it is noted that there is no difficulty of swallowing; pulse 96.

10th. The patient slept for periods of two or three hours several times last night, and therefore only took the tincture of the bean six times. At 4.20 a. m. he had a slight fit of opisthotonos. The bowels have not been opened by the croton oil, but he has taken food pretty well, even chewing a small piece of bread; pulse 84.

11th. The treatment was continued yesterday, and he had only a slight fit at 6 p. m. He took a dose of the tincture at 8 p. m., and slept from that time till six in the morning, when he had half a drop of croton oil in half an ounce of castor oil, and the doses of the tincture were resumed every hour. The patient is rather more rigid than usual this morning, and opisthotonic spasms are readily induced by the slightest touch of any part of his body. At 4.45 p. m. he still continued universally rigid, and the dose of the tincture was increased to fifteen drops; but at 6, 6.45, and 8 p. m. the patient had fits of opisthotonos, the last fit being very severe, and lasting for about ten minutes. During the fits there was very profuse perspiration, and great rapidity of the heart's action. The bowels acted freely once, and the patient was able to take a little bread and meat-juice for dinner.

12th. The patient slept well during last night, but was roused every two hours to get the tincture of the bean. He had no fits, but occasionally started. This morning the muscles generally are much more relaxed than they have yet been; pulse 80.—Evening: No fits to-day, but starts when disturbed. Has had 15 drops every hour to-day. Bowels not opened.

13th. Had no fits last night, and slept well, but had his medicine every two hours. Seems improving this morning.—10 p. m.: The patient has had fifteen drops of tincture every hour to-day. To-night, when asleep, the jaw was completely relaxed, so that the mouth could be opened widely without force or waking the boy. The respiratory movements of the chest were likewise more natural; pulse 68, weak, and somewhat irregular; the pupils were very much contracted, but whenever the patient woke out of sleep he became rigid as before. The administration of the bean was suspended at 9 p. m., and at midnight he had half a drop of croton oil in half an ounce of castor oil.

14th. No fits last night. Bowels freely opened this morning. The tincture of the bean, in fifteen-drop doses, has been given every two hours till 8 a. m., and afterwards every hour. Pupils rather dilated. Could eat porridge, and even some bread, for breakfast this morning. At 3 p. m. the tincture was changed for that which had been used in a previous case, and as it was supposed to be stronger than that which the boy had been taking, the dose was diminished to six drops—a dose which had been effectual in the other case; and it should be remembered that the patients were about the same age, as well as under the same disease. This dose, however, proved insufficient in the case I am now relating; for the patient had a severe fit of opisthotonos at 1 a. m. (15th), and remained rigid for some time thereafter.

The dose was therefore immediately increased to eight drops every hour, but even that did not seem to produce the desired relaxation. The tincture formerly used was now resumed in the old dose of fifteen drops, and this was increased to twenty every half hour during the day and every hour during the night.

On the 18th the dose was increased to thirty drops every hour with good effect. During the last four days the patient gradually recovered from his relapse, apparently caused by the sudden diminution of the dose on the 14th. He had no decided fit of opisthotonos, except the one reported on that day; but he had for a time greater rigidity and opisthotonic starts. He is reported to be much more relaxed on the 18th than he had previously been; but it is noted that rigidity, especially of right limb, occurs when the wounded foot is washed, though that operation is performed in the gentlest way—simply by pouring tepid water over it. It is then laid in clean water dressing.

On the 19th, at 9 a. m., the patient being as above reported, the dose of the tincture was diminished to twenty drops every two hours; and he was ordered four ounces of sherry daily. At 3 p. m. he had two short fits of opisthotonos, and remained afterwards very rigid all over the body. The dose of the bean was therefore again repeated every hour during the day, and by the evening he was once more relaxed in all his muscles, and was able to separate the teeth about an inch. As his pulse was feeble he had then three ounces of brandy, which were given during every night for a time.

Next day he was again rigid and starting more readily, and this state continued during the 20th, though the dose of tincture was increased to thirty drops.

On the 21st, his bowels having been opened by croton oil, I ordered the *tincture* to be discontinued, as it did not seem to be sufficiently strong to combat the disease in this case, and I prescribed instead of it the alcoholic extract dissolved in spirit. Of this he took a dose equivalent to one-eighth of a grain of the extract every hour, and after three such doses it is reported that he became much more relaxed; and at 10 p. m. he was ordered one-sixth of a grain every two hours during the night, to save waking him too often.

On the 23rd the patient continued to improve, and chewed a steak for dinner, Appetite good, but bowels again confined. He therefore had a dose of castor oil alone (half an ounce), and has continued regularly the doses of the extract prescribed on the 21st.

After this date he improved, though rather slowly, the muscles of the abdomen and back being the last in this case to give way. The dose of the extract was regularly given, and gradually increased till the patient took half a grain every two hours. This was from the 21st of June till the third of July, or twelve days. The effects produced were thorough relaxation of the muscles, occasional vomiting, without any great or at all events long-continued sickness—for the boy had a good appetite all the time,—and a noteworthy change as to the action of purgatives, which at first hardly acted, though strong, and by-and-by acted plentifully, though only of usual strength; latterly none were required at all. I think these circumstances show that, as the drug overcame the tetanus, it began to exhibit its physiological action on the alimentary canal—viz., a degree of irritation indicated by abundant secretion of mucus, and evacuation of both stomach and bowels.

On the 3rd of July, therefore, I stopped the administration of the bean entirely, and ordered an increase of stimulants and gentle nourishment. Under this treatment the symptoms just mentioned soon subsided and disappeared. In a few days he could sit up freely in bed. He slept well at night, without even starting, and took his food with relish and ease. The wound of his foot had by this time nearly healed, and by about the middle of July he could rise from bed, and very soon ran about the ward as if he had never been ill. On the 30th he was dismissed cured.

It will not, I think, be questioned by any one that the preceding was an *acute case of* traumatic tetanus. The patient was admitted to the infirmary *on the second* day of the disease and he had already severe general

convulsions. The administration of the bean was at once commenced by Dr. M'Gibbon, my house-surgeon, and henceforward the severity of the disease was mitigated, though in this case its progress was not at first very decidedly checked. The reason of this was twofold. 1st. The smallness of the doses of the bean. I wished to try if we could not counteract the tetanus without throwing the patient into such a state of alarming depression as occurred in my first case; but we found that while these small doses mitigated and checked the violence of the fits, they did not altogether prevent their recurrence. Probably this was very much caused by (2nd) the severity of the disease in this case. I have elsewhere shown that the Calabar bean produces the exact counter-effect on the spinal marrow to the state in which tetanus originates. Hence when the latter is very well marked, so to speak, the former must be the more fully developed. Thus each case must be judged of pretty much by itself, and the doses of the bean proportioned to the severity of the disease in every individual instance. In the case just related the disease was, in my opinion, of a very severe type, and hence it was that the doses of the bean given at first only served to keep it in check. They were not powerful enough to conquer it altogether, and had they been neglected, the disease would soon have shown itself in all the distressing force so familiar to those acquainted with its phenomena. This statement, as well as the fact of the controlling influence of the bean, could not be better illustrated than by reference to the case itself. On the 11th of June the patient had a very long sleep, during which the drug was not given from 8 p. m. to 6 a. m., and in consequence the fits recurred, and the rigidity became worse. Again on the 14th and 19th, owing to the diminution of the doses, similar relapses occurred. Lastly, the duration of the case was much shortened by the increased strength of the dose towards the end of the treatment, when the extract was used instead of the tincture. Now all these relapses could not be mere coincidences with the diminution of the quantity of the bean taken by the patient. On the contrary, I think that unprejudiced readers will perceive a direct connexion between the diminution of the drug and the relapses, while the increase of the drug and the recovery are also most obviously related as cause and effect.*

It must be acknowledged that much has yet to be done in the way of procuring a perfectly reliable form for the therapeutical administration of the bean. In the recent edition of the British Pharmacopœia, I perceive that the powder of the bean and the alcoholic extract are the only preparations mentioned. In my earlier cases, and even for a long time in the preceding one, I employed a tincture made as recommended by Dr. Fraser, of Edinburgh, by percolation of the powdered bean with spirit; but I fear that this must be a preparation of very uncertain strength. And it was owing to this doubt that I changed it for the extract dissolved in spirit towards the end of the case just related. Eight grains of the former gradually rubbed down in and mixed with an ounce of spirits of wine, form a solution or tincture, every ten minims of which contain one-sixth of a grain of the extract. The dose can thus be easily regulated and given safely in solution. I do not, however, think that this tincture would continue of equal strength, as it by-and-by throws down a muddy deposit. I have not experimented with or otherwise tested this deposit; but I always had a fresh supply of the tincture sent to the ward every twenty-four hours. I believe that such a tincture as that now described is more certain and reliable than the tincture made by percolation; and, if it be found to retain its strength after filtration of the turbidity referred to above, it will be both as elegant and as permanent on keeping.

I shall now conclude with a short account of the fatal case referred to in the beginning of this paper.

On the 15th of September, about half an hour after midnight, a man, aged 30, was brought to the hospital in violent tetanus. The ball of his great toe

* A case of traumatic tetanus has recently been reported which was treated entirely by hypodermic injections of tincture of Calabar bean. The result was quite satisfactory, and forms the seventh case treated by the bean, with only one death.

had been slightly cut with a rusty nail about a fortnight previously, but was now nearly healed. The wounds were skinned over, though still red. He had been quite well, only very confined in his bowels, till two days before admission, when the jaw became stiff; and in due time he had general convulsions. These were much aggravated by his transmission to the hospital, so that when seen there by the house-surgeon of the ward into which he was brought, he was in a very exhausted state, and steaming with perspiration. The fits occurred almost incessantly, and were very severe. He could not swallow either stimulants or the tincture of Calabar bean. The latter was soon procured, but, from this obvious cause, its administration was unsuccessful.

When I saw the patient in the morning, about 9 o'clock, his pulse was very rapid and feeble, his face of a livid hue, and the tetanic fits were continual, and so severe that the man himself declared he was dying. I gave him half a grain of the extract of the bean dissolved in spirit and diluted with a tea-spoonful of water. He swallowed that with some difficulty, and, from his doing so, I hoped that we might yet gain time for the action of the drug; but in this I was mistaken, for he died in a severe convulsion which came on about half an hour after my visit.

When I heard of the great difficulty which this man had in swallowing, I at once thought of the hypodermic method of administering the bean. But I delayed carrying it out, because the man did take a tea-spoonful of fluid from myself, and I hoped he would continue to swallow a little, of the stimulants especially, until I returned, as I appointed with the house-surgeon to do after a short interval, to inject the tincture of the bean beneath the skin. I should have done so at the nape of the neck, and I intended to have introduced ten minims in two portions. I regret that I did not try this expedient at my first visit, for I never had another opportunity. I may add, however, that the hypodermic administration of this drug is peculiarly applicable to such cases as present great difficulty of swallowing; for it is both certain and rapid in its effects. If another such case were to occur to me I should employ this practice at once without delay, warned by the present case that the patient's life may at any moment be cut short by tetanic spasm of the heart itself.

It can hardly be said that the bean was employed at all in this case, and so far as its operation is concerned it proves nothing. One lesson, however, may certainly be enforced from it, and that is the importance of commencing treatment with the bean in the earliest stage of this disease. The symptoms of that stage are quite characteristic and evident to one who has watched them but a few times, and no delay should be permitted before the administration of the bean. We should not wait even for the action of a purgative, desirable though that be, but ought to give the bean at once, and the purgative as soon as may be afterwards. It is only thus that we can have a good hope of success; for when once the disease has firmly established its power over the nervous system, it is difficult in all, and impossible in some cases to overcome it. This fact was well proved in some of my experiments with strychnia, related in the Edinburgh Medical Journal. When strychnia was first given and allowed to act, I never could restore the animal; but by giving even a fatal dose of strychnia, and at the same time a certain dose of the Calabar bean, I rescued the animal from the fatal effects of the former.—*Lancet, April* 4 *and* 11, 1868, *pp.* 434, 463.

14.—CASE OF GENERAL PARALYSIS SUBSEQUENT TO CHOLERA.

By Dr. Walter G. Smith, Dublin.

[The patient first passed through a severe attack of cholera, but a few weeks after recovery his attention was attracted by his lower limbs failing in strength This increased rapidly until he was unable to stand without assistance.]

During the next week he rapidly lost power over all the extremities, became so helpless that he was unable to turn in bed, and if his feet were crossed, he was quite incapable of separating them.

His condition at that time was really distressing; completely paralysed, he passed some days in an almost vegetative existence; motionless, not even possessing strength enough to draw the bed-clothes over himself, yet with his mind clear and active. He was much annoyed with severe tensive pain in the feet, which was completely relieved by friction, and the slightest movement or exertion caused intense pain and distress. At no time were the involuntary muscles seriously affected, though for two days, after he came up to town, he partially lost control over the rectum and retained the fæces with difficulty. He could always command the functions of the bladder, but the sensibility of the urethra was evidently diminished at one period, for he could scarcely feel the contact of the urine with the mucous membrane as it passed out of the body.

His urine, I regret to say, was not examined.

One of the most remarkable points connected with this case is the following:—

During a period of about ten weeks, for a portion of almost every day, he was unable to introduce the smallest particle, solid or fluid, into the stomach. Whatever he swallowed passed readily down the œsophagus, but near its termination seemed to be suddenly arrested by an impediment in, or constriction of, the passage, through which not an atom found its way. A few minutes after deglutition, part of the food regurgitated into the mouth, the act being attended with slight hiccup, and in a short time the whole was returned.

[Various means of treatment were adopted in the case.]

Finding the above means unsuccessful it was decided, at my father's suggestion, to have recourse to electricity, and the first trial of it was made on the 25th of last February.

The apparatus I used throughout the case was constructed by Messrs. Elliott of London.

It consists of a single Smee's element, the amalgamated zinc plates and the central platinised silver lamina, being about $4\frac{1}{2}$ by $1\frac{1}{2}$ inches each, thus exposing a surface of nearly seven square inches. They are excited by dilute sulphuric acid; one part of oil of vitriol to eight parts of water. The poles of the battery are readily put into connection with the thick primary or inducing wire of an upright induction coil, and the electrodes can be so arranged that the operator may collect at pleasure, either the extra current developed by the mutual action of the spirals of the primary wire on each other, or the more intense current induced in the fine secondary wire, or, if he wishes, can call into action a combination of these two currents.

The intensity of the induction stream is increased by the presence of a bundle of soft iron wires, in the centre of the bobbin, while the influence of this central temporary magnet is regulated by withdrawing more or less of a copper envelope which closely surrounds these wires.

The necessary interruption of the battery current is secured by an ordinary trembler or rheotome, and the rapidity of the intermissions is governed by a screw which alters the distance of the trembler, from the central core of soft iron. The whole fits into a box 7 inches in height, 6·5 inches in width, by 4 inches in depth, portability and lightness being specially aimed at in its construction.

The slight inconvenience arising from the necessity of using acid with such a machine, as compared with the rotating magneto-electric machines is, I believe, far more than counterbalanced by the direct advantages resulting from the employment of a volta-electric arrangement, for

1. The latter can be worked without any assistant for hours at a time.
2. Its action is more uniform, and more easily regulated.
3. The current evolved from it does not cause so much pain, or such irregular muscular contractions as the magneto-electric current.
4. The ordinary magnetic machines are more liable to get out of order,

considerably greater influence on the motor nerves than the s
only.

The right and left extremities were usually excited on alternate
the total number of applications, extending over six weeks, was as f
Lower extremities about thirteen times, right arm about twelve t
left arm about nine times. It is right to mention that for the gre
of the time during which the treatment by electricity was pursued
no medicine of any description, with the exception of a few diuretic
(soda bicarb., pot. nitr. aa. gr. v.) for the relief of some temporar
of the feet.

A liniment of aconite and chloroform was rubbed in along the s
day or two, but beyond this no internal or external medication was
to.

The steps towards restoration to health were encouraging, and
progressive.

Very shortly after the employment of the induced current the arı
to gain in strength, and in about a fortnight he could stand with a
though still with a tendency to fall forwards.

Gradually and slowly he regained the power over the lower ex
and simultaneously there was a marked increase of susceptibility to
tric influence.

It was not till a week later that he was successful in moving fo
step or two without assistance, and with his eyes shut.

Two marks of improvement he especially noted himself, viz.:
hands lost the yellow hue which they exhibited in the early periods
ment, and that he no longer suffered from cold feet. The circumf
the limbs increased rapidly, and within six days the right arm had
the extent of three-quarters of an inch. The calves of his legs b
resumed their natural dimensions, and on the 27th March he fe
enough to drive out for a short time.

As soon as he was able to write firmly the faradisation of the
omitted, and on the 11th of April I saw him for the last time, an
that he had walked two and a-half miles the day previous in an ho

cords of electro-therapeutics is, I am convinced, traceable to the carelessness and inaccuracy of writers with reference to the form of electricity selected, and the precise manner of applying it.

Dr. Althaus, in his valuable work, gives some striking examples of errors arising from this source (see p. 163 and 343). Electricity is an agent potent for evil as well as for good, according as it is directed, and though when unadvisedly employed, it too often becomes an instrument of much mischief, yet, under judicious management, it is a tractable as well as a valuable servant, and it is especially remarkable as being one of the few potential remedies which the physician can command.—*Medical Press and Circular, Feb. 26, 1868, p. 180.*

15.—CARBOLIC ACID A CURE FOR TOOTHACHE.

[A correspondent writes to the Lancet as follows:]

Among the many virtues of carbolic acid is that of giving relief from the pain of toothache. I have tried it in a great many cases, and with invariable success. To one drachm of collodium flexile (B.P. 1867) add two drachms of Calvert's carbolic acid, full strength. A gelatinous mass is precipitated. A small portion of this precipitate inserted into the cavity of an aching tooth gives immediate relief. It may be kept in the cavity by means of a bit of lint dipped in the collodium.—*Lancet, Feb. 22, 1868, p. 275.*

DISEASES OF THE ORGANS OF CIRCULATION.

16.—ON THE PHYSIOLOGICAL DIAGNOSIS OF CARDIAC VALVULAR DISEASES.

By Dr. W. T. Gairdner, Professor of Practice of Physic in the University of Glasgow.

[No class of diseases requires more careful study and more care in forming both diagnosis and prognosis than does that of the heart. Most diseases of the valves are the result of endocarditis, but the amount, situation, and character of the lesion, as well as all the attendant circumstances of the case, must be taken into careful consideration. The study of murmurs, with great care and exactness, is necessary to preserve us from serious errors. A murmur always has a cause. It is a matter of reasoning in the particular case what that cause is.]

1. First, of *Aortic disease*; in which, as you know, there is usually a very distinct murmur, single or double as the case may be, VS, or VD, or VS and VD according as there is obstruction, or regurgitation, or both together. In aortic disease you have the orifice through which the blood passes from the left ventricle into the arteries in some way or other interfered with; and you might easily infer from this, that the tendency of aortic disease will be towards some kind or degree of interference with the systemic circulation. And in point of fact, it always is so to this extent, that in aortic disease sufficiently severe to derange the circulation at all, it is always the force and completeness of the systemic circulation that is first interfered with. But you must not estimate the force, or the completeness of the systemic circulation solely by the prominence, or apparent strength, or fulness, of the radial pulse; nor can you estimate it accurately through the apparent strength of the heart's action. The reason is easily explained. In aortic disease, whether obstructive or regurgitant, when it affects the force of the systemic circulation, you invariably have a compensating hypertrophy of the left ventricle. I call it *compensating*, because the first effect of aortic disease being always to a greater or less degree to restrict the flow of blood through the arteries into the systemic capillaries, or in other words, to diminish the tension of the arterial system generally, and thus to interfere with the distribution of the systemic blood, there is always a spontaneous impulse on the part of nature to overcome this difficulty. That spontaneous impulse takes the effect, after a time, of a habitually increased action and hypertrophy of the *left ventricle*. You may say that it is an almost invari-

able rule, that aortic disease, of whatever kind, tends to hypertrophy of the left ventricle; and accordingly, if you find a murmur of some standing, which has given rise to serious symptoms without more or less evident hypertrophy of the left ventricle, it becomes rather probable that the murmur, however it may resemble that of aortic disease, does not, in fact, originate in the aortic orifice. And if you find a murmur, or combination of murmurs, associated with hypertrophy of the heart, but with the right ventricle predominating; there might in such a case be aortic disease as one element, but it would be extremely probable that there was some other element, qualifying the aortic disease, and modifying its after-effects. Now, in aortic disease occurring alone, it often happens that the pulse is apparently not affected, or at least does not become feeble up to a very late period; the only obvious effect that follows the disease is this relative hypertrophy of the left ventricle. Sometimes, though rarely, there are not even any serious symptoms; a little palpitation, a little sense of vague uneasiness, sometimes a little tendency to faintness or giddiness, may be all that is observed. The explanation is, that there is in these cases only a comparatively slight amount of obstruction, and that it is under ordinary circumstances exactly met, or compensated, by the ventricular hypertrophy. I have known a man suffer (if suffering it can be called) from such a slight form of aortic disease for a quarter of a century, knowing, too, all the time that he had a cardiac murmur; and yet, so slight has been the interference with the circulation, or so perfect the compensation, that he did not undergo any of the usual consequences of cardiac disease, and died at last of a different kind of disease altogether. This case is no doubt exceptional, but it was that of a medical man, and was very accurately observed. The usual fact, unhappily, as regards aortic valvular disease, is that it proceeds through a series of secondary changes, of which the hypertrophy of the left ventricle is perhaps the earliest in date, towards a fatal issue.

The tendency to diminished control of the heart over the systemic circulation is characteristic of aortic disease in a very special manner, as compared with other valvular diseases; and the compensating hypertrophy of the left ventricle is accordingly a characteristic of the same group of cases. But the tension of the arterial circulation, and the pulse, are differently affected, according as obstruction predominates over regurgitation, or regurgitation over obstruction. The direct effect of aortic obstruction, supposing it to exist alone, is to make the pulse smaller in proportion to the degree of the obstruction; and in extreme cases, where the orifice is reduced to a mere chink, this effect is actually witnessed, it may be said, of necessity. But as in moderate cases of obstruction the effect is modified by the hypertrophy of the left ventricle, it often happens that aortic obstruction does not produce a pulse appreciably smaller than natural. The degree to which the pulse is affected in simple obstruction is dependent on two circumstances; on the one hand, the degree of the obstruction; on the other hand, the failure of the left ventricle to maintain the full current of blood through the partially obstructed orifice. It is a complex problem in mechanics; but the practical result is that, so long as the obstruction is moderate, and the left ventricle maintains its power, the pulse is little, or may be not at all, affected; but when the obstruction is considerable, or when the left ventricle passes into the state of dilatation with diminished power, the pulse becomes small and feeble. But in aortic regurgitation the case is quite different; and this is on the whole the more common, and by far the more dangerous, case. Here you have, as in the other case, a labouring ventricle, and a hypertrophied heart; but the labour is in vain; for at every impulse the ventricle sends a large quantity of blood into the artery, only to fall back again into the ventricle through the incompetent valves. The consequence is that, when regurgitation is extreme the artery cannot hold, cannot transmit, the blood it receives; there is a large over-full ventricle, expelling at every systole a great volume of blood into the aorta; but the smaller arteries are not fed, the systemic circulation is not profited thereby. The pulse is large and full, in accordance with the increased impulse and increased

delivery of blood; but it is, at the same time, soft, undulating, and compressible, because the tension of the arteries is not maintained. The tension is not maintained, because the blood falls back towards the heart, instead of being passed on towards the capillaries. The arterial system is always unfilled, although the heart is working at high pressure, and the aorta is usually dilated. Such is the theory of the "pulse of unfilled arteries"; so accurately described by Dr. Hope as characteristic of aortic regurgitation. The pulse is large and full, because the impulse *a tergo* is increased; undulating and soft, because the tension of the artery is diminished. The expansion of the artery at each pulse of the heart is considerable; but the constant pressure, depending on the fulness of the whole arterial system, is much reduced in amount, and the capillaries, accordingly, are starved of their supply. In this state of matters the blood tends to accumulate in the left ventricle, and behind it. It stagnates, so to speak, within the heart. It does not get on. A great effort is made by the heart, but the force of the impulse is lost in partially distending the aorta and larger arteries; it does not reach the smaller vessels; it does not fill the capillaries. Hence the patient is almost always pallid, anæmic-looking. To judge from his face and surface, you would say that he was bloodless; and the capillaries of the surface are in fact very imperfectly supplied with blood in such cases. But examine the heart, the lungs, the liver, and you will find that a large portion of the blood of the body is being heaped up there. The heart is gorged, over-worked; its pulsations are frequent, agitated, labouring; but it is the labour of Sisyphus; the burden always rolls back. It is difficult to avoid a somewhat figurative language here, but as I am satisfied that the figure is correct, I will pursue it further. The heart, thus worked constantly to the very summit of its power, or even beyond its power, has its innervation affected. It loses hold of its work; becomes *conscious*, as it were, that it fails to keep up the systemic circulation. The sensory nerves reflect these impressions through the ganglia, and the motor nerves respond by new efforts, but often by feeble and ill-sustained efforts. The heart *gets into a fright*, as it were, and is at the same time exhausted. Perhaps also its own nutrition is directly affected, through the inadequate circulation, and the inequalities of pressure in the coronary arteries. Then begins a new and terrible symptom. The patient gets anguished and distressed to a frightful degree. Often there is no positive pain, but in other respects the symptoms are those of *angina pectoris;* and this form of heart-suffering is, I think, peculiarly, though not exclusively, characteristic of aortic regurgitation. The anguish gets written upon the countenance; you see that the patient has never a moment's ease; it is a look at once of distress and of exhaustion, which, once seen, is not easily mistaken. If not relieved, it is hardly probable that a patient can survive long in this state; yet I have known it last for days, and in a modified sense for weeks, during which little or no sound sleep has been obtained, though from sheer weariness the patient was always dropping off to sleep for a moment or two. He will tell you sometimes that he is *afraid* to sleep; and I have known a man in this state actually desire his attendant to keep him awake, for fear he should sleep the sleep of death. With all this, there may be no lividity; most commonly there is little dropsy; and it is only after a considerable time, and in certain cases, that there is hæmoptysis. A labouring heart, an imperfect systemic circulation, and the functional symptoms that I have endeavoured to describe; these, with the characteristic murmurs, form a typical picture of a case of aortic regurgitation. Now, although there may be sudden death in this state, or death from gradual exhaustion and sleeplessness, you are not to suppose that the majority, or even a large proportion, of patients with aortic regurgitation die suddenly. On the contrary, many of them survive in a very extraordinary way for a long period. The heart becomes enormously large; the left ventricle, from being hypertrophied chiefly, becomes dilated; the apex becomes much rounded, and the apex-beat diffused or undefinable; there is heaving movement over the whole precordial region. One of the consequences *of this further* development of the disease is that the mitral

valve, which may have been quite competent hitherto, tends to give way; and you may find a mitral regurgitation-murmur (VD) superadded to the aortic murmur. There is not necessarily any deposit on the mitral valve; its incompetency in this case is purely mechanical; a result of the dilatation of the left ventricle. But another result, which either follows or precedes the leaking of the mitral valve, is increased congestion of the lungs, and often hæmoptysis; and afterwards you may have all the other sequelæ of disease of the heart in their most extreme development. Such is a description, open to correction, no doubt, in particular cases, but on the whole, I think, according to nature, of the course of development of aortic disease.

2. Let us now turn to *Mitral Disease*. In mitral disease you may have an AS murmur, indicating obstruction, or a VS murmur, indicating regurgitation; or you may have both murmurs together, and perhaps in a few cases a VD murmur superadded, indicating a complicated condition of obstruction and regurgitation, as already explained. In mitral disease the systemic circulation is less directly involved than in aortic disease, and the pulmonary circulation more directly. Further, the evil effects of mitral disease, whether as regards the pulmonary or the systemic circulation, follow much more rapidly, as a rule, from regurgitation than from obstruction. Why is this? you will ask. Chiefly, I think, because the filling of the ventricles from the auricles is a slow process, which may, in cases of difficulty, extend over the whole of the long interval from the 2nd to the 1st sound. In cases of moderate obstruction, therefore, the filling of the ventricle, though it goes on more deliberately, and with a somewhat increased *vis a tergo*, is not necessarily very imperfect. I have known, indeed, the whole of this long interval occupied by murmur, showing that the filling was very slow, and yet the patient has appeared not much the worse. The auricle and great veins in such cases, no doubt, accumulate a larger reserve of blood; the tension of the pulmonary venous system is increased, and the auricle usually becomes hypertrophic; but, after all, the work is done, and done pretty completely; the ventricle fills at each diastole, or rather before each systole, and the circulation goes on. Hence, it is only in very extreme cases of mitral obstruction that you find after death, what perhaps you would expect to find in the majority of cases, a reduced capacity of the left ventricle. When this is the case, when the left ventricle becomes permanently atrophied in connection with mitral obstruction, it is evident that the obstruction has gone very far; that the auricle and great veins have not been able to throw enough of blood into the ventricle to maintain its full capacity and force. In most cases of mitral obstruction, however, the left ventricle preserves its size and capacity, and it may even, especially if regurgitation be also present, become enlarged or dilated. When mitral regurgitation, again, is present without obstruction, the left ventricle is almost always dilated, and often very much dilated. Here, as in the case of aortic regurgitation, there is a labour of Sisyphus; the ventricle is regularly filled at each diastole; it may be, overfilled. It then contracts and endeavours to empty itself; but the emptying is as much, or it may be more, towards the lung than towards the arteries; and both the pulmonic and the systemic circulation suffer accordingly. Hence I have been accustomed to observe, and I think the observation is true in fact, whatever may be the theory, that even a considerable degree of mitral obstruction, without regurgitation, is much more easily tolerated by the system than a corresponding degree of regurgitation, without obstruction. And conversely, among cases of mitral regurgitation, unquestionably the least favourable are those in which there is a dilated orifice, and the most favourable are those in which the orifice is contracted. It is scarcely too much, therefore, to infer that a moderate degree of obstruction in the mitral orifice, though, of course, always an evil *per se*, tends in cases of regurgitation through the same orifice to limit the amount of the regurgitation, and therefore to restrain the yet greater evils springing from regurgitation.

In mitral disease, then, it will depend very much upon the character of the *disease whether* the systemic circulation and the left ventricle will be much *involved or* not. If the disease be obstruction merely, and moderate in

gree, there may be little or no detriment to the systemic circulation. If the disease be obstruction with regurgitation, the systemic circulation will be involved mainly in proportion to the amount of the regurgitation—the obstructive disease usually acting in such cases as a limit, or barrier, to the injurious effects of the other. In the case of regurgitation simply, the amount of the evil will reach the maximum, and will be proportionate, in some degree, to the amount of the regurgitation, and the degree of coincident dilatation of the left ventricle. In all cases, however, of mitral disease, whether obstructive or regurgitant, there will necessarily be a certain amount of increased tension, or over-distension, in the left auricle and pulmonary veins, owing to the increased resistance at the auriculo-ventricular opening, and the stasis of the blood behind the point of resistance. Whatever be the state of the systemic circulation, it is thus inevitable that in mitral disease the pulmonary circulation shall suffer. Permanent distension of the auricle; dilatation of the veins and capillaries; hypertrophy first, and finally dilatation with over-distension of the right ventricle—such is the order of events to be looked for in all cases of serious disease at the mitral orifice. The pulmonary circulation is performed in the face of a difficulty; all the blood-vessels concerned in it become enlarged: an excess of blood is permanently lodged in the lungs, and it is transmitted through the distended capillaries at a slower rate than natural, and under an increased pressure; the blood is thus less perfectly exposed to the air in the pulmonary vesicles; cyanosis or lividity follows; frequently the distended capillaries give way, and a peculiar form of hemoptysis, with condensation (hemorrhagic condensation, or "apoplexy" of Laennec) in the air vesicles, is the result. And these changes are invariably associated, sooner or later, with hypertrophy, and ultimately with dilatation of the right ventricle; which in its turn, leads to increased tension and stasis in the systemic veins, enlargement of the liver, jaundice, dropsy of the limbs and serous cavities, albuminuria and secondary disorder of the kidneys, and often of the stomach.

In accordance with these changes are found certain modifications in the physical signs. The radial pulse, distinct and regular at first, may become small, feeble, irregular or intermitting, or it may be almost lost at the wrist; these alterations corresponding either with regurgitation to a considerable extent, or with the last extremity of obstructive disease; and in the former case being usually associated with dilatation, in the latter with relative atrophy of the left ventricle, When the pulse is thus affected, it is not uncommon for the murmurs to become indistinct, or even to be lost; partly because the circulation is nearly brought to a stand, and partly because the dilated heart does not transmit the sounds so readily as before to the site of the apex-beat. In many of these extreme cases, in fact, the apex-beat is lost, or exchanged for a diffused pulsation corresponding with the dilated right ventricle. Unless in extreme cases, and almost always in cases of moderate obstructive disease without regurgitation, the apex-beat remains distinct, often preternaturally distinct. It is in such cases that a sign is usually observed which may be regarded as the first note of undue tension in the pulmonic circulation—the loud, sharp, jarring *snap*, felt as much as heard, which takes the place of the normal second sound over the pulmonary artery. This phenomenon is much more common, and much more highly developed in obstructive than in regurgitant disease; and it is observed only when the heart is acting strongly and regularly, without any considerable dilation of the cavities, or weakening of the fibre. When dilatation occurs, the organ is *broadened* rather than *elongated;* the dull percussion is extended transversely, and both to right and left; the apex is ill-defined, the sounds are muffled and low pitched. In such cases you may expect the murmurs to become obscure, and the pulse feeble and small as the dilatation proceeds; occasionally, too, you will observe a very remarkable change in the position, and sometimes also the character of the murmur; the explanation of which is, that while the mitral murmurs are becoming obscure, and are thrust into the back-ground, as it were, owing to the dilatation of the right ventricle, a new murmur is arising in that ventricle *itself*, one of tricuspid regurgitation. Indeed, tricus-

pid regurgitation in this case bears to mitral disease almost exactly the same relation as mitral regurgitation sometimes bears to aggravated aortic disease; it is the witness of a ventricle dilated and overpowered to such a degree as to cause leaking of the valve.

3. *Tricuspid disease*, which we may take next in order, is rarely otherwise than of secondary origin, *i. e.*, the valve is rarely thickened, the orifice rarely obstructed by deposit. Even in museums, preparations of tricuspid disease in this sense are rare; and in museums their frequency is apparently overstated, as it were, from the fact that preparations of unusual lesions are much more apt to be preserved than of common lesions. In *post-mortem* examinations, continuously and carefully pursued, it may probably be assumed as a datum that tricuspid disease will be found in about one in a dozen cases of disease of the valves; meaning by disease, of course, in this instance *obvious* disease, *i. e.*, thickening, contraction of orifice, or obstruction by deposit. And further, in all but an inappreciable proportion of these cases, the disease occurs in combination with mitral or aortic disease, or both together; so that tricuspid structural disease, absolutely alone, may be said to be one of the rarest of pathological phenomena. But although tricuspid structural change is rather rare—very rare indeed, as a primary and uncomplicated form of disease—tricuspid regurgitation is by no means rarely met with in practice; partly because it is one of the forms of structural change most commonly associated with mitral disease, but still more because *incompetency* of the valve may occur, and probably often does occur, without structural change, chiefly in consequence of dilatation of the right ventricle. Speaking generally, you may say that, in the immensely greater proportion of cases in which it occurs, tricuspid regurgitation is a secondary phenomenon. It is a *leakage*, not a *deformity*, of the valve; and it is a leakage depending commonly on dilatation of the right ventricle. Therefore, speaking still in general terms, the causes of tricuspid disease (for obstruction of the orifice may be dismissed from consideration at present as extremely rare) are simply those of dilatation of the right ventricle; and among these we have seen that mitral obstruction and regurgitation assume a very high importance. It is has never been quite clearly shown whether tricuspid regurgitation is, or is not, always attended by murmur; but, as a matter of fact and experience, I can assure you that it is not very uncommonly attended by murmur; and that the murmur of tricuspid regurgitation is associated in a large proportion of the cases in which it occurs with that of mitral regurgitation, or perhaps still more frequently with that of mitral obstruction. To detect this association it is necessary, of course, to be very careful; for the murmur of mitral regurgitation closely resembles that of tricuspid regurgitation, and in some cases the diagnosis absolutely cannot be made at all except as a matter of inference, and with a chance of error. But in other cases the diagnosis is not at all difficult, if you carefully observe the rules and principles laid down in a former lecture; and the certainty I have that this distinction can be made very accurately in some instances, while in others it is difficult or impossible, has led in my mind to a belief that the mitral and tricuspid murmurs have frequently been confounded by auscultators; or, to speak more correctly, the latter have been frequently overlooked. The cases in which the distinction is most easy are those in which there is a mitral obstruction-murmur with tricuspid regurgitation; the AS murmur being heard at one point very distinctly, and the VS murmur equally distinctly, but at quite another, and more or less remote, point of the chest the first at the apex only or chiefly, the second only or chiefly over the right ventricle. It is, however, quite possible in some cases to make out the tricuspid regurgitation-murmur, when occurring along with the mitral regurgitation-murmur only; and, what will strike you as more problematical, it is possible, though difficult, to distinguish in the same case mitral obstruction and regurgitation from aortic obstruction and regurgitation, and tricuspid regurgitation from both of these. This I have proved by examination after *death in* more than one instance.

The only other determining cause of tricuspid regurgitation-murmur which

has sufficient clinical importance to be noted here, is emphysema of the lungs. Speaking in a practical sense, almost all that I know about tricuspid murmurs is their association with mitral murmurs on the one hand, and with emphysema on the other. And the nature of this association is not doubtful; it depends evidently on the tendency to dilatation of the right ventricle which accompanies emphysema of the lungs in many cases. Indeed, the chief difficulty in this matter is not to explain the presence of the murmur, but to account for its absence in cases in which a murmur might reasonably be expected to be present. For it is quite certain that you may have emphysema of the lungs, and widely dilated right ventricle such as almost necessarily implies tricuspid regurgitation without any murmur at all.

It is common in books to describe pulsatory movement of the veins in the neck as characteristic of tricuspid regurgitation. This, I am almost sure, is not correct. Pulsation of the jugular veins is, no doubt, frequently associated with tricuspid regurgitation, inasmuch as the latter depends usually upon dilatation of the right side of the heart. But by far the most striking case of exaggerated venous pulse that I ever saw was one in which I am much disposed to think there could have been no considerable tricuspid regurgitation, though there was a very manifest obstruction at the tricuspid orifice. In this case the pulsation must, I think, have been due to a dilated and thickened right auricle. A lesser degree of pulsation in the jugular, and innominate veins is frequently observed in a state of health, or at least, in the entire absence of anything like cardiac disorder; and, on the whole, I have come to the conclusion, from this and other experience, that venous pulse is not to be depended on as an indication of tricuspid regurgitation. Its presence or absence in any particular case seems to depend more on the competency of the venous valvular apparatus than on any other cause; although, of course, the closure of the venous valves is more likely to be imperfect with a dilated heart, and pulsation will be apt to be more distinct in proportion as the veins are permanently dilated, and the valves incompetent. In many cases of very considerably dilated right heart, however, I have observed that the venous valves remain nearly competent, even when the veins are much distended, and in such cases there is of course little or no pulsation.

The other symptoms of tricuspid regurgitation are altogether those of dilated heart, and it is not necessary to enlarge upon them at present.

4. Obstructive disease of the right side of the heart, whether of the tricuspid or the pulmonic orifice, is, as I have already said, rare under all circumstances, and exceedingly rare as a primary form of disease. I can tell you very little about either, except what results from the observation of isolated instances, and from others recorded at intervals in medical journals. I have seen personally, I think, a few instances of primary disease of this kind; one, almost certainly, tricuspid obstruction, and perhaps two or three of pulmonic valvular disease. Strange to say, although in these cases the physical signs were extremely intense and characteristic, the symptoms were of remarkably moderate intensity, so much so that two of the cases have been under observation, more or less, for years, but with little alteration, if any, towards the worse; and a third was kept in view for some time, but has since been lost sight of. There is, of course, a risk of fallacy in all such incomplete observations, and I do not ask you to found any conclusions upon them. The general course of experience goes to show that obstructive disease of the right side of the heart is usually attended by very marked lividity, or cyanosis; so much so as to form a distinguishing character of this form of disease. In two of my cases, just mentioned, there was *occasional* lividity to a considerable extent; in the third, none. In other cases (mostly, however, complicated and fatal), I have seen the characteristic cyanosis well developed; but, on the other hand, in one remarkable case, of which I witnessed the dissection many years ago, there was scarcely any lividity, although aortic, mitral, pulmonic, and tricuspid openings were all very much obstructed by chronic disease. In this case I can tell you nothing about the

physical signs, as they were not observed, and the patient, indeed, was hardly known to have had heart disease at all.

5. Exocardial, anæmic, and aneurismal murmurs are very confusing, and are apt to lead to great errors of diagnosis, unless the physician is guided by a thorough and exact knowledge of their typical characters, and also of all the collateral circumstances likely to be associated with them in particular cases. It may be useful for you to note the following particulars, which are all that can be introduced into the present lecture. Exocardial murmurs more commonly resemble those of tricuspid and mitral, aneurismal and anæmic murmurs those of aortic disease. Exocardial murmurs are very commonly double, but not *exactly* rhythmic, nor blowing in character; anæmic murmurs are almost invariably single, VS in rhythm, and audible chiefly near the base and over the vessels; aneurismal murmurs are more commonly single than double, usually VS in rhythm, but sometimes also VD; and, as they are apt in this last case to be associated with the murmurs actually arising from valve-disease, the diagnosis is often of great difficulty and nicety, requiring much patience and skill, and a very comprehensive survey of all the available facts. The same remark applies to certain difficult and complex cases of disease to which no rule can be applied but those arising from accurate knowledge of principles and much clinical experience. I have attempted on the present occasion to convey to you only the first principles of cardiac diagnosis; but, while no department of our inquiry has been nearly exhausted, I think you will find also that no subject has been treated in so superficial a manner as to conceal from you the real difficulties of the questions that arise at the bedside. I trust also that you will find some degree of assistance from the form which our inquiry has naturally assumed in the course of oral delivery.—*Glasgow Medical Journal, Dec.* 1867, *p.* 283.

17.—ON A CASE OF EMBOLISM OF THE RIGHT VENTRICLE OF THE HEART.

By Dr. R. Richardson, F.G.S., &c., Rhayader.

By Embolism (ἔμβολος, a *wedge* or *stopper*) we understand that a vessel has been obstructed by a fibrinous clot, which might have formed in the heart or in the circulatory system, either arterial or venous, and by impulsion forced into a vessel so as to obliterate its cavity completely. The consequence attending such an accident depends upon the importance of that vessel to the function of life. When it occurs in the pulmonary artery, death is almost instantaneous; if the vessels of the brain, it causes apoplexy and softening of the brain; but if the arteries of the limbs are affected with it, gangrene of the part is the result.

These concretions are sometimes found in both sides of the heart, but more frequently in the right. Authors differ in opinion as to the mechanism of the formation of these clots. Some hold that the arrest of circulation and stagnation of blood in the heart is the cause; others, that inflammation of the internal membrane of the heart, valves, or vessels is the primitive cause of them. But I am inclined to believe, from the following case, that the cause in some cases must be in the blood itself, as there is no history of any previous illness whatever up to the day of death.

On Sept. 24th I was called to see Miss T—, aged eight years and a half, who was taken suddenly ill. On my arrival I found her in bed constantly spitting, and sometimes vomiting, some frothy mucus; her countenance looked pale, eyes sunken, and no pulse perceptible at the wrist; the action of the heart was tumultuous; very deep inspirations; feet and legs icy cold. I could at once see the serious aspect of the case. She did not complain of any pain, only of something in her throat. She turned over on her left side herself, and, with a deep inspiration, expired, as if with a sigh. She possessed her *faculties to* the last, and was dead in about ten minutes after my arrival, and *all that time* I was present

The history of the case is as follows. She had been to church on the Sunday, and on the Monday she was about as usual, but complained rather of headache. On the Tuesday (24th) she complained of a great quantity of phlegm coming in her throat, and when she moved it produced a vomiting; nothing but phlegm, which was very tenacious and frothy, came up. She was carried to bed about three o'clock p. m., and died about six the same evening, having been about the house in the morning.

The question naturally arose, what could have caused such rapid death? She was very tall for her age, and of a fair complexion, clear skin, and altogether she had a tubercular diathesis. Her mother died suddenly when my patient was about a fortnight old, after having suffered nearly a fortnight from phlegmasia dolens. I have no doubt an embolus was carried by the circulation into the pulmonary artery, and that accounts for so sudden a death.

As there was no reason for suspecting foul play, I could not very well account for so sudden a death in a child, as there was no symptom of any previous disease of the heart or of the brain. So, on the Friday, in conjunction with my friend, Mr. Rowland, F. R. C. S., of Strata Florida, who kindly assisted me, a post-mortem examination was made.

On inspection of the body there was nothing particular to observe; the body was well developed and very tall. On opening the abdomen we found all the viscera healthy; the stomach was healthy, and contained about an ounce of dark-coloured fluid; the spleen was congested, and the liver rather large. Chest: The pleura and pericardium were healthy; the lungs were very much congested, and the bronchiæ were full of frothy mucus; the left lung contained miliary tubercles. The heart looked normal. On opening the left ventricle, it was found to contain only about a drachm of dark blood; all its membrane and valves healthy; the auricle also healthy. On opening the right ventricle we found an embolus, about two inches and a half in length, an inch and a half in breadth, and about half an inch in thickness, which plugged completely the pulmonary artery. It was not adherent to the membrane nor the valves of the heart; its colour was pale-red, much like the appearance of uncooked veal. The right auricle was healthy. The head was not opened.

The case must be considered as quite unique, coming on without any apparent anterior cause. As we find the clot perfectly free from any adhesion to the heart or its valves, we must conclude that it was of recent date, probably not more than twelve hours, as when it commenced to develop it must have caused a certain amount of trouble in the system, which we do not find here before the fatal day. It must have been caused by the state of the blood itself—from the dyscrasia of the tubercular state as containing some morbific material. My namesake, Dr. Richardson, attributes the fluidity of the blood to the ammonia it contains; but it yet remains to be proved whether, in a case like the one above narrated, embolism occurs from lack of that substance.—*Lancet, Nov.* 23, 1867, *p.* 641.

18—ON INTERMITTENT PULSE AND PALPITATION.

By Dr. Benjamin W. Richardson, F. R. S.

[Intermittency of the pulse should be taken as a failure entirely confined to the action of the left ventricle. The ventricle does not contract when stimulated by the arterial blood contained in it, and then by a prolonged thud makes up for the beat missed. What is the cause of this frequent affection?]

We should naturally begin by looking into the heart itself for a cause. We should now be wrong. The fact alone that during the intervening periods of intermittency the action of the heart is natural, would go far to indicate that in it there can be no great amount of organic lesion. Still this of itself would be little were it unsupported by more direct evidence. Being greatly interested in this *matter. I seized* once the opportunity of examining after death

the heart of an aged man, who for many years presented the phenomenon of intermittency more determinately than I ever before had seen: his pulse never, as far as I could learn, failed to intermit less often than once in eight beats. His death was from senile decay, but his circulation may be said to have outlived all the other of his systemic powers. When quite insensible, the pulse, with long hesitation, came up again, and the pulse was beating at the end even when the respiration had ceased. And so after death, instead of a diseased heart, the heart was found the healthiest of the organs. There was no trace of valvular disease. There was no departure from the natural size and condition of the cavities or the thicknesses of the walls; the coronary arteries were normal, and the muscular structure, quite free from fatty and granular degeneracy was merely, as the tissues are in the aged, a shade paler than is common in the young and robust. Since the occurrence of that case I have confirmed the experience then gained by three other experiences; and I feel bound to say from what I have seen, as positive truth, that the most marked intermittency of the heart may be present without evidence of any known form of organic disease of the organ itself; and as in this case one fact carefully assured is as good as a thousand, I think we may accept that there is no known morbid condition of the heart itself, structurally considered, that produces the phenomenon of intermittent action. Intermittency may co-exist, of course, with other signs of cardiac derangement essentially of structural origin; a fatty heart may intermit; a heart with faulty valvular mechanism may intermit; and intermittency with structural change may form, and often does form, a most serious complication. This we must at once allow, but we must allow it feeling that the intermittent action has no necessary connexion with the structural disorder, but is evoked by a cause remote and independent. Pre-existent diseases of a special kind, such as acute rheumatism, do not, so far as I can learn, leave intermittency in their train; neither, as far as I know, is the phenomenon more common in those who have structural disease of the heart than in those who have not.

Intermittency is not peculiar to either sex. Intermittency is not governed by age exclusively. In one instance I noticed it in an infant on the day after its birth, and it continued in him in the most marked degree for five years. It then gradually passed away. A medical friend once brought me one of his children, a boy five years old, who had the symptoms in the most intense form, so that his parent was seriously alarmed—the boy himself, however, not being conscious of any ailment. In this case also the symptom has disappeared, the boy being now nearly twelve years old. Looking over my notes of cases, I find, in short, that there is no particular age at which I have not met some persons who have intermittency of the pulse. But this must be admitted at the same time, that the symptom is most frequently seen in persons of advanced life; in fact, in very aged people the absence of it is the exception rather than the rule. It is by no means unfrequent in persons of middle age, and it is as common in those who are prematurely as in those who are veritably old. It is least frequent between the ages of ten and thirty.

Intermittency of the pulse is not peculiar to the human species. A neighbour of mine had an old Italian greyhound which presented the phenomenon in the most singularly distinct form. I also had a dog that presented the symptom; this animal was not young, but hearty and disposed to fat and somnolency.

I shall probably assert what many will not agree with when I state that, according to my experience and inference, intermittency of the heart has no relation to what is commonly called dyspepsia. It is true that many dyspeptic persons have intermittent pulse, but this fact does not affect the question, because it is equally true that many persons who have determinate intermittency of pulse have the most keen and excellent digestion. I have a patient at this very time whose case is strictly in point: his pulse intermits every sixteenth beat, but his tongue is clean, his urine natural, his appetite good, his sleep sound, and his bowels regular. After taking food he has no pain, he has no flatulency, and, according to his own often repeated expression,

"he does not know that he has a stomach." On the other hand, we constantly see dyspepsia in all its varied and severe forms without the sign of intermittency.

I know of no diseased condition of the blood with which the phenomenon of intermittency is connected. Neither have I been able, after careful research, to trace it, in the light of effect from cause, to any affection of the lung, the liver, the kidney, or other secreting or excreting organ.

Thus we are driven at last to one sole system of the body in which to seek for the origin of the phenomenon of intermittency of the heart; and that is the nervous system. Followed to this seat, all the evidence is too unequivocal to be doubted. The suddenness of the phenomenon, its purely functional character in so far as the heart is implicated, and the other symptoms by which it is attended, leave no room to doubt the correctness of the view that the momentary cessation of the left ventricle occurs from deprivation of the force which it should receive to enable it, under the stimulus of the blood thrown into it by its auricle, to contract and regulate the blood-current in its arterial course.

All the evidences, again, point to the fact that, in every case of true intermittency, one particular point or centre of the nervous system is the primary seat of the derangement. The phenomenon is too uniform to admit any explanation less definite; it speaks to us and says there is a deficiency of force in the centre of the nervous system which provides for the ventricular contraction on the left side of the heart, or there is some centre which balances that supplying centre, and which is over-active.

The derangement might be in the ganglionic centres of the heart itself, but if it were, the nutrition of the organ would surely be more decidedly influenced, and the symptoms would be confined to the heart. The derangement might be from irritation in the periphery or in the line of the great nerve trunks, as the pneumogastric; but if it were, it would hardly be so persistent for years and evolve no other signs of muscular disturbance. Where then is the primary mischief? I believe it to be in the cerebrum. The clinical history of every case I have seen points to that seat. In the aged it is an invariable follower of failing cerebral power; in the very young it presents itself with other indications of cerebral derangement. But that which impresses me most in favour of the cerebral origin of intermittent pulse is the mode in which it appears in the prime of life. I have never met with a case in which it has not been traceable to some form of cerebral excitement with succeeding depression. Grief, imposed by the deaths of relatives and friends; shock from failures of enterprises in business; disappointments; violent outbursts of passion; remorse; degradation; and, most fruitful cause of all in this madly striving age, over-work of brain—these are the outside influences leading to the systemic change on which the phenomenon of intermittency of the pulse most frequently depends.—*Medical Times and Gazette, Jan.* 4, 1868, *p.* 3.

DISEASES OF THE ORGANS OF RESPIRATION.

19.—ON THE NATURE AND TREATMENT OF PULMONARY CONSUMPTION.

By Dr. C. J. B. Williams, F. R. S., Consulting Physician to the Hospital for Consumption at Brompton.

Pulmonary Consumption may be defined to be *a disease characterised by the consolidation of more or less of the lungs, with a tendency to degeneration, softening, and excavation of the parts affected, and this is attended with wasting of the whole body, and other evidences of its imperfect nutrition.*

In a large proportion of cases the consolidations, which precede the destructive process, occur in the form of *tubercles*, miliary or clustered; not necessarily preceded by inflammation in the lungs or air-passages. In others

the consolidation is more uniform or diffused, and is the result of some form of inflammation of the pulmonary textures. In either case the progress of the destructive disease is marked by the appearance of yellowish opaque spots, which spreading, convert the consolidations, granular and diffused, into crude yellow tubercle, whilst this further tends to softening and disintegration, and final excavation of the affected textures.

It is this tendency to degeneration and destruction which stamps the *consuming* character of the disease, and the more strongly this tendency is manifested the more irresistible and rapid will it be in its fatal course. In certain cases the disease is so acute and extensive as to carry off the patient in a few weeks or months. In others it is more limited and slow, and may not destroy life for five, ten, twenty, or more years. In the former cases medicine has little or no control over the disease; decay and death invade the frame so overwhelmingly that there is neither sufficient power in nature to resist them, nor time for art to aid that power. One of the most vital organs of the body becomes suddenly invaded by a disease changing its structure, obstructing its functions, and spreading through it the seeds of further decay, which not only in the organ itself, but by the blood, diffuses its destructive influence through the whole system.

Let us briefly sketch the two most terrible forms of the disease.

A man of middle age is attacked with fever, with pungent heat of the body, cough, viscid expectoration, extreme oppression, and overwhelming weakness, resembling that of continued fever; and the likeness sometimes appears also in the coated or dry brown tongue, sordes on the teeth, and occasional delirium. The vesicular breath sound is superseded everywhere by bronchial rhonchi and mixed crepitation. On percussion, the chest is dull nowhere, but less clear in the posterior than in the front part. This case might be supposed to be one of universal capillary bronchitis, with general pulmonary congestion. So it is; but this is not all. In spite of blisters and other remedies the breathing remains short and difficult; the pulse more rapid and feeble; the lips, cheeks, and nails become livid; clammy sweats break out, and the patient dies in the third or fourth week from his first attack. The lungs are found congested, and the bronchi loaded with viscid mucus; but more than this, innumerable miliary tubercles are scattered throughout the pulmonary tissue, and these are the obvious cause of the intractability of the case. They break out simultaneously, like the eruption of an exanthem, and by their numbers and bulk induce such an amount of obstruction and congestion in the lungs as to destroy life before there is time for any considerable degeneration or softening to take place. The *acute tuberculosis* is the worst and most surely rapidly fatal form of consumption.

The second form of acute consumption begins with pneumonia in one or both lungs. The patient, generally a young subject, is of consumptive family, and may have previously had cough and occasional hæmoptysis. The fever attendant on the inflammation may not be very high at first, and the expectoration by no means so viscid and rusty, nor the crepitation so fine and even, as in simple inflammation of the lungs. But the symptoms are more persistent. The pulse and respiration remain frequent. The heat of the body, particularly of the chest, continues remarkably high, almost burning the ear of the auscultator as he examines the back. But this intense heat is alternated with occasional chills and profuse sweats at night. The cough continues distressing, and the expectoration becomes opaque, purulent, and clotty; the flesh wastes, and the strength ebbs away; and if the appetite does not return, the progress of consumption and decay is rapid. Auscultation reveals the steps of the destructive process in the lung. The affected part, or the whole side, or part of both sides, becomes dull on percussion, or only varied with the cracked-pot note from the gurgling within; the loud tubular sounds are replaced by course crepitation, in parts amounting to gurgling; and the diffused bronchophony is modified into detached islands of voice, loud *and protoriloquous*, or into the snuffling or whispering sounds equally characteristic of a cavity. This form of *galloping consumption* may also prove *fatal in a few weeks*; and the lungs are found after death in a state of con-

solidation little more dense than the hepatisation of pneumonia, but their red is mottled with grey and yellow patches of tuberculous or aplastic matter, and excavated in various parts into numerous small cavities communicating with the bronchial tubes, and containing more or less of the same compound matter which was expectorated during life, consisting of mucus, pus, degenerating epithelium and exudation-matter, with disintegrated fragments of lung-tissue. This form of acute phthisis, although generally rapidly fatal, is not universally so. When not too extensive, it may sometimes be arrested and brought to a chronic state; and the chance of this result will very much depend on the recovery of the appetite, and the power of the stomach to bear strong nutriment, tonics, and above all, cod-liver oil.

And in a large majority of the cases of consumption the destructive element is still less extensive and less active, and its progress is much more slow; and we have both time and means to resist its inroads and to fortify the system against its operation to a greater or less extent. In the greater number of instances the disease begins with the symptoms of common cold, often referred to the throat as much as to the chest; and there is, in truth, more or less of bronchial irritation and inflammation attendant on the development of the disease, and recurring with renewed intensity at the time of its increased activity. Often the disorder is mistaken for a common cold, until either its remarkable persistence, or the occurrence of hæmoptysis, of night sweats, of loss of flesh, or of some other uncommon symptom, gives intimation of its more serious nature. Then it is found that, in addition to the signs of bronchial catarrh, there are some of the signs of consolidation of the lungs, generally near an apex; slight dulness or raised tone of the stroke-sound at or below a clavicle, or at or above a scapular; a tubular character in the breath-sound and voice; an undue intensity or duration of the sounds of expiration, or a weakness or absolute obstruction of the inspiration; and sometimes the various slight degrees of crepitus substituted for the proper breath-sound; and various other signs which it is unnecessary here to detail. These signs, however, are the indications of *incipient pulmonary consumption*—that is, of a disease which tends, sooner or later, to injure and destroy the structure of the lung, and to deteriorate and waste the flesh and blood of the whole body. And the progress of this work of injury and destruction is marked by signs of increased density and diminished motion of parts of the lungs; by more of the moist crepitus, from augmented humidity, in and around the consolidations; and eventually by signs of excavation at one or more points, which announce the removal of the diseased tissue.

The progress of this disease may vary infinitely in time and in extent. The more extensive the mischief, generally the more rapid will be its progress, which goes on in the worst cases, uninterrupted by any check or pause, attended by the distressing train of symptoms, harrassing cough, opaque clotty expectoration, increasing shortness of breath, burning fever alternated with profuse sweats and chills, rapid loss of flesh, and strength and colour, sometimes diarrhœa, and aphthous mouth, terminating in death in a few months.

But in other cases, and these are by far the most common, the destructive lesions are less extensive, and their progress is more slow and intermittent, and often seems in great degree to depend on occasional attacks from cold or other external causes, in the absence of which the disease may be quiescent or stationary, and may not destroy life for years.

Powerless as medicine is in the overwhelming and rapid types of the disease, it has yet considerable influence over these milder forms; and the succeeding paper will give some evidence that under careful treatment life may be prolonged for many years in comfort and usefulness, and in not very few cases the disease is so permanently arrested that it may be called cured.

[Although an admiring pupil of Laennec as far as regarded his sagacity and accuracy in detecting and describing disease, Dr. Williams has never considered his views of the nature of tubercle scientific and satisfactory. From 1828 until now Dr. Williams has held and taught that tubercle is a product of degraded nutrition, a material defective in organisation, and tend-

ing to still further deterioration and decay. The progress of pathological research, so far from superseding this view, has in most parts remarkably confirmed and substantiated it.]

There is a common variety of *cacoplastic* deposit, which takes place when textural nutrition is degraded, still further than in false membranes and healthy cicatrices: this is the transparent, miliary, grey, and tough yellow forms of tubercle. Instead of (with Laennec) classing tubercle under the vague term of "accidental productions," or (with Carswell) as a "secretion *sui generis*," I have for many years been induced to refer it to a degraded condition of the nutritive material from which old textures are renewed and new ones formed, and to hold that in its origin it differs from the normal plasma, or coagulable lymph, not in *kind*, but in *degree* of vitality and capacity of organisation. Every gradation may be found between euplastic and aplastic deposits, the cells and fibres, which are the representatives of organisation, diminishing in number and completeness, and the material becoming more granular, amorphous, or more abounding in fat-globules, in proportion as the deposit is degraded; until, in opaque, crude, or yellow tubercle, it is altogether aplastic, consisting of an aggregation of granules and fat-globules interspersed with the mere traces or remains of cells.

I consider that the more solid forms of tubercle are entitled to rank among cacoplastic deposits, because, although destitute of vascularity, they seem to possess a kind of structure, like that of the lower kinds of granular deposit. In the semi-transparent grey, tough yellow, and miliary varieties, vestiges of cells and fibres are commonly seen. The affinity of these with granular degeneration and fibrous deposits is shown by their commonly occurring in the same subjects, and by their frequently exhibiting the same tendency to contraction. In a very large majority of cases of chronic granular disease of the kidneys or liver, there are found more or less traces of tubercle in the lungs, the chief seat of its deposition; and in very few instances of chronic phthisis have I failed to find some degree of granular disease in the liver or kidneys. In acute phthisis and in acute granular disease local causes accelerate the degenerative change to a destructive extent in one organ before there is time for others to become affected.

We now proceed to trace the history of cacoplastic deposits by a few examples. The dense false membranes formed on the surface of serous membranes may be often seen to be surrounded with a radiated wrinkling or puckering of the adjoining parts, indicating that the abnormal substance has shrunk in size subsequently to its being first laid down. A similar contraction is noticed in the deposits that occur on and under the lining membrane of the heart and its valves, and here it causes a serious disturbance of the mechanism. The narrowing of the chest in some cases of pleurisy is in part dependent on the same property of cacoplastic deposits. This tendency of false membranes to contract was long since pointed out by Laennec and Hodgkin, and the fact was subsequently applied by Carswell to explain the diminished size of the liver in cirrhosis, which he considered to be due to a deposition in the intravascular and filamentous texture prolonged from the capsule of Glisson. I do not myself think that the contractile deposit in cirrhosis is confined exclusively to one texture, but believe that it is generally exuded from distended blood-vessels. False membranes which exhibit the same contractile character are sometimes found on the free surface of serous membranes, and especially along the course of the vessels, veins as well as arteries; and depressions caused by atrophy of the substance of the organ beneath these deposits are sometimes seen in the liver and lung. There can be little doubt that these deposits originate in a fibrinous material exuded from the vessels in certain pathological states (congestion, chronic inflammation, and malnutrition), and forming a dense structure of low vitality, which tends by its subsequent contraction to constrict and compress the adjacent parts, and so more or less to interfere with the passage of blood through them, and consequently to derange their nutritive and vital operations. The *same tendency* is evinced in the higher (less plastic) forms of tuberculous *disease*. Miliary or granular tubercles in the lungs, when in considerable *numbers* and not soon softening, cause a contraction in the substance of the

organ, chiefly at the upper part, and a corresponding collapse in this portion of the chest. I have met many cases in which a sinking of the infraclavian region has taken place before any signs of softening has occurred; indeed, this is a very common sign of tubercles which remain long stationary. The still greater amount of pectoral collapse in the advanced stages of tuberculous lesions, although partly dependent on other causes, is also still in some degree connected with the contraction of cacoplastic deposits in the lungs and pleura. In the peritoneum agglutinated tubercles often cause considerable contraction. I have seen the omentum puckered up by them into a knotty mass.

The microscopic examination of cacoplastic deposits, at different stages of their formation, throws considerable light upon the true nature of this process. When recent the effused matter consists of fibres, with a great quantity of granular, and more or less of amorphous substance, with or without an admixture of cells. At a later period, and after it has contracted, it is much more tough, and it is then found to be more distinctly fibrous, much of the granular and amorphous matter having disappeared. Old contractile adhesions and cicatrices are still denser, and when examined by the microscope are seen to consist of closely interwoven fibres, differing from those of fibro-cartilaginous tissue in being less regular and distinct. Grey or granular tubercles wither, and become more tough or horny ("cornify," Rokitansky), and thus remain quiescent. It appears, therefore, that it is by the partial absorption of the granular and amorphous portion of the deposit, and by the closer approximation of the fibrous or more organised constituent, that the condensation takes place. Hence it is that the structure thus contracted is less liable subsequently to the further degeneration to which cacoplastic products are prone. Certain it is, that of different portions of cacoplastic deposits in the lungs and elsewhere, those that are contracted remain unchanged, whilst the rest pass into the aplastic state of opaque and softened tubercle. The contractile process seems, therefore, to raise the deposit to a higher standard, so that, although still inferior in vitality to the adjoining textures, it gets to be tolerated by them, so to speak. But this very change may seriously injure the textures of organs by contracting and compressing their vessels, and interfering with their nutrition and other functions; and in this respect it may be compared with the highest, or most animalised variety of degeneration, the fibrous, which has already been described. It is in this way that the contractile diseases of the liver and kidneys gradually infringe on the circulation and secretion of those organs, and eventually prove fatal. The contraction which takes place near the apices and roots of the lungs in the more limited, and therefore more chronic, varieties of pulmonary tuberculous disease, often lays the foundation of emphysema of the organ and habitual asthma.

Having described the less degraded and degenerating form of cacoplastic deposits, it now remains to notice the variety which is of a lower character, and which tends to become aplastic. This includes all the commoner forms of tubercle. In the granular, miliary, grey, or drab-coloured tubercles of serous membranes and of the parenchyma of organs there is found a dense homogeneous solid, closely resembling some of the cacoplastic deposits which have just been described. The resemblance, even the identity, of these may often be very well traced out in chronic or subacute arachnitis, peritonitis, and pleuritis; portions of the affected membrane are covered with diffused masses of semi-opaque deposit, which no one would hesitate to call false membrane, whilst in other parts separate granules of precisely the same deposit exhibit all the characters of granular or miliary tubercle. In chronic and scrofulous inflammation the matter effused is, from the first, less ductile and more consistent, and the granular condition consequently more generally prevails, and remains even in spite of pressure and friction; and it is admitted that the product of chronic inflammation of the peritoneum and of the arachnoid membrane is always more or less granular. In acute inflammation in tuberculous subjects recent lymph is commonly studded with opaque granules, which subsequently assume the character of tubercles.

But tubercular deposit is not always preceded by inflammation. In many cases tubercles are found so extensively disseminated through different textures, after few or no symptoms of inflammation, that it is quite impossible to regard them otherwise than as the result of modified textural nutrition.

Miliary tubercles exhibit a considerable variety in size and consistence, according to their age. When recently formed they are plump, and although feeling harder than the surrounding textures, yet they can be crushed with firm pressure between the fingers. But in lapse of time they contract in size, and become tougher, so that no pressure can crush them. This change in the lung is accompanied by a darkening of colour, so that old tubercles are often quite black. This is an indication of the tubercle having become quiescent or obsolete, and having little tendency to degenerate to the yellow state.

Tubercles rarely occur in numbers, or spread much, without exhibiting another change of appearance: they lose their semi-transparency, and become of an opaque or dead pale-yellow hue, like the colour of raw potato or parsnip. This marks the transformation to crude yellow tubercle first described by Laennec, and this is the result of a further degradation or degeneration of the deposit. The few fibres and cells which are to be detected in grey tubercle are no longer distinguishable here; the interstitial hyaline or amorphous solid is greatly diminished, oil-globules appear in its stead, and the mass becomes proportionately less coherent and more granular, and is now, indeed, quite *aplastic*. Generally the change begins in the centre of the mass, apparently because, there being no permeating vessels, the centre is the part furthest removed from the vivifying influence of the blood. The cornea and other non-vascular textures occasionally become opaque in a similar way, in consequence of deficient supply of sustaining nutriment in the plasma, and may, indeed, entirely lose their organisation. A parallel form of degradation is that observed in the degeneration of fibrous and cellular tissues into granular matter, recently pointed out as resulting from mal-nutrition. But not only will deficient supply of healthy plasma promote the transformation of tubercle from grey to opaque; an undue flow or accumulation of blood, produced by inflammation or congestion in the neighbourhood, will also accelerate the change, just as the same occurrence hastens the disintegration of lymph and of compressed textures in suppuration, the conversion of the plastic into aplastic, through the increased warmth and afflux of fluids exalting chemical affinities in a material which has none of that vital power of resistance by which living tissues maintain their integrity.—*Lancet, March* 21 *and* 28, 1868, *pp.* 369, 403.

20.—WHAT IS TUBERCLE?

At a meeting of the Clinical Society last Friday, an interesting discussion took place on a case of Fibroid Phthisis. Of late years, this name has been given to a disease which has long been recognised; but which, although differing in its duration and etiology from ordinary phthisis, has been included under the class of tuberculous affections; and the question at issue seemed to turn on whether this disease was of the nature of tubercle, or whether it should be described as a distinct variety of phthisis.

The word "tubercle" has been applied to so many morbid changes which differ from each other in their clinical history, as well as in their histological characters, that, from originally denoting a small rounded body, it has been made to include any process which results in the breaking down of lung-tissue and the formation of cavities. When a word thus becomes altered in meaning—when some use it only in a restricted sense, and others as almost synonymous with phthisis—it must follow that confusion arises in discussion; and this, while pathologists are describing the same morbid process, *they will look* upon it as tuberculous or not, according to their preconceived notions on the subject.

ıe term "tubercle" is beginning now to be restricted to the grey ulation: a definite body, formed of oval or round nuclei, and generally ›unding a vessel or bronchus; in cases of tubercular meningitis, it may ily be studied in the pia mater, and it is also to be seen scattered ıgh both lungs or in other parts of the body; in yet other cases where ›ourse of the disease is not so rapid, the granulations may be found to ggregated together, and in the most advanced portions to have become ıed in the centre and formed cavities; in other parts of the lungs, grey rcle may be seen scattered about and in various stages of transformation. ımpanying this pathological state, there is a distinct clinical history, h much resembles typhoid fever, and like that disease, has great daily ‹tions of temperature.

the yellow or cheesy masses so often found in the lungs, Dr. Clark not assign the name "tubercle." Under the microscope, they are seen ›nsist of round cells, with nuclei and granular contents, and, as they ıd, nutrition being cut off from the central parts, they soften and break ı. The origin of this "cheesy" matter may be a pneumonic product h has not been absorbed or cleared up, or a syphilitic or cancerous sit, or as a result of embolism, or concretions in the bronchial tubes. appearance, according to Dr. Clark, although to be found accompany- he grey granulations, may be met with independently of them.

another class of cases, a more chronic change occurs, and, instead of a monic product, a fibroid material results; the pleura is found much ɛened, and proceeding inwards are numerous septa, which divide the into segments, and this change takes place in the perivascular or ›ronchial tissue; it consists either in a proliferation of the connective e cells, or in an exudation of an amorphous material into the air-vesicles selves.

ıis process is one which may last many years, and although cavities form, they extend but slowly, as there is such a dense wall of fibroid ɛation around. Generally affecting one lung, and the left more ıently than the right, it is often accompanied by an amyloid state of or kidney; it is also to be met with in rheumatic or syphilitic individ- or in drunkards.

inically, it is remarkable for its long duration, as the disease may last ? years; the absence of night-sweats and of any rise in temperature. It be accompanied by hæmoptysis, diarrhœa, and loss of flesh; and ical signs of cavities or consolidation of lung-tissue are to be met with. ough it may occur without any grey granulations being present, yet is not always the case; and, if this fibroid induration surround tubercles, ay be looked upon as due to a slow process of irritation, which, ıning in congestion, ends in a proliferation of connective tissue nuclei; ›, in other cases, where the congestion is more intense or the change acute, a local pneumonia results, and the cheesy material is met

r thus restricting the term "tubercle" to a definite histological product mpanying a well defined condition of body, and by giving a fresh name ›cesses which are different in their origin and course, Dr. Clark has ɯn much light on a subject which has hitherto been very obscure. .e the term "phthisis" may be kept as indicating a state the chief ›tom of which is a wasting away, it will be well if we recognise ›us forms of it, as different from each other as the contracted gouty ey from a large white one; then phthisis will be as genuine a name as ht's disease, and the word "tubercle" will have a distinct annotation, ıot, as heretofore, include a number of perfectly distinct pathological ɜses.—*British Medical Journal, March* 7, 1868, *p.* 222.

21—ON SOME ANOMALOUS CASES OF ACUTE PHTHISIS.

By Dr. Sydney Ringer, Physician to University College Hospital.

[Certain anomalous cases, not following the same rules as the ordinary cases of phthisis, struck Dr. Ringer whilst investigating the temperature in that disease.]

In all the cases of which we shall first speak, the patients suffered from acute phthisis. The deposit was yellow tubercle, or, as it is now called, scrofulous pneumonia of the lung. This was either in the infiltrated or discrete form. Thus far these cases are in no way peculiar: their peculiarity consists in this—the deposit occurred first at the base of one or both lungs, and was always most abundant there, and was often almost entirely absent on the upper lobes, even so when the lower lobe was absolutely solid, or riddled with small abscesses, or converted into one large hollow.

In their course, they simulate, as regards their physical signs, various other diseases. The difficulty in the diagnosis is still further increased by the generally accepted doctrine that tubercle, whether grey or yellow (*i. e.* scrofulous pneumonia), always invades first and most the upper part of the lungs. By the aid of the temperature, with the course the physical signs and symptoms run, we have been able to obtain a correct conclusion of the nature of the disease—a conclusion afterwards confirmed by the post-mortem examination.

We shall at first confine our attention to the physical signs met with, at different periods, in these cases. Some of the patients were first seen by us when the disease had well progressed; others were under our care from the very commencement, and were carefully watched throughout.

At one time of the disease the only physical sign present may be crepitation at either base, which often has all the characters of pneumonic crepitation in perfection; at other times it is a little larger; but, as every practical man knows, in pneumonia itself a very great difference is met with in the size of the crepitation.

At this stage it might be easily thought that the disease was genuine pneumonia. This error is the more probable, as the disease may begin acutely with high fever, stitch in the side, slight cough, and scanty tenacious expectoration, and this crepitation may be from the first present, and detected by auscultation. But even at this stage our suspicions will be aroused by various circumstances. Thus, generally there is but little weakness; that extreme prostration so common and characteristic in pneumonia is absent. The patient walks with tolerable ease into the room or moves without assistance in bed. Another circumstance which, in conjunction with others to be mentioned, will assist us in the solution of a difficulty similar to that under our consideration, is that the breathing is but little oppressed, and would not strike an observer as being in any way affected. It is, however, generally a little hurried. The expectoration is not rusty unless there has been decided hæmoptysis; but when such occurs, and for a few days after it has ceased, the expectoration may be rusty. The very fact of the hæmoptysis which preceded the rusty sputa would excite our apprehension, and would lead us to fear that the patient was consumptive. Yet with all these characters, apart from hæmoptysis, a positive conclusion cannot be formed. In a few days, however, the course the physical signs observe clears up all our difficulties, and confirms the suspicions of the case, and justifies the diagnosis of consumption. For while in simple pneumonia the few physical signs mentioned above are soon accompanied by some dulness on percussion, with alteration of the breath-sound, and in a few more hours by absolute dulness with great increase of the vocal fremitus and tubular breathing, such is not the case with scrofulous pneumonia of one base. But instead of this, day after day passes, and yet the physical signs remain just as few and indeterminate; the crepitation continues, or even this may be heard abundantly one day but be unheard on the next, and again on the following one be as abundant as ever; sometimes a little dulness may be added to the crepitation, but

this is usually slight. This almost stationary condition of the physical signs is always suspicious of scrofulous pneumonia, and the longer it continues the more characteristic does it become of the disease.

In some cases the paucity of the physical signs may continue for weeks with only a small amount of dulness added, and without any appreciable alteration in the characters of the breath-sound. This long continuance of such physical signs is of itself quite sufficient to prove the disease before us is not simple pneumonia, and, added to the condition of the temperature, is amply sufficient in most, if not in all, cases to conduct us to a correct diagnosis.

Of œdema of the lung, which may be accompanied by fine crepitation at the base, it is hardly necessary to speak, as the general condition of the patient will generally decide whether such a state of lung is present or not.

Then, again, pulmonary apoplexy, which is often seated at the base of the lungs, may be accompanied by crepitation like pneumonic crepitation. But then, again, the presence or absence of a disease sufficient to cause pulmonary apoplexy, with a general consideration of the symptoms and physical signs, will settle any doubts in our minds in such a case; and, again, in these two last-named diseases the temperature is natural, as far as a limited experience in this matter will allow me to speak.

In what way does the temperature comport itself in scrofulous pneumonia? Some of its characters are of greater importance than others. We shall speak first of those of lesser importance. The amount of elevation in scrofulous pneumonia is usually less than in simple pneumonia, generally not reaching higher than 102° and 103° Fahr., while the temperature of simple pneumonia is often 104° and 105° Fahr.; yet this distinction is by no means a constant and hence important one. Another difference in the temperature of the two diseases is this: while in scrofulous pneumonia it often falls much in the morning, so that the diurnal variations are great, in simple pneumonia the elevation is maintained at much the same height throughout the day; but, again, too much stress must not be attached to this character of the temperature. The great point to look to is the time it remains elevated. In simple pneumonia the inflammation ceases at some time between the fifth and tenth day, and with this cessation the temperature falls and becomes natural. Should the temperature remain high for a longer period than this, then we are justified in the diagnosis of scrofulous pneumonia, if the physical signs given above are present. Thus, this combination of circumstances—namely, crepitation, fine or coarse, heard at the base, with an elevation lasting twelve or more days—is sufficient to lead to a correct diagnosis of scrofulous pneumonia, and every additional day of continuance of these circumstances confirms the conclusion.

We have said if crepitation be present; but it is not necessary to have the crepitation exactly like that often met with in pneumonia. Indeed, in most cases such a rhonchus is not present, but very generally one with many of its characters. Generally, in scrofulous pneumonia, it is a little larger, and often accompanies expiration as well as inspiration.

From this point the disease gradually increases, and with this increase the physical signs and symptoms alter, until another phase of the disease is presented—such a one as may come first under the notice of the medical man—and then the diagnosis will have to be made on the strength of the signs of this new phase. This new condition of things thus comes about. After some days, or it may be weeks, dulness at the base occurs. This increases very slowly in extent and in intensity. At the same time, and *pari passu* with this increase of the dulness, the vocal fremitus becomes more marked, and the breathing blowing, metallic, and soon perfectly tubular.

Here, again, are circumstances sufficient at first sight to lead to the impression that the disease might be simple pneumonia of the second stage, as we have dulness at the base of one or both lungs, with increase of the vocal fremitus, with tubular breathing and fine true crepitation. Yet there are peculiarities in these characters which suffice often for a determination of the *nature of the disease; for* in most of these cases the crepitation is pe-

culiar in this respect. As the physical signs deepen in intensity, this fine rhonchus continues, and is heard over the whole of the consolidated lung: on some days being small in quantity, or even absent for a short time, and replaced by a larger rhonchus, but soon it again returns, and even when the physical signs have reached the intensity described above, this crepitation is heard over even the most dull parts. If we had watched a case thus slowly progressing, the task in diagnosis before us would be comparatively easy; but the case is otherwise when we are summoned to see a person for the first time with signs such as those described. The fact of the crepitation being heard over the whole of the consolidated part of the lung and not at its confines, its seat in simple pneumonia, would be a circumstance that should attract at once the attention of the observer and excite his suspicion of some other disease than simple pneumonia. Then, in such a case as we are now supposing, and I have in each instance quoted from notes taken at the bedside, the previous history of the patient would assist us very greatly, as, if the patient had been ill for some weeks or months, and we could connect his then physical signs with his previous illness, we should be convinced that simple pneumonia was not the disease he suffered from. But it is not always easy to decide such a question; and it may be he has suffered with some complaint which has been complicated with simple pneumonia. If such a doubt arise, a few days' observation will set the difficulty at rest, for the temperature will come to our assistance. On examination, if the case be one of scrofulous pneumonia, we find this to be 102 or 103 every evening, and to have all the characters witnessed in the first stage of the disease. Such an elevation, or one higher, would occur in simple pneumonia, but if the consolidation were due to the last-named disease, then we should expect the inflammation, and with it the temperature, to soon decline, and the last to become natural, as, if a stage of consolidation were reached, the disease must have lasted a little while and thus be near its termination. Now, if we find the temperature continue elevated day after day, we must conclude, with such physical signs, our patient is the subject of scrofulous pneumonia.

When this phase of the disease is reached, the physical signs characteristic of it continue week after week, even month to month, with scarcely any alteration, or with merely a very slow increase in their intensity and extent. The continuance for some time at the base of great dulness, increased vocal resonance, tubular breathing, and wide-spread crepitation, with an elevation of the temperature to 102° and 103°, justifies a diagnosis of scrofulous pneumonia. The symptoms met with at this stage of the disease are not those of simple pneumonia; the weakness is less marked, and the breathing but little hurried, and the pulse respiration ratio much less disturbed. Often the patients can get about, and even do much of their work, while their breathing appears to be quite calm.

If life be continued long enough, the disease advances another step, and cavities are formed. Then to the tubular breathing, absolute dulness, diffused crepitation, there are added at places cavernous breathing and cavernous rhonchus, sometimes even amphoric note on percussion. This state of things may be present at the base while the upper part of the chest is free from unhealthy physical signs, and when, as the post-mortem shows, the upper part of the lung is nearly free from the deposition from the disease.

Here is a collection of physical signs which might suggest diagnosis of ulceration of the bronchial tubes at the base, leading to the production of cavities and to consolidation between the bronchi. Such a disease is occasionally met with. The latter may run a rapid or slow course. It is very generally a very chronic disease, and then does not produce any elevation of the temperature, or a very slight and very occasional one to 100° F. ahr. This distinction between the two diseases was lately well shown in our wards, where we had a patient suffering from scrofulous pneumonia, and opposite to him another with this ulceration of the bronchial tubes. The diagnosis in *both cases* was confirmed by the post-mortem examination. In the case of *scrofulous pneumonia*, the temperature was very considerably elevated every *day; while* the patient who suffered from ulceration of the bronchial tubes

and the formation of cavities the temperature was almost always natural, and only occasionally rose to 100° Fahr. There are also usually marked differences in the physical signs of the two diseases, while also the expectoration in ulceration of the bronchial tubes of that kind of which we are speaking is generally different from that of scrofulous pneumonia.

Most of the cases on which our remarks are based were examined after death, and the state of the lungs carefully ascertained. It was then found that the lower part of the lung, often the whole lower lobe, with perhaps part of the upper, was completely consolidated, and presented the appearance of what was formerly called infiltrated yellow tubercle, but which has now received the name of scrofulous pneumonia. The whole or the lower part of the lower lobe was absolutely solid, or here and there were seen a few small spots of less affected or healthy lung. The consolidated tissue was quite airless; the section dry, of a straw or grey colour, mottled with spots and streaks of black pigment. It broke down with some ease, and the fracture was granular or cheesy. In some parts it was softer, more brittle, than at others, and had more the character of ordinary pneumonic infiltration. These parts, the most recently affected, were more raised above the cut surface than the older deposits, and also were more granular, less opaque, and, as we have said, more brittle and more easily broken down. The older parts gave what has been termed the horse-chestnut look and consistence.

The disease appeared to originate in two ways in a manner similar to that when the disease first affects the upper part of the lung. Sometimes small nodules, the size of pins' heads or peas, were formed, and by their increase in number the lung was gradually destroyed until, by their coalescence, large tracts of lung were consolidated. In other cases there were seen innumerable little straw-coloured spots, closely placed together, and looking as if isolated vesicles were filled with the diseased products. Each of these was separately affected, and those in their immediate neighbourhood were healthy to look at. These latter in their turn appeared to become affected, and thus extensive consolidation is attained. Where the consolidation is great and the deposit of some duration, it is tougher, smoother, denser, less granular, and more opaque than at those places where the deposit is of recent formation. These consolidated portions were sometimes riddled with small cavities formed apparently by the deliquescence of the diseased lung, for the cavities are then everywhere immediately bounded by consolidated lung, and not by a fibrous capsule. These cavities at the base may reach to the size of an orange. The consolidation may be thus intense, and the excavation thus advanced, while the upper lobe may be almost free from the disease, and contain only a few scattered nodules of yellow tubercle. Sometimes the disease begins at one base, and then gradually invades the whole of that lung, and then attacks the other lung, setting first upon its apex, and involving this most extensively in the disease. At other times both bases are the first to be affected, and the disease is always most marked at these parts.

In these cases, it appears, we have yellow tubercle (scrofulous pneumonia) affecting primarily the base. Yet in these cases the physical signs and the morbid anatomy are somewhat different than when the disease is situated at the apex. Thus, the crepitation, its persistence and wide-spread character, so marked when the disease is at the base, is rarely met with in attacks which commence at the apex. The tubular breathing, again, which is so intense in the form of the disease of which we are writing, is absent where the disease fixes on the apex. This latter difference is well explained by the anatomy of the two forms. The consolidation is greater—that is, it is more general and more intense—where the base is primarily affected, and passes less slowly into excavation, than is the case with tubercle at the apex. In respect of the symptoms, they are the same whether the disease first seizes upon the apex or the base. Copious spitting of blood may be one of the earliest symptoms, perhaps the first to attract the patient's attention and to excite his fears. This may be often repeated. Where it occurs, the expectoration, for some days after it has ceased, may be rusty in colour, and this appearance *of the phlegm* may easily lend countenance to the idea of

simple pneumonia being present. The cough is, very generally, very tearing and hacking, frequent, and sometimes paroxysmal. It is one of the most distressing of the symptoms, causing from its violence and hardness much pain to the patient. The expectoration is at first composed of a mucilaginous-looking fluid, a little opaque, rather tenacious, very little aerated, and looks not unlike rice-water, but is thicker. It is at first free from purulent-looking matter. As the disease increases, the mucus becomes mixed with yellow purulent-looking expectoration, which may be pretty equally mixed with the mucus, or take on the shape of pellets. After a time, mucus ceases to be present, and all that is brought up is purulent. These are the characters of the expectoration in the various stages of ordinary phthisis.

The course of the disease is somewhat variable; as with the disease when situated at the apex, its course is for the most part a downward one, and the disease almost always ends in death. The temperature marks well these variations in the severity of the disease. In some cases throughout it is elevated, every day rising to 103° or 104° Fahr., while in the morning it often falls very considerably, even to the degree of health. In other cases, after an elevation of some weeks, a gradual fall occurs, and continues till the temperature of health is obtained, and continues at this for a very long time, with an occasional rise to 100° or 101° Fahr. After a course of this kind, it may again rise considerably, and remain elevated till the patient dies. In more favourable and in exceptional cases the temperature becomes natural, and remains so while the patient recovers strength. His symptoms disappear one by one till good health is restored. With each temporary lull in the course of the temperature there is generally witnessed a decided improvement in the condition of the patient. This is sometimes so marked as to raise hopes of a permanent recovery—hopes which our experience shows us to be in most cases deceptive.

Such is the course the temperature adopts in all forms of acute phthisis, and, in conclusion, I may state that a very extended experience enables me to affirm a statement put forward by me in a pamphlet some time ago—namely, that a disease of the lung causing a considerable elevation of temperature lasting many days—say ten to twenty, or more—is almost always due to the deposition of either grey or yellow tubercle. But one exception to this general statement has as yet occurred to me, and there the elevation was due to acute inflammatory ulceration of bronchial tubes at the base of both lungs, leading to rapid excavation of the organ.—*Medical Times and Gazette, Feb.* 29, 1868, *p.* 226.

22.—PHTHISIS PULMONALIS AND ITS TREATMENT BY "PANCREATIC EMULSION OF FAT."

By R. J. Kinkead, Esq., A.B., &c.

In order to treat phthisis pulmonalis with any hope of being successful, it is absolutely necessary to consider carefully as much as we know of the pathology of the disease, the exciting and predisposing causes, and to inquire into the nature of what we call the tuberculous deposit.

It is only by so doing that we can act from knowledge like men endowed with reason, and not by guess-work and routine practice, in many instances sacrificing our patients' lives, and bringing disgrace on our profession.

It is this knowledge which enables us, from amongst the multitude of remedies offered for our use, to choose those likely to prove beneficial, and to reject the useless and injurious. This knowledge enables us also to avoid the too common mistake of thinking that a medicine which in some cases does good, must be a specific, and of itself and by itself, able to cure phthisis, and so neglect that combination of remedies which, in this disease, is most essential to guide it to a successful issue.

Our knowledge of the constitution of tubercle is anything but satisfactory. *Two opinions* are chiefly supported by pathologists, one class affirm-

ing that tubercle is an exudation essentially morbid, the other, that it is merely a retrograde metamorphosis of pre-existing structures, either natural or morbid.

One of its essential characters is that, however deposited, it is incapable of development, but on the contrary, has a constant tendency to degenerate. Tubercles are either grey or yellow; the grey are tough, soft, of a pearl grey colour, have a certain degree of organisation, being composed of round, oval, or triangular-shaped bodies imbedded in a basis substance, and are soluble in liquor potassæ.

The yellow appear to be a degenerated, and more especially a fatty degenerated, form of the grey.

Tubercle is generally produced without inflammation, as implying pain, redness, swelling, and the production of lymph or pus. The question now arises as to the origin of tubercle. What are its exciting and predisposing causes? The exciting causes are, generally speaking, cold, and breathing an atmosphere loaded with irritating particles.

Our knowledge of the predisposing causes is not at all so clear. What we do know seems to be something like the following:—That it is an essentially constitutional disease. Whether local irritation, such as living in an atmosphere loaded with minute atoms of organic matter, can occasion the local development of tubercle without the constitutional tendency being present; whether "the knife-grinder's rot," the so-called cases of "consumption" in flax-scutchers, &c., are really cases of tubercular phthisis, or merely of hectic fever, induced by numerous spots of circumscribed inflammation, running on into abscess, or causing excessive purulent expectoration from inflamed bronchi, occasioned by the lodgment of minute particles of extraneous matters in the lungs, is yet to be solved. But be this as it may, the constitutional nature of tubercle, whether developed in the head, lungs, or mesentery, is one of the best-established facts in medicine. At the head of these constitutional predisposing causes stands hereditary taint, and then comes whatever tends to bring the system into a state of deficient nutrition.

In this state of the system, in which tubercle is likely to be developed, we cannot say that the blood is in fault so much as the pabulum, which ministers to the nutrition of the body, through the blood, which impoverishes the blood, and, as a consequence, brings the tissues of the body to an impoverished condition likewise. In this state the blood becomes albuminous, and white corpuscles become increased.

The gastric and intestinal acids become greatly augmented, more than neutralising the alkaline secretions of the saliva and pancreatic juice, and so render them incapable of preparing fatty matters for absorption, and under certain circumstances render the albuminous constituents of the food more soluble. Hence more albuminous than oleaginous elements enter the blood, and cause the albuminous character of the blood, which, infiltrating the tissues, may in some instances be changed by coagulation into grey tubercle.

It is a certain fact that in most cases a low state of the system, accompanied by a poor state of the blood, is, if not a certain, at least a most probable predisposing cause, and in all cases tubercular phthisis is accompanied by this cachectic state of the body.

In considering the progress of tubercle, the tendency to degenerate which characterises it is an example of what has not inaptly been called the "vis naturæ medicatrix."

Grey tubercle degenerates into yellow, and the yellow to a puriform mass, which sometimes excites just sufficient inflammation to cause its own expulsion, leaving a cavity behind which (if there are no more tubercles or cavities in the neighbourhood) contracts. The walls pucker in, and finally become obliterated, or the watery particles may be absorbed, the earthy constituents concrete, and the mass be converted into a hard chalky substance, principally consisting of phosphate and carbonate of lime, which is sometimes coughed up, or covered by a false membrane, may remain for years harmless in the lungs.

Cases of phthisis pulmonalis may, in my opinion, be divided into three classes:—

I. Cases in which tubercles are developed rapidly in great numbers, and of small size (miliary), occupying, perhaps, the greater portion of the lungs (tubercular infiltration of Baillie), accompanied by inflammation, and running a rapid course.

II. Cases in which tubercles are confined to one portion of the lungs, but are constant there, appearing for an uncertain time neither to get better or worse, but occasioning a certain amount of ill health—throat or stomach symptoms being most frequently complained of.

III. Cases in which the tubercles are deposited in crops in limited portions of the lungs, giving rise to febrile exacerbations, and intervals of health, according as the tubercle is deposited and got rid of by expectoration or concretion.

In the first class we can do little or nothing. In the second, and more especially in the third, class we can do much.

On being "called in" to a case of phthisis our efforts must be directed to subdue any inflammation present, to allay any irritation in the lungs which would encourage the growth of tubercle; secondly, we must endeavour to procure the elimination or absorption of the tubercular matter; and, thirdly, we must strive to prevent the redeposition of tubercles.

Our treatment of the inflammatory attacks must be guided by the symptoms and nature of each case. The remedies that would produce the best effects in one might be most injurious in another.

The treatment likely to produce the elimination of tubercles is almost identical with that necessary to prevent their redeposition, and constitutes the, so to speak, "specific" treatment of phthisis, and may, in a word, be said to consist of putting into and keeping the body in as perfect a state of health as possible.

I purposely omit to speak of the efficacy of climate, although I believe that in most cases a judiciously chosen climate is absolutely essential to a perfect and permanent recovery. The other indication will be best obtained by procuring the assimilation of fats which supply respiratory food to the lungs and help to enrich the blood.

At the head of oleaginous preparations stands cod-liver oil, of which it is not stating too much to say "that its use has, to some extent, removed tubercular consumption from the list of incurable diseases."

The chief obstacles to its administration are the disagreeable taste, and its occasioning sickness. The class-books teem with recipes to obviate the former; the latter, I believe, is occasioned, not by any intrinsic quality of the oil but by that excessive acidity of the gastric and intestinal secretions so characteristic of scrofulous cachexia, as to have gained the name of "strumous dyspepsia." This acidity may, in many instances, be overcome by combining alkalies with the oil. But even this method will fail in some cases, and the stomach still continue to reject the oil.

In these cases, and in those in which the repugnance to the taste cannot be overcome, I have found "pancreatic emulsion of fat" a most valuable agent. The office of the pancreatic juice in the body (it has alkaline properties) is to emulsify the fats and render them fit for assimilation by the lacteals. The acid state of the gastro-intestinal tract in phthisical patients neutralises the action of the pancreatic secretion, and in some cases causes an excess of acidity. Now, pancreatic emulsion offers to the stomach the most easily digested fat (mutton suet) prepared for absorption by the very juice which disease has neutralised. So that, in one case, we have pancreatic juice neutralised by acid, and pancreatic juice and fat free to act, and, therefore, the fat easily absorbed; and where the acid is in excess by the addition of an alkali, we have pancreatic juice and alkali neutralising acid, and pancreatic juice and fat free to act. But what is perhaps its most important action, is the educating the stomach to receive and digest oils, for, after the emulsion has been administered for some time, it will be found that cod-liver oil will agree in those cases in which before it did not.—*Medical Press and Circular, Feb. 26, 1868, p.* 178.

23.—THE GRAPHIC METHOD APPLIED TO THE MOVEMENTS OF RESPIRATION.

By DAVID C. M'VAIL, Esq., House-Surgeon to the Alnwick Infirmary, Northumberland.

Somewhat more than six months ago, the idea occurred to me that the graphic method might, with very great advantage, be applied to the movements of respiration; and after considering the matter very attentively, I had a model constructed, which, on being tested, satisfied me that the idea would work beautifully, if properly carried out. Accordingly, I engaged a first-class workman to construct from the model and some drawings a small machine which I had every reason to believe would answer the purpose. And I have not been disappointed, for the instrument, so far as I have yet wrought with it, answers my most sanguine expectation. The "spirograph" (for so I purpose calling it) in no way resembles the sphygmograph, except in so far as both it and the sphygmograph resemble all the other graphic instruments—namely, in that they write on paper moved by clockwork. Nor is it from any love of novelty that it differs from its elder brother, but from the fact that no instrument constructed on the plan of the sphygmograph would be of the slightest use as a spirograph. And the reason of this is very obvious. As most of the readers of the Lancet are aware, Marey's improved instrument consists essentially of a lever of the third kind, there being a fulcrum at one end, and the pen which writes on the moving paper at the other, the power being applied between and close to the fulcrum, in order that the comparatively small motion of the pulse may be considerably multiplied. Now, of course, the pen moving at the end of the lever would, if a uniform motion were applied—such as would be given by a small sewing machine,—describe, not perfectly straight lines (as, to be theoretically correct, it ought to do), but a series of arcs of a circle, thereby necessarily confounding the curved tracings caused by the instrument itself, and those which may be imparted to it by the part under examination.

Now, in the case of the sphygmograph, where the motion of the pulse is so very small, and the circle described so very large, the imperfection is practically of but little consequence, although I can see no reason for working with an incorrect instrument (especially one so delicate) when a perfect one can be so easily constructed. But in the case of respiration, where usually the motion is pretty considerable, and in forced breathing is very great indeed, the lever is quite inadmissible; for any lever of a workable size would give tracings so very much curved as to be quite useless. For the markings to be of any use the lever would require to be several feet in length, and the pencil end would move through such a space, and, consequently, would require paper of such breadth and length as to need a moving power little short of a small steam-engine to work the paper drums. And, besides, it would take so much power to work the lever that its delicacy, and consequently its truthfulness, could not be relied on.

The instrument appears to me to possess the following recommendations:—

1st. It is exceedingly simple and portable, for although the one with which I am at present working is in size about as large as a small French clock, there is no necessity for its being much if at all larger than a fair-sized snuff-box.

2nd. It can be applied to any individual part of the chest or abdomen, and to any extent of the chest or abdomen at once that may be desired.

3d. It is, if properly made (the teeth especially must be properly cut), *absolutely correct*; for if the piston be worked by a uniform motive power, the tracings must be quite rectilinear.

4th. By having variously shaped end-pieces for the piston it will, I am certain, be easily applied either to the parts concerned in respiration, to the heart, or to the blood-vessels; in fact, it will be a spiro-cardio-sphygmograph, or "— brevity's sake, a "motograph." My present instrument is rather too li — the arteries, but there is not the slightest difficulty in the way of ting one that will do for all.

As this paper is quite long enough already I must defer at present giving any account of my practical work with the spirograph, but will forward a brief account at an early date.—*Lancet, March* 7, 1868, *p.* 312.

24.—TREATMENT OF HOOPING-COUGH BY THE INHALATION OF CARBOLIC ACID.

By V. W. Blake, Esq., Birmingham.

Many of my patients suffering from hooping-cough having derived great benefit from the inhalation of carbolic acid, I have designed a simple apparatus, manufactured by Mr. T. P. Salt, of Bull-street, Birmingham, for the evaporising and diffusion of the carbolic acid in the atmosphere of the rooms occupied by the patients.

The effect of its inhalation on hooping-cough is marked by its quickly reducing the frequency of the cough and cutting short the spasmodic paroxysm. I have some cases of phthisis pulmonalis under the same treatment, which, I have every reason to believe, are much benefited by its inhalation.

As a fumigator and a disinfecting agent in fevers, &c., I think the evaporiser will prove very useful, and I now invite my professional brethren to make trial of it. In hooping-cough its success is very great.—*Medical Times and Gazette, April* 11, 1868, *p.* 405.

DISEASES OF THE ORGANS OF DIGESTION.

25.—ON THE SUCCESSFUL USE OF ARSENIC IN CERTAIN CASES OF GASTRIC PAIN.

By Dr. Arthur Leared, Senior Physican to the Great Northern Hospital.

[Dr. Leared considers that neuralgia may attack the stomach and intestines in common with every organ in the body.]

This primary pain occurs by no means so frequently as that which attends dyspepsia. There is, perhaps, no kind of suffering more severe when in its worst form. The anguish of extreme pain is supplemented by the anguish of utter prostration, and what the patient calls a sinking sensation often adds to his distress. This sinking and other indefinable sensations in the stomach are probably due to its connexion with the solar plexus of the sympathetic nervous system.

The painful affection happily intermits; but the intervals between the attacks, which at first may have been measured by months, come at length to be spanned by weeks, or even by days, and, in some instances, the patient is attacked even on successive days. The seizure happens with but little warning, and generally when the stomach is empty. The patient may at the time be in his usual health, which, however, as will be afterwards seen, has been impaired by nervous exhaustion.

The pain is no less variable in degree than in kind. In some cases, it is intense and fixed to a spot under the epigastrium. Sometimes it is more diffused and cramp-like, as if the stomach was being forcibly dragged or compressed. The pain may even extend over a great part of the abdomen. When it has continued for some time, vomiting of a glairy fluid, or of bile, or of food, if any be present, ensues. In less severe attacks, there is no vomiting.

When the pain is intense, the state of the patient borders on collapse. The pulse is feeble; often slower than natural. The surface is bathed in cold perspiration. The face is pallid, and the features pinched. He rolls on his bed or on the floor in a manner which expresses great suffering. In some instances, stimulants or food give relief; but in others they increase *the pain.* The length of the attack, like the intervals between them, is very

variable; it may only last half an hour, or extend to several hours. For some time after it has subsided, the epigastrium is sore on pressure. In other respects, with the exception of great prostration, the patient feels well. The tongue is pale and flabby, and the bowels may be quite regular. He eats with appetite, and digests quickly.

Most men in practice can call to mind cases to which the foregoing description applies, in which so-called gastric colic, or intense gastrodynia, has time after time unaccountably recurred. They will also bear me out in the assertion that ordinary sedative and tonic treatment is of little avail. But, if the pain is not connected with indigestion, can we be certain that the stomach, and not some adjoining part or organ, is the real seat of suffering? The position of the pain, the sinking and allied sensations, plainly indicate to the patient that it is so, and vomiting very often affords additional proof. As has been said already, the pain always precedes the vomiting, which is, therefore, caused most probably by reflex nervous action. In every instance of the disorder which has come under my notice, the subjects of it were persons of middle age, debilitated by some influence previously at work—such as loss of relations or friends by death, or that no less active cause of depression, loss of money. Malaria was also specially noted as being an excitant.

Dr. Budd has described an affection of the empty stomach "closely allied to waterbrash," brought on by similar causes, except that no mention is made of malaria. He cites two cases which would range under the description of the disease now given. But there is in general a wide distinction between it and waterbrash. Dr Budd's chief remedy is hydrocyanic acid for the pain, and tonic medicines between the attacks. In "persons," he says, "who have once had it," the disorder is very apt to recur,

Dissatisfied with the effects of ordinary remedies, I thought of giving arsenic a trial. There can be no question of the extraordinary value of this drug in many forms of disease, and notably in some neuralgic affections of the head. If, then, it was a true conclusion that this gastric disorder was a neuralgia, the means by which it is subdued in the one situation ought to be effective in subduing it in the other. Here is the first case in which the remedy was tried.

A lady, of stout habit, 46 years of age, consulted me in July, 1863. The sudden death of her husband by accident, and the anxieties about a large family, weighed upon her heavily. About once a week she suffered such violent pain in the stomach, that, although it seldom lasted more than half an hour, left her in a weak condition for a long time afterwards. The pain would come on quite unexpectedly, whether the stomach was empty or contained food; and it was not influenced by diet. There was no habitual gastric symptom except flatulence, and the bowels were quite regular. Bismuth and various remedies were prescribed with little effect. Liquor arsenicalis was now given, at first in doses of three minims, subsequently increased to five minims. This treatment was only continued for a fortnight. The attacks were promptly suppressed by it; and, in a letter written to me at the end of this period, she says that she had already, in consequence of her improved health, encountered with impunity a great deal of fatigue, both mental and bodily. In the course of the following year, the disorder relapsed for a time, apparently from increased anxieties about her children, who had got fever. In this instance the treatment was not continued sufficiently long. Although the beneficial action of the remedy was very apparent, I quote it as one of my least satisfactory cases.

In the following case, the effect of treatment was very marked.

I was consulted, in January, 1866, by a gentleman, 54 years of age, strongly built, but thin and worn looking. His habits were temperate. About two years previously, he was for the first time seized with a most violent cramp-like pain in the stomach, from which he afterwards suffered many repetitions. At first, the intervals between the attacks would be as long as a month; but for some time before this he had been rarely free from them for a single day. They generally came on at night after going to bed; and it is worth noting that he dined early. The pain lasted from two to four hours; and he de-

scribed it as being almost insupportable. It was attended by vomiting of acid fluid, which however did not relieve his suffering. But, with the exception of slight flatulency, he had none of the ordinary symptoms of indigestion. The appetite was good, and the bowels acted well. He had right inguinal hernia, and told me that the late Dr. Babington, under whose treatment he was for some time, was of opinion that the attacks depended on the hernia. A variety of treatment had been tried without effect. He was director of a public company, which, about the time of his first seizure, was broken up under unpleasant circumstances, and caused him great trouble and anxiety. This was ascertained by questioning, and it helped the diagnosis materially. Liquor arsenicalis, in two-minim doses, was ordered to be taken after food three times daily. On the fourth day from commencing to take the medicine, he called and told me, with the exception of the first night, he was free from the attacks, and his nights were altogether better than for a long time previously. The dose was then increased to five minims. On the eleventh day he again called. His nights had been good, and, except on two occasions in very modified degree, there had been no pain. Six minims of the arsenic solution were now prescribed as the dose. On the eighteenth day, he reported himself as going on well; but there had been two slight attacks of pain. The dose was increased to seven minims. On the twenty-fourth day, or six days from the last report, he stated that there had been no return of pain. He was rapidly regaining the appearance of health. In order to make the cure more certain, the medicine was continued for two weeks more; the dose having been increased to eight minims. I saw him lately; he has had no return whatever of the pain, and is in every respect well.

In March of the present year, a case was brought to me for consultation by Mr. Wheeler, of Bexley, under these circumstances. The patient was 38 years old, and rather robust. He had had, for three or four years previously, frequent eructations of what he called acid fatty fluid. But he brought up the same fluid in larger quantity about six p. m. daily, and this was afterwards repeated two or three times during the evening. Severe gastric pain preceded, and was relieved by these discharges. He was flatulent; but the appetite and digestion were good. The bowels were moved several times daily, and the fæces were frothy, and often mixed with mucus, described by the patient as closely resembling frog-spawn. Finding that he had had ague, although many years previously, and that the spleen was now large and tender on pressure, I advised a trial of quinine in three-grain doses, combined with bismuth three times daily; this was continued for some time with little result. I then suggested that arsenic should be given, the dose being gradually increased, and that counter-irritation should be kept up over the spleen. These measures were attended by cessation of the pain and gastric discharges, and great improvement in the condition of the bowels.

Although other remedies were here employed, there can be no doubt that arsenic was the effective agent. It is interesting to observe that, notwithstanding the irritable state of the bowels, it acted as a sedative. The case differs in several particulars from those which precede it. It was a kind of water brash, but differing essentially from that commonly seen, in which arsenic would act as a strong irritant.

The following case shows that the same treatment has proved useful when the pain in the stomach depended upon irritation transmitted from another organ.

The wife of a tradesman consulted me on account of the "dreadful agony" caused by taking any solid food. She had lived almost exclusively on milk for three months previously. There was great flatulence; and the tongue was most remarkable—it was precisely as if thickly coated with white paint, with a yellowish streak in the centre. She was very thin, and had a slight cough. On stethoscopic examination, a large dry cavity was found to exist in the upper part of the left lung. She improved considerably at first under the use of manganese, with small doses of morphia; but the improvement lasted only a few days. Arsenic was now prescribed, with the effect of entirely removing the pain; and she continued free from it for some weeks afterwards, during which she remained under observation.

These cases represent three classes, apparently of very different nature, in which arsenic was curative. But a close connection may be traced between them. A peculiar state of a nerve, call it exaltation or depression according to the theory adopted, is the cause of pain. And when pain occurs spontaneously, without inflammation, and is sudden in its coming and its going, we term it neuralgia. These attributes of neuralgia were well marked in the two first cases. In this class, experience leads me to say that it will hardly ever fail.

In the case dependent upon marsh-poison, as traceable from the previous ague and the state of the spleen, the inscrutable connection between ague and neuralgia suggested itself. This connection led to the exhibition of a remedy of known power against both.

In the case of phthisis, it is worthy of note that the pain was induced by food. But the failure of ordinary remedies for pain after food, as well as analogy, led me to try arsenic. Neuralgia is often developed in parts at a distance from the irritation upon which it depends. Thus, a carious tooth will produce it in the upper part of the face; or a foreign body, as a bullet or a splinter, in one part of the tissues will cause it in another part. The late Dr. Brinton, speaking of the kind of pain at present under consideration, says: "These cases of gastric phthisis may thus be regarded as a kind of neuralgia of the pneumogastric and sympathetic nerves; certain thoracic segments of these conjoined nerves forming the starting point of an irritation and morbid action which is transferred to their abdominal connections." (*Diseases of the Stomach*, second edition, p. 350.)

The contact of food with the stomach in the foregoing case, was the exciting cause of pain. But the effect of treatment alone distinguishes it from ordinary gastrodynia, in which arsenic is always injurious. One of the main indications that the pain in the first class of cases was neuralgic was its great severity. But the probability that gastric pain of the same nature, but of lesser degree of severity, existed, soon occurred to me. Acting upon this, I have treated with success a considerable number of cases of the more moderate kind by the same means. In proportion, however, as severity diminishes, difficulty of diagnosis increases, and here diagnosis is of the greatest importance. Care must be taken, lest the arsenic do harm instead of good. In the ordinary case of pain from intolerance of the mucus membrane of the stomach to the contact of food, arsenic acts as a direct irritant. The gastric pain, sense of heat, epigastric tenderness, and redness of the tip of the tongue, are speedily increased. In doubtful cases, the best rule is not to give arsenic until after due attention has been given to diet, and certain remedies have failed. Bismuth is one of the best known of these. But, if I may be allowed to speak here of another remedy of my own introduction into practice, I would add that the purified black oxide of manganese is superior to bismuth, except in cases to be presently mentioned. If the merits of both drugs were in other respects the same, manganese possesses one great advantage in not constipating the bowels, as bismuth almost invariably does. But, in a few cases, the irritability of the gastric mucous membrane is continued in that of the small intestine The pain is then associated with a tendency to diarrhœa, and the astringency of bismuth becomes valuable. It is in such cases alone that I now employ it.

It will be well to set down here the cases of gastric pain, in order of frequency of occurrence, for which arsenic is unsuitable.

1. Unnatural sensibility of the stomach to the contact of food, except that which occurs in certain cases of phthisis.
2. Pyrosis, not of malarious origin.
3. Subacute gastritis.
4. Ulcer of the stomach.
5. Unnatural sensibility from gastric congestion, due to disease of the heart.
6. Cancer.

In the selection of cases for treatment with arsenic, certain things must be kept well in view. It should be clearly ascertained whether the pain is in-

duced by taking food or not. If the pain be violent, and if it come on generally when the stomach is empty, and be not dependent upon one of the causes specified as being beyond its reach, the remedy will almost to a certainty succeed. If the patient live in a district where ague prevails, and especially if he have had the disease, or if he have had neuralgia of the face or head, there are strong indications for the treatment. But if the papillæ of the tip of the tongue be red and prominent, if the epigastrium be constantly tender on pressure, if the skin be hot and dry and the pulse quickened, the remedy will not succeed.

The usual precaution of giving arsenic soon after a meal should be practised, and it is always proper to begin tentatively. If the drug do not disagree, the dose should be gradually increased. When large doses are reached, a few drops of tincture of opium may be desirable to prevent purging. The constitutional effect of arsenic must, of course, be attended to, as warnings to diminish or to omit its use.

Treatment of Gastrodynia.—[The following is from a letter to the Editor of the British Medical Journal by Dr. J. T. CAMPION, of Dublin. Dr. T. Campion says Dr. Leared's paper recalls to his mind the treatment of gastrodynia by Dr. Ross, Medical Officer of the Warrenpoint and Rostrevor Dispensaries.]

But his mode of administration was the arsenious acid, or common white arsenic in powder—one grain divided into twelve pills—one of which was taken after breakfast and after dinner, daily. Dr. Ross told me that he was in the habit of using this remedy very largely, and that he always found it act almost as a specific in most cases of distressing gastrodynia.

[Dr. NORRIS, of Stourbridge, says:]

For twenty years I have frequently suffered from tic in the face, and a chronic pain in the central bowels, continuing some hours without disorder of any function, except occasional flatulency, and yet it does not appear to have affected my health in any way, for at my age I am unusually healthy and active, with a flushed countenance, free from any tinge on the skin betokening organic disease. Some writers think neuralgia does not affect the health, and this appears in part proved by my case.

I read some excellent remarks on gastrodynia in the British Medical Journal a few weeks ago, which induced me to believe that I had been long suffering from neuralgia in the bowels; in fact, I sometimes thought so, and had now and then found relief by taking arsenic and applying chloroform, but I never gave either of them a fair trial, and have of late trusted to a small dose of laudanum, or one cigar, and the pain has usually faded away. The pain has generally come on two or three hours after dinner; and I at first attributed it to drinking water from a leaden pump many years ago; and, as I and some of my family have been subject to tic in the face, it is quite fair to infer that I have been long suffering from neuralgia in the bowels, and what makes it still more probable, for the last few weeks I have had very little pain in the face and much more pain in the bowels, till I began to take arsenic, as recommended by Dr. Leared, and now for some weeks I have scarcely had any suffering in the bowels, and I think I shall soon be free from complaint. I ought to remark that my abdominal pain generally came on during digestion; and it is probable that other persons subject to neuralgia would have it in an organ or structure predisposed to it when in a greater state of excitement. I took about five drachms of liquor arsenicalis, in doses of five or six drops twice a day. In less than a week, the pain began to diminish, and then gradually left me.

I feel much obliged to Dr. Leared for his valuable and interesting remarks; and I trust and hope the remedy will be fairly tried, and that hundreds of others will obtain the same relief that I have done.

It will probably give the remedy a fairer chance, if the digestive organs are at the same time carefully regulated; and should slight diarrhœa come on after taking it for a few weeks, a few drops of laudanum may be conjoined with each dose, and a light nutritious diet should be taken.—*Brit. Med. Journal, Nov.* 23, 30, *Dec.* 14, 1867, *and March* 28, 1868, *pp.* 467, 494, 560, 297.

26.—A CASE OF ENCEPHALOID DISEASE OF THE LIVER.

Under the care of Dr. WADHAM, at St. George's Hospital.

A case involving some points of interest in regard to cancerous disease of the liver was lately in this hospital. Of 83 such cases analysed by Frerichs, only 7 were in persons under thirty years of age. The age of this patient was twenty-eight. The rapidity of progress was unusually remarkable. As more often happens than not in cancer of the liver, there was no jaundice. Dr. Reginald Thomspon, medical registrar, has obliged us with notes.

The patient, Henry H., was a carpenter, who was admitted on Nov. 14th, 1867. He had been in perfect health until five weeks before admission. His illness began with rigors, and pain in the right side of the thorax and in the liver. He had not been jaundiced.

On admission, he was suffering principally from dyspnœa. On being closely questioned he said he had a feeling of constriction—of tight lacing—across the liver. This organ was found to be enormously enlarged; it extended from the level of the right nipple to below the umbilicus, and measured in this direction eleven inches; it was nodular and hard. There was no tenderness and no jaundice. The right lung was dull, and the sputa contained a large quantity of elastic tissue. The cervical veins were much distended. The disease was diagnosed to be encephaloid deposit in the liver and right lung. The patient died on the 22nd November, the liver having increased in size.

The post-mortem examination was made by Mr. Pick. Several masses of encephaloid cancer were found in the right lung and in the liver. This organ was of an enormous size, and weighed 210 ounces.—*Lancet, Jan.* 4, 1868. *p.* 12.

DISEASES OF THE URINARY ORGANS.

27.—PEROXIDE OF HYDROGEN AS A REMEDY IN DIABETES.

By Dr. JOHN DAY, Geelong, Australia.

[The patient was 36 years of age, and was progressively getting worse, passing as much as five quarts of highly saccharine urine during each night.]

While pondering over the hopeless condition of my patient, it occurred to me that if I could oxidise the sugar that had been taken up in the general circulation, it would be an approach towards the natural mode of its elimination by the lungs. With this object in view, I gave half-drachm doses of ethereal solution of peroxide of hydrogen mixed in an ounce of distilled water, three times a day.

To enable me fully to explain the theory on which I base my treatment, would occupy far more of your valuable space than I could justly claim. Schönbein believes that peroxide of hydrogen is HO + antozone, and that the blood-corpuscles possess, in a very high degree, the property of decomposing it, and of transforming its antozone into ozone, without, in themselves, undergoing any very rapid change; and he further believes that ozone is the only condition in which oxygen possesses any active combining properties. Assuming these views to be correct, we should possess in ethereal solution of peroxide of hydrogen, which would be rapidly absorbed, a ready means of destroying, by oxidation, the sugar in the blood, and of also maintaining the animal heat, which, in the treatment of diabetes, is an important consideration. I may observe, that what is sold by Mr. Robbins as Dr. Richardson's ozonic ether is, in reality, a solution of peroxide of hydrogen in ether. This may be readily proved by adding a few drops of it to a weak solution of chromic acid: a beautiful blue colour will be the result, caused by the formation of perchromic acid. This preparation is in every respect similar

to that which I have been using, and in the therapeutical effects of which I have now had some years' experience.

I commenced the use of this new remedy on August the 10th, and, as the following extracts from my case-book will show, with most gratifying results to the patient:

Aug. 12th. From 10 p.m. to 10 a.m., passed about five pints of urine. Previously, for many months, the quantity of urine passed during the night averaged five quarts.

13th. Quantity of urine passed during the night, rather less than three pints and a half. Thirst not so urgent.

14th. Quantity of urine passed during the night, two pints and a half. Urine strongly acid; specific gravity 1046. Thirst much less urgent.

16th. Quantity of urine passed during the night, rather less than forty ounces. The patient very much improved in every respect. I give her own words:—"I have no thirst now; no more than I had in olden times. I feel that I am cured if it will only last."—*Lancet*, *Jan* 11, 1863, *p.* 45.

28.—ON THE URINARY PIGMENTS.

By Dr. Henry Veale, Royal Artillery.

The plans which are ordinarily recommended for examining the urinary pigments are either very troublesome, or of such little practical value as to be but seldom had recourse to by the clinical physician. The method recommended by Dr. Payne (*Indian Annals*, Sept., 1858) was to boil a certain quantity of urine in a test-tube, and then to treat it with nitric acid, drop by drop. By this proceeding, the urine usually becomes darker with each addition of the acid until a maximum of colour is produced; after which, if more acid be used, the changes are retrogressive. Dr. Payne was of opinion that, in cases of hepatic disease, the colour thus produced was always much darker than in health. In my hands, however, this has not constantly been the case. In inflammatory diseases of the liver, I have frequently observed exactly the reverse; and notwithstanding many trials, I have been unable to determine that Dr. Payne's method is of any diagnostic value.

The simple proceeding of Heller and Carter of allowing a certain portion of urine to mix slowly in a test-tube with a smaller quantity of sulphuric or hydrochloric acid is of more value. I have not, however, been able to obtain sufficient uniformity in the results from the use of either of these acids. As tests available at the bedside, they are decidedly inferior to the strong nitric acid when properly employed.

Dr. Andrew Clark of the London Hospital has shown that there is no better mode of testing for small quantities of albumen than that of allowing a small quantity of urine to trickle slowly down the side of a test-tube upon about a half a drachm of fuming nitric acid. I have employed this test extensively, and fully agree with all that Dr. Clark has said as to its certainty and simplicity. I had not used it long, however, before I was struck with the difference in the appearance of the pigmentary band. By this term I mean the coloured rings which form at the line where the urine floats upon the acid. The quantity of nitric acid employed should always be sufficient to reach the portion of the test-tube, which is uniform in calibre. If the acid be poured into the tube on one side, the urine should be allowed to trickle down only on the opposite; and it will be found convenient to use a pipette for letting the urine flow into the tube. If more than a drop or two runs down at once upon the acid, there is apt to be a mixture of the fluids, which spoils the experiment. Inclining the tube at an angle of from 50 to 60 degrees, or even more, much facilitates the operation, but in restoring the tube to the vertical position, care must be taken not to agitate the contents. To see the result, the tube should be held in a certain light, and generally it will be best to have a white background, such as a whitewashed wall or a sheet of white paper.

If the experiment has been properly performed, there will be found, when healthy urine has been employed, a yellow ring at the line of contact of the two fluids, and over this a narrow ring of an almost ruby-red colour. In many scores of observations, I have not yet met with an exception to this rule. In certain morbid states of the system, however, instead of the ruby ring, the urine strikes a purple, bluish, or absolutely blue ring. When bile is present, there is also a green ring. These coloured rings are produced immediately, and are at first very distinct, but they gradually cease to be visible as the mixture of the urine and acid progresses.

In the intermittent and remittent fevers of India the urine will frequently display the purple and blue rings. In nearly every case of dysentery that has come under my observation since my attention was drawn to this subject, I have found a decidedly blue ring to be produced so long as the disease was progressive, and the ruby-red ring to reappear when the patient began to recover. I have not often seen a blue ring in cases of congestion and enlargement of the liver, unless there happened to be some other concomitant state, such as fever or diarrhœa, to which it might be ascribed. In a case of enlarged liver, anæmia, and consequent functional cardiac disorder, attended with much debility, which came under my notice some months ago, there was no movement of the bowels for six days, notwithstanding several doses of colocynth pill, calomel, and julep, castor-oil, &c. On the sixth day of the constipation, I prescribed one minim of croton-oil, which occasioned one motion after the lapse of several hours. I then ordered the dose to be repeated, and the result was, that within the ensuing twenty-four hours the bowels were moved eight times. After the eighth motion, I examined the urine in the manner above described, and found a distinctly blue ring to be struck over the yellow. During the next twenty-four hours, there was no movement of the bowels, but the urine still presented the same appearance. On the third day the bowels were still constipated: the urine, however, no longer displayed the blue ring. On the fourth day, after a dose of pil. aloes and myrrhæ, the bowels were moved eleven times; the pigment then became of a dark purple colour, but not distinctly blue. From this time he improved, and the blue and purple rings ceased to be produced. From this case it would appear as if even artificial purgation were sufficient to induce the change in the condition of the urinary pigment.

This blue or purple pigment is of course no other than the uroglaucin of Heller, or the indigo-blue of Carter and Schunk; but although it may be possible, as the latter authors have shown, to obtain this substance from nearly every specimen of normal urine, it would be erroneous to conclude that the almost instantaneous formation of the blue ring, when urine and nitric acid are brought into contact in the manner above mentioned, is a normal or physiological change. Up to the present time, I have never seen it produced in the urine of healthy persons. As a temporary symptom in the course of a malarial fever, I have not observed it to indicate danger to life or the reverse; but in all cases hitherto in which it has been detected day after day, where it has been persistent, in short, the disease has taken a downward course.

Judging from the nature of the cases in which this condition of the urine is found (dysentery, severe malarial fevers, excessive purgation, &c.), I think it may be concluded that it is in direct relation with the amount of destruction or loss of the colouring matter of the blood, and with the inability of the system to repair the waste at the time. Under normal conditions, it would appear that the supply of colouring matter to the blood somewhat exceeds the waste, but that, in certain diseases in which the loss of colouring matter is excessive, or in which the supply of the requisite materials cannot be duly profited by, there is an interruption in the process of pigment formation, which probably essentially consists in an insufficient amount of oxidation. Whatever may be the true explanation, it is to be hoped that those who are practising in cold or temperate climates, and who have to deal with diseases of a different kind from those usually witnessed in India, will give to the profession the result of their observations on this subject.—Edinburgh *Medical Journal, Dec., 1867, p.* 548.

SURGERY.

BONES, JOINTS, &c.—AMPUTATIONS, FRACTURES, DISLOCATIONS.

29.—ON FRACTURE OF THE THIGH.

By LAWSON TAIT, Esq., Resident Surgeon, Clayton Hospital, Wakefield.

[In the following paper the writer brings under notice the particulars of some cases in which he used a new plan of treatment of fracture of the thigh, with good results.]

The method referred to is that usually known as Buck's, and is described in Hamilton's elaborate treatise. Two years ago I had the good fortune to make the acquaintance of Dr. George K. Smith, of Brooklyn, U. S., who had been attached to one of the rear hospitals of the Federal army during the late war. Dr. Smith is the author of a most exhaustive paper on fractures of the neck of the femur, and on this account was allowed to select, as an especial study, cases of gunshot fracture of the thigh. His treatment consisted entirely in the use of Buck's method, and the success obtained by it has been extremely satisfactory. In the elaborate reports and catalogues issued by the Army Medical Department his cases are noticed; but I had the opportunity of examining photographs of many of his patients, and must express my opinion that the success obtained during that unfortunate war in the treatment of gunshot fractures of the thigh was such as immeasurably exceeded that of any other known campaign.

The principle on which the plan is based—that of continuous extension by a weight and pulley—is not new, but the credit of adapting it successfully to the treatment of fractures of the thigh is certainly due to the American surgeons. The following will be found a convenient method of applying Buck's treatment:—Having placed the patient on a convenient bed, fasten two strips of ordinary diachylon plaster (that spread on unglazed calico is best) reaching from about three inches above the knee to the malleolus, and from two to three inches wide, one on either side of the injured limb, and leave as much as will tie round a piece of wood adapted to the sole of the foot. This foot-piece must have a hole bored in it about three inches from the heel, through which a cord is run from above downwards and secured by a knot; it must also have a flat notch on either side in which the plaster straps are laid. The cord ought to be in front of the straps. The limb is now to be carefully bandaged from the toes to the upper ends of the plaster straps, and the bandage to be saturated with dextrin or paraffin. The foot of the bed is to be raised about four inches to obtain counter-extension by the weight of the body. The cord is to be reeved over a pulley, and have a weight attached to it. The fragments having been adjusted, four Gouch splints are to be applied of suitable length on the four surfaces of the limb, and these are best retained in position by small straps buckled. A slight addition, which I have found of great importance in preserving the heel from injury, consists in having a small cradle placed over the foot, to which the foot is slung by a small cord running through a hole in the toe of the foot-piece; this also prevents the tendency to eversion which the natural form and axis of the limb create. The more robust the patient, the greater the extending weight required, and more weight is necessary during the first week than *afterwards*. In children I have had good results with so slight a weight as *four pounds*; and in the case of a full-grown man I have had to use sixteen *pounds*. I have treated fractures of the thigh in all the various sites of [illegible]

tures, and the result has been uniformly satisfactory. Of course, in cases of fracture close to or above the trochanter the splints on the thigh are unnecessary—the extension alone must be trusted to. It is not necessary that the splints should command the knee-joint. Indeed, except in cases when the fracture is very close to the condyles, I think it better that they should not do so. There is no object to be gained in restricting all motion of the hip-joint; indeed, all the cases have been allowed, if they wished it, to sit up in bed after the first week.

The true indication for treatment in all fractures is to reduce the patient, as far as his injured part is concerned, to the condition of a crustacean, to supply him with a temporary dermoid skeleton until his vertebrate is repaired. The best illustration of the successful fulfilment of this indication is to be seen in the treatment of Colles' fracture by means of Professor Gordon's splint; and when we have equally efficacious means of treating every injury to the osseous system this department of surgery will be perfect.

The cases in which I have used this treatment are eleven in number—two compound and nine simple.

Besides the actually better results obtained from this method of treatment the patients are much more comfortable during its continuance than under any other that I have seen. In cases where bedsores threaten they can be more easily obviated in this method than in any other.—*Dublin Quarterly Journal, Feb.*, 1868, *p.* 71.

30.—ON THE FRACTURES OF THE PATELLA, TREATED BY A RING.

By Dr. William A. Gibson, St. Louis, U. S.

[The account of the following case was first published in the St. Louis Medical and Surgical Journal, but it is taken by us from a pamphlet by Dr. Paul Eve.]

The patient, Mr. James Ferree, aged 35 years, had been the victim of rheumatism, and having so far recovered that he could use crutches, in attempting to get into a buggy, felt a sudden shock, as if some one had hit him with a stone on the knee, and could scarcely be convinced that the fracture was caused by muscular contraction.

The patella of the left knee was fractured transversely through its middle, the fragments separating about one inch. I did not admire any of the modes of treatment for the fracture which I had ever seen recommended or used, and while hesitating what plan I would adopt, it occurred to me that a ring would answer the purpose. I therefore took a measurement of the sound patella, and had a ring made of iron (allowing for padding), which I padded well with cotton wadding, cut in strips and wrapped around the ring, over which I applied a bandage. To each side of the ring I sewed strips of bandage. I then placed a well-padded splint twenty-four inches long to the posterior aspect of the leg and thigh, which I secured by a few turns of bandage at the lower and upper ends, the bandage being loose so as not to interfere with the circulation. I next brought the two fragments of bone into apposition, and placed the ring around the patella, and tied the strips of bandage over the splint thus securely holding the ring in its place, and keeping the broken bone always in complete apposition, thereby giving the greatest possible chance for a bony union. At the expiration of thirty days, I removed the ring, and commenced passive motion of the limb, and to-day (June 15th) he has very good use of the limb. During the time this case was under treatment, I invited Drs. E. H. Gregory, J. J. McDowell, E. A. Clark, E. S. Frazier, and other physicians, to examine the appliance, and they all expressed the opinion that it was *just the thing* for a fractured patella. The advantages of the ring in the treatment of this troublesome fracture are so apparent, that it seems to me hardly necessary to point them out. The appliance did not give the patient the least pain, and there was no interruption of the circulation by the bandages. It was impossible in this case for the

patella to escape from the ring, but possibly in some cases, as of women when there is a good deal of adipose tissue, and but little prominence of the patella, it may not be so easy to apply the ring; but I am persuaded that it will give entire satisfaction in all cases. I claim by the application of this ring to have reduced one of the ugliest fractures of the human frame to one of the simplest for treatment.—*Pamphlet on Fracture of the Patella, p.* 4.

31.—A NEW APPARATUS FOR THE TREATMENT OF FRACTURES OF THE LIMBS.

By Christopher S. Jeaffreson, Esq., Senior Resident Surgeon to the Royal Free Hospital.

[In this paper Mr. Jeaffreson recommends the use of elongated water cushions, placed between the splints and the limb, the splints being applied just as usual. Three are required for the leg, one for either side and one for the under surface of the limb. The idea is a good one, on account of the evenness of the pressure produced, and the consequent comfort of the patient. We do not think it necessary to give the paper. The apparatus may be seen in use at the Royal Free Hospital.]—*Lancet, Nov.* 9, 1867, *p.* 576.

32.—ON THE TREATMENT BY MANIPULATION OF DISLOCATION OF THE THIGH BACKWARDS.

By George W. Callender, Esq., Assistant Surgeon to, and Lecturer on Anatomy at St. Bartholomew's Hospital.

[The patient, whose case is related, was a robust labourer. The head of the femur lay low down in the ischiatic notch. Various attempts at reduction were made, but without success.]

Very much vexed at the failure of these attempts, I was at the time disposed to think that the position and extent of the rent in the capsule might be the source of difficulty. The capsule might be interposed, curtain-like, between the head of the femur and the acetabulum, or the rent in the capsule might be so situated that the head of the bone could not be brought opposite to, or directed so as to pass through it. But on looking more closely into the question, I am of opinion, from the evidence we possess, that the capsule could not in this instance, and indeed that it never can, offer any obstacle to the reduction of dislocations at the hip. As Sir Astley Cooper says, the idea of the neck of the femur being girt or confined by the capsule is quite untrue; the violence, indeed, which causes the dislocation (I write only of dislocation the result of violence), and which forces the bone through the capsular ligament, ensures also that the latter shall be torn to pieces. An account of a rare form of dislocation on the tuberosity of the ischium has been given by Mr. Wormald who has also described the specimen which is in the museum of our hospital. It may be referred to as showing the great damage sustained by the capsular ligament. In a dislocation of the femur upon the ischiatic notch, which occurred three weeks before death, the opening in the posterior part of the capsule is widely torn, although its size has been reduced by partial union. In a recent dislocation upon the lower edge of the obturator externus muscle the laceration of the capsule on the inner (obturator) and lower sides is extensive; and in a fourth case, one of dislocation in the ischiatic notch, the capsule is described as widely lacerated by the head of the bone, and the rent is so considerable that it is evident the ligament could offer no obstacle to the reduction of the displaced femur. I have looked through the specimens in the museums of several of the London hospitals, and I find that they show just the same state of the parts, tearing of the capsule to a great extent, and a free passage for the head of the bone into the acetabulum.

Dr. Fenner has, however, recorded an instance of a rent in the capsule (which

was half torn through) through which the head of the bone could not be forced, even when the parts were tested at a post-mortem examination. Such a case must be exceptional. I do not consider our present pathological experience would justify us in thus explaining any failure in the reduction of a dislocated hip.

Not long ago I examined a recent dislocation at the shoulder, and found the head of the humerus driven through the subscapularis, so that the neck of the bone was tightly surrounded by muscular fibres; and I have seen with Mr. Wormald a dislocation of the radius forward which could not be reduced because, as we thought at the time, the bone had been forced through, and its neck nipped by the supinator radii longus. So also it is just possible that with luxation at the hip, muscular fibres may interpose and hinder the return of the head of the bone into its socket: thus in Wallace's case the edge of the glutæus medius passed over the head of the femur, and in a case related by Mr. Syme the head of the thigh-bone was imbedded in the substance of the glutæus maximus. I do not lay much stress upon the difficulties which may originate in such conditions as the preceding, but they are worth a passing notice, as also in the hindrance to reduction which has been known to result from locking of the head of the bone when deeply placed in the ischiatic notch, as in one of the cases recorded by Mr. Wormald.

Sir Astley Cooper has described the difficulty which attends the reduction of dislocation on to the ischiatic notch, but I am not aware that any explanation of this difficulty has been offered, save the one which he refers to in describing the distance which the head of the bone has to travel outwards from the fossa of the ischium before it can be lifted over the edge of the acetabulum. Hence the stress which has been laid, since his observations were published, upon the importance of endeavouring to lift the head of the bone upwards and outwards whilst extension is being made.

There is, however, one very evident obstacle to the reduction of this class of dislocations to which attention may be directed, as it seems to give the only fair explanation of the difficulties which are experienced in the treatment by extension as well as by manipulation. When the head of the bone lying on the ischiatic notch is pulled forward by extension, it is drawn over the convex outer surface of the acetabulum; but instead of passing towards the socket it happens, from the formation of this part of the pelvis, that it can follow an easier route which is open to it. From the lower part of the great sciatic notch a broad, smooth, pulley-like surface leads to, and then curves round, the inferior border of the acetabulum, and thence ascends towards the obturator foramen. The obturator internus above, and the great sciatic nerve, and the upper border of the obturator externus below, rest within it; but between these the groove is covered with fat; and from its surface fibres of the capsular ligament take their origin. If the head of a femur be placed in its corresponding pelvis in this groove it will be seen how accurately the convexity of the head of the bone fits the concavity of the ischium. If also the head of the femur be placed on the sciatic notch, and is then moved towards the socket, it will be found that it slips over the convex outer surface of the acetabulum, and is directed into and along the groove just referred to; and this I believe must happen when extension is employed for the reduction of these dislocations—the head glides below the socket, and slips up again when extension is remitted. In attempting to replace the femur by manipulation, as this operation is usually practised, by the rolling outwards of the shaft, the head is also carried along this groove, its progress through which may be traced easily enough, and incurs the risk of passing round the acetabulum, so that the head, when the thigh is extended, comes up on the obturator side, or rises even to the pubes, having described a half circle below the socket. This rolling of the head round the acetabulum is a chief objection to the plan of reduction by manipulation, and is almost sure to occur if the thigh is rotated outwards; in this way the ischiatic nerve has been seriously crushed, and the bruising of the parts has been such as to give rise, in one instance at least, to subsequent disease of the hip-joint.

To return to the case before us. After the patient had rested for several

days I adopted the following plan:—The thigh was bent upon the abdomen, and I slowly moved the limb into a straight line with the body, so that the head of the bone could be felt projecting in the buttock, outside the tuber ischii. The limb, in a straight line with the trunk, without allowing any rotation outwards, was then drawn forward from the abdomen, and forced downwards (extended), and the head of the bone at once slipped into the acetabulum. These movements were made slowly and steadily, and the limb was extended with care, remembering the great leverage which we were making use of, and the position of the head, which was being pressed up into the socket. In two recorded cases the neck of the femur has been broken under a somewhat similar strain.

If these manœuvres are examined by the help of the skeleton, it will be found that by flexion, and by moving the thigh into a straight line with the body, the head is brought from the notch into the groove just above the outer side of the tuber ischii. Here it is opposite the least prominent part of the lower edge of the acetabulum, and if the femur is depressed whilst in this position the head easily slips into the socket. Dr. Markoe, with the addition of a rocking movement as the thigh is extended, and Dr. Hamilton, employ somewhat similar manœuvres, and they speak favourably of their success.

I have been anxious to give some explanation of the manner in which the method by manipulation acts, and to insist upon the importance of not abducting or rolling the limb outwards, for if this is done the head of the bone is almost certain to roll past the acetabulum to its inner side; or if an obturator dislocation is under treatment, and the thigh is rotated inwards, the head of the femur will, as I have several times seen it, roll round on the ischia to notch, just reversing the movement which takes place when an ischia dislocation is improperly manipulated.

Three steps complete the operation. First, the thigh is bent on the abdomen; secondly, it is brought into a straight line with the long axis of body; thirdly, it is forced down (or extended) in a straight line, parallel with its fellow. The dislocation is thus reduced without difficulty and without the need of any assistant. And, what is of chief importance, the operation avoids all risk of rolling the head of the bone round the acetabulum, an accident which is so apt to complicate manipulation as commonly practised.—*Lancet, March* 14, 1868, *p.* 343.

33.—ON A NEW METHOD OF REDUCING DISLOCATION OF THE HEAD OF THE HUMERUS.

At the Frankfort meeting of German Naturalists and Physicians, Dr. Heine, of Heidelberg, described the following novel plan of reducing a luxation at the shoulder-joint. The case in which it was first applied with success, was one of subclavicular luxation of the humerus of seven weeks' standing. Extension and other means of reduction had been attempted, with the aid of chloroform; but with no success, as the head of the bone could not be lifted over the anterior border of the glenoid cavity. The elbow of the affected side being bent at a right angle, Dr. Heine grasped the arm with his right hand and the forearm with his left: he then raised the extremity to the vertical position and forced the elbow backwards, so that the upper arm formed an obtuse angle with the posterior surface of the neck (hyper-elevation and retroflexion). The humerus was thus converted into a lever of the second form. The scapula formed the fulcrum upon which the surgical neck of the humerus rested, and the head forming the short arm of the lever was, whilst the shoulder was forced by an assistant, readily brought up to the level of the articular surface of the scapula. The arm was then circumducted from behind forwards—the head of the patient being turned to the opposite side; and as it was finally depressed and rotated inwards, the head of the humerus slipped audibly into the articular cavity. The action of the limb was now free, and the patient in a short time was able to perform nearly all the normal movements.—*Deutsche Klinik*, 46, 1867.—*Brit. Med. Journal, Feb. 22, 1868, p.* 168.

34.—CASES OF WOUNDS INTO JOINTS.

Under the care of JOHN BIRKETT, Esq., at Guy's Hospital.

Penetrating wounds of the large joints always excite alarm in the mind of the surgeon. Even incised wounds, most carefully made, and with every precaution adopted to prevent inflammation, as for the removal of loose cartilages from joints, have been attended with disastrous consequences. Wounds inflicted by rough tools, and complicated with injury to the surrounding parts, must always be attended with corresponding risk to the integrity of the injured member and the life of the sufferer. Taken in time, however, and carefully treated with the appliances of modern surgery, the danger accompanying these serious injuries may at least be averted, and as the following cases illustrate, the usefulness of the injured joint need not necessarily be compromised. The principles upon which the treatment of the injuries was carried out were as follows: the immediate closure of the wound, the adoption of perfect repose of the body and immobility of the injured member, the local application of ice, and a moderate but nourishing diet.

Case 1.—*Incised wound into the knee-joint.*—The report taken by Mr. E. N. Edwards, the dresser of the case, is as follows:—On Feb. 8th, 1867, a healthy carpenter, 25 years old, fell from a height of several feet whilst carrying a basket of tools, with the sharp edge of one of which a wound was inflicted over the outer side of the left knee-joint. It was partly over the patella, and extended obliquely backwards and downwards about an inch and a half as the limb reposed in the extended posture. The edges of the wound were carefully kept in contact by adhesive plaster and a compress of lint, but synovial fluid escaped freely from the wound the day after the accident. The surface of the whole joint was warmer than that of the opposite side, and the capsular ligament was distended with fluid. The limb was fixed on a straight back splint, ice applied over the joint, and a moderate but nourishing diet allowed.

The progress of this case was satisfactory; for neither local inflammation nor constitutional disturbance to excite apprehension arose, and the upper half of the wound healed in a few days. Synovial fluid continued to ooze from its posterior termination, and retarded the healing process. Ice was continued for twelve days, when the effusion into the joint had subsided. The wound was not covered, and it seemed to heal more quickly under this condition, an incrustation forming over it, beneath which cicatrisation advanced.

The man left the hospital forty-eight days after the infliction of the injury quite well.

Case 2.—*Lacerated wound of leg communicating with the knee-joint.*—The following case is reported by the dresser, Mr. J. A. Sharp:—A healthy, hard-working man, 21 years old, was admitted on August 23rd last into Accident ward, in consequence of an injury received about an hour before, and produced by the kick of a horse. The calkin of the horse-shoe had struck the man just below the left knee-joint, the articulation at the moment being flexed. A wound was inflicted in front of the leg, over which a surgeon quickly placed a pad of lint and plaster. As blood flowed from beneath this dressing, it was removed, and a contused lacerated wound of the integuments was seen. It was somewhat quadrilateral, oblique in direction, and about three-quarters of an inch in diameter, and situate two inches and a half below the anterior edge of the upper articular surface of the tibia, a little below the tubercle of the tibia. The soft parts were cut through to the surface of the tibia. The knee-joint was swollen, and when pressure was made over the patella, the blood flowed more freely from the wound. The last observation led to a very careful examination of the injury by Mr. Birkett, who found that there was not only blood within the synovial capsule, but also air; for upon continuing the compression, bubbles of air came out of the wound with the blood, and upon percussing over the swollen joint, the peculiar dull sound of a circumscribed collection of air beneath the integu-

ments was recognisable by all the bystanders. It was not possible to expel all the air; but the size of the distended capsule was much reduced by expressing the blood from within it. The diagnosis was then clearly established, that from the integumental wound a free passage extended through the synovial membrane into the knee-joint, that blood flowed into its cavity, and that air had also gained admission therein. The limb was placed in an extended posture, and kept so by means of sand-pillows. Pressure was made over the joint to expel as much of the blood and air as possible; a compress of lint was strapped over the track of the injury *between* the wound and the head of the tibia, and water-dressing was applied on the wound itself. In spite, however, of this local treatment, the joint soon became again swollen, probably from effused blood. The character of the bleeding was venous. Strict repose was enjoined, ice was kept constantly in contact with the whole anterior and lateral surfaces of the joint, and a moderate diet only was permitted.

There was not an untoward symptom in the progress of this man's recovery. The wound healed in about a fortnight; the joint continued swollen, but was never painful; and he was very unwilling to follow the strict rules imposed as regards the absolute repose of the member. Ice was employed for three weeks, it being continued after the external wound was healed. For five weeks the man was kept in bed, and appliances used to maintain extension of the joint; and when he left the hospital, forty-three days after the infliction of the injury, the movements of the joint caused no pain.

The patient called at the hospital on Dec. 2nd, and stated that he did not experience the slightest inconvenience from the injury.

Case 3.—*Wound of the elbow-joint, complicated with fracture of the olecranon.*—This patient was admitted on the 4th April last, and from the report of the case by Mr. C. H. W. Parkinson it appears that the man, 20 years old, was struck at by a fellow-workman with an ordinary carpenter's hand-saw, the back edge of which was very thin and sharp. With this weapon a wound was inflicted transversely across the posterior region of the left elbow-joint. The integuments were divided for about two inches in length, rather obliquely; and three-quarters of an inch of the olecranon ulnæ was cut off, which retained its ligamentous and tendinous attachments, and allowed the cavity of the joint to be seen during its flexion. Three sutures were employed to adjust the wound of the skin, and the arm was placed on a straight splint, well padded at the elbow. Moist lint was laid over the wound. After a day or two the wound became inflamed, and the man complained of pain. Ice was applied over the joint, which reduced the inflammation, although the wound assumed a sloughing aspect. The sutures were removed, and a lotion with permanganate of potash was applied over the wound. An angular screw splint was substituted in place of the straight one, but this, even, caused pain and swelling of the arm. In a few days it was removed, and the arm steadied by pillows. Ice was used for thirty days. After forty-six days the wound was quite healed, but still the joint was kept perfectly at rest, somewhat flexed. He left the hospital in sixty-nine days after receiving the injury, the member saved, but the movements of the joint impaired.—*Lancet, Jan.* 4. 1868, *p.* 11.

35.—ON THE ANTISEPTIC SYSTEM OF TREATMENT IN SURGERY.

By Joseph Lister, Esq., F. R. S., Professor of Surgery in the University of Glasgow.

The cases in which this treatment is most signally beneficial are divisible into three great classes—incised wounds, of whatever form; contused or lacerated wounds, including compound fractures; and abscesses, acute or chronic—a list, indeed, which comprises the greater part of surgery. In each of these groups its aim is simply to prevent the occurrence of decomposition in the matter of the wound, and thereby that its reparatory powers may be left undisturbed by the deleterious and poisoning influence of putrid materials. In

pursuing this object we are guided by the "germ-theory," which supplies us with a knowledge of the nature and habits of the subtle foe we have to contend with; and without a firm belief in the truth of that theory, perplexity and blunders must be of frequent occurrence. The facts upon which it is based appear sufficiently convincing. We know from the researches of Pasteur that the atmosphere does contain among its floating particles the spores of minute vegetations and infusoria, and in greater numbers where animal and vegetable life abound, as in crowded cities or under the shade of trees, than where the opposite conditions prevail, as in unfrequented caves or alpine glaciers. Also it appears that the septic energy of the air is directly proportioned to the abundance of the minute organisms in it, and is destroyed entirely by means calculated to kill its living germs—as, for example, by exposure for a while to a temperature of 212° Fahr., or a little higher, after which it may be kept for an indefinite time in contact with putrescible substances, such as urine, milk, or blood, without producing any effect upon them. It has further been shown, and this is particularly striking, that the atmosphere is deprived of its power of producing decomposition as well as organic growth by merely passing in a very gentle stream through a narrow and tortuous tube of glass, which, while it arrests all its solid particles, cannot possibly have any effect upon its gases: while conversely, "air dust" collected by filtration rapidly gives rise simultaneously to the development of organisms and the putrefactive changes. Lastly, it seems to have been established that the character of the decomposition which occurs in a given fermentable substance is determined by the nature of the organism that develops in it. Thus the same saccharine solution may be made to undergo either the vinous or the butyric fermentation, according as the yeast plant or another organism, described by Pasteur, is introduced into it. Hence we cannot, I think, refuse to believe that the living beings invariably associated with the various fermentative and putrefactive changes are indeed their causes. And it is peculiarly in harmony with the extraordinary powers of self-diffusion and penetration exhibited by putrefaction that the chief agents in this process appear to be "vibrios" endowed with the faculty of locomotion, so that they are able to make their way speedily along a layer of fluid such as serum or pus.*

Admitting, then, the truth of the germ theory, and proceeding in accordance with it, we must, when dealing with any case, destroy in the first instance once for all any septic organisms which may exist within the part concerned; and after this has been done, our efforts must be directed to the prevention of the entrance of others into it. And provided that these indications are really fulfilled, the less the antiseptic agent comes in contact with the living tissues the better, so that unnecessary disturbance from its irritating properties may be avoided.

The simplest conditions are presented by an unopened abscess. Here as no septic particles are present in the contents, it is needless to apply the antiseptic directly to the part affected. All that is requisite is to guard securely against the possibility of the penetration of living germs from without, at the same time that free escape is afforded for the discharge from within. When this is done we witness an example of the unaided curative powers of Nature as beautiful as it is, I believe, entirely new. The pyogenic membrane, freed from the operation of the stimulus derived from the presence of the pus pent up within it, without the substitution of the powerful stimulus of decomposition as has heretofore been the case after the opening of abscesses, ceases at once to develope pus-corpuscles, and, exuding merely a little clear serum, rapidly contracts and coalesces, discharging meanwhile its unirritating contents completely, whether the outlet be dependent in position or otherwise. At the same time the irritative fever and hectic hitherto so much dreaded in large abscesses are, with perfect security, entirely avoided.

* I have seen vibrios, so minute as to be only just discernible with the highest power of an excellent microscope, shoot across the field of view with a velocity that astonished me.

In suppurations of the vertebræ or of the joints the results of this system are such as I ventured with trembling hope to anticipate; patient perseverance being rewarded by a spontaneous cure in cases where excision, amputation, or death must have resulted from any other known system of treatment. In short, the element of incurability has been eliminated from Caries.

In compound fractures and other severe contused wounds the antiseptic agent must in the first instance be applied freely and energetically to the injured parts themselves, the conditions being the opposite of those in unopened abscesses. The wound being of complicated form, with its interstices loaded with extravasated blood, into which septic organisms may have already insinuated themselves during the time that has elapsed before the patient is seen by the surgeon, mere guarding of the external orifice, however effectually, is not sufficient. After squeezing out as much as possible of the effused blood, a material calculated to kill the septic particles must be introduced into the recesses of the wound: and if the substance employed is of sufficient strength to operate to a certain extent as a caustic, this is regarded as a matter of little moment in comparison with the terrible evil of inefficiency in its antiseptic action. For experience has abundantly shown that parts killed in this way, including even portions of bone, become disposed of by absorption and organisation, provided that the subsequent part of the treatment is properly managed.

Sloughs, as ordinarily observed, are soaked with the acrid products of decomposition, and therefore produce disturbance upon the tissues around them, leading first to their gradual transformation into the rudimentary structure which when met with at the surface of a sore, is termed "granulations," and afterwards to the formation of pus by the granulations. But a dead portion of tissue, if not altered by adventitious circumstances, is in its proper substance perfectly bland and unirritating, and causes no more disorder in its neighbourhood than a bullet or a piece of glass, which may remain embedded in the living body for an indefinite period without inducing the formation of pus; while the dead tissue differs from the foreign bodies alluded to in the circumstance that the materials of which it is composed are susceptible of absorption.

Antiseptic substances, being, like the products of decomposition, chemically stimulating, will, like them, induce granulation and suppuration in tissues exposed for a sufficient length of time to their influence: but there is this all-important difference, that an antiseptic merely stimulates the surface to which it is applied, becoming diluted and weakened by the discharge which it excites; but the acrid salts which result from putrefaction are perpetually multiplied and intensified by self-propagating ferments, so that every drop of serum or pus effused through their agency becomes a drop of poison, extending its baleful influence both in the injured part and in the system generally.

These pathological considerations indicate the after-treatment of compound fracture, and explain the progress of the case. The antiseptic introduced into the wound is soon washed out by the discharge or carried away by the circulation, so that the blood and sloughs at first imbued with it become unstimulating and amenable to absorption, while at the same time they are prone to decomposition should any living atmospheric germs gain access to them. The further treatment, therefore, must consist in maintaining an efficient antiseptic guard over the orifice of the wound until sufficient time has elapsed to ensure complete consolidation of the injured parts.

The sanious and serous discharge which occurs at the outset will give place in a few days to a small amount of pus, if the wound is dressed in such a way that the antiseptic continues to act upon the raw surface. This discharge, due to the stimulating nature of the application, being merely superficial, and involving no inflammatory or febrile disturbance, will occasion no anxiety to one who understands its cause; and I venture to repeat the caution given in a previous communication, that the surgeon must on no account be induced to explore the wound and pry into the source of the suppuration, *so long* as all is going on well otherwise; for such a course, by admitting

germs into the interior, may produce the most disastrous consequences in an otherwise promising case.

But although suppuration resulting from the stimulating influence of the antiseptic is no cause for anxiety, it is more convenient that it should be avoided; and this may often be done entirely by leaving the lower layers of the dressing permanently on the limb and changing only its superficial parts—a plan which, while it protects the wound against the introduction of mischievous particles, permits the foreign body in contact with the tissues to part with its antiseptic material and become an unstimulating crust, under which complete healing by scabbing may occur in wounds of a size hitherto regarded as inconsistent with this process in the human subject.

Upon these principles a really trustworthy treatment for compound fractures and other severe contused wounds has been established for the first time, so far as I am aware, in the history of surgery. In an hospital which receives an unusually large number of patients suffering from machinery accidents, and in wards which, from circumstances to which I need not here allude, were peculiarly unhealthy, my experience of compound fractures in the lower limb was formerly far indeed from satisfactory, even in the selected cases in which alone I attempted to save the limb. But since the antiseptic principle has guided us, not only have ordinary cases of this formidable injury been treated by my successive house-surgeons with unvarying success, but limbs such as I should once have condemned without hesitation have gone on to complete recovery without either local or constitutional disturbance: a statement which might be suspected of exaggeration were it not that it refers to proceedings in a public hospital, witnessed not only by students, but by gentlemen once my pupils, and now practitioners in Glasgow.—*Lancet*, *Nov.* 30, 1867, *p.* 668.

36.—ON THE ANTISEPTIC PRINCIPLE OF TREATMENT IN SURGERY.

By Prof. JAMES SYME, Surgeon to the Queen in Scotland.

The antiseptic system, in order to be employed with advantage, must be carefully studied and fully understood, theoretically as well as practically. The preparations employed by Mr. Lister, which have been adopted here, may be denominated carbolic oil, carbolic lotion, and carbolic paste. The composition of the first is carbolic acid and boiled linseed or other fixed oil, in the proportion of one to five; that of the second, carbolic acid and water, in the proportion of one to thirty; and that of the third, carbolic oil with whitening, in the proportions requisite for the consistence of soft putty.

Case 1.—*Parotid Tumour.*—Mrs. T., aged 32, from Aberdeen, recommended to my care by Dr. Kerr, one of the surgeons to the Royal Infirmary of that city, was admitted on the 20th of November last for the removal of a tumour occupying the whole of the parotid region. It was of ten years' duration, firm consistence, and nodulated form, hardly admitting of motion, but very distinctly defined. On the 26th I performed the operation without any difficulty or injury of the *portio dura*, twisted the vessels, sponged the wound with carbolic lotion, stitched the edges together by silver sutures, and applied a plaster of carbolic paste. On the third day the wound was found to be quite healed; on the fourth the stitches were taken out; and on the eighth the patient went home.

There are few wounds less apt to heal by the first intention than that which results from the excision of a parotid tumour. The depth and irregular form of the cavity, together with the oozing of blood from the glandular texture, which must be cut more or less during the operation, sufficiently account for the difficulty thus experienced; and the perfect union, without a drop of matter, which has just been related, may therefore be regarded as very satisfactory testimony in favour of the antiseptic treatment combined with torsion.

Case 2.—*Wound of the Knee-joint.*—On the morning of August 10th, J D., a farm servant, aged 53, while going out to cut grass in the neighbourhood of Carnwath, about thirty miles from Edinburgh, fell upon his scythe, which inflicted a severe wound on the right knee. He was put into a cart and conveyed to the nearest railway station, whence he travelled to Edinburgh, and was taken in a cab to the hospital. It then appeared that a wound, about four inches in length, extended obliquely across the knee, dividing the quadriceps extensor tendon, and affording free access to the joint. Carbolic oil was freely applied, by sponging the cavity; the cut edges, which had been widely separated, were brought together by sutures; a pledget of carbolic oil was placed over the wound; and a splint was applied to keep the limb straight. Not the slightest constitutional or local disturbance followed, the pulse never being beyond sixty-three, and on the fourth day, the wound being quite healed, the stitches were removed. On the 13th of September the patient left the hospital with the limb strong and flexible.

There are few more serious injuries than wounds of the knee-joint, even under the most favourable circumstances; and, when aggravated by their large extent, rudeness of the weapon that caused them, or roughness of the treatment to which they are exposed, they still more endanger the patient's limb and life. The very satisfactory result of a case in which all the adverse conditions were so conspicuously present, must therefore be considered a very remarkable, if not unprecedented, occurrence.

Case 3.—*Compound Fracture* —J. P., a boy, aged 5, was admitted on the 30th of September, with his leg so seriously injured by having been caught between the spokes of a cart-wheel, that it seemed at first sight to admit only of amputation. There was a compound fracture of both bones, and, in addition to this, a wound of the integuments and muscles almost completely surrounding the limb at a higher part. The bones were much displaced, and the soft parts severely bruised. Chloroform having been administered, the carbolic lotion was freely injected between the broken ends, and lint soaked in it was applied over the wound. The bones were then carefully adjusted, and retained in their proper position by lateral splints. There was not the slightest constitutional disturbance, or any discharge of matter from the cavity, to the orifice of which carbolic paste had been regularly applied, and the limb is now perfectly straight and strong.

Case 4.—*Compound Fracture.*—A. L., a woman, aged 25, was admitted on November 18th, soon after sustaining a compound fracture of the tibia and fibula by falling down a stair. The carbolic lotion was injected between the broken ends of the bones; the carbolic paste was placed over the wound; and lateral splints were applied after the fracture had been properly set. There was no constitutional disturbance, and no discharge of matter from the cavity. The limb is now perfectly strong and straight.

The most remarkable feature of the last two cases, is the total absence of fever and purulent discharge from the cavity; the wound, in the course of a few days, ceasing to have any communication with the broken bones, and becoming quite superficial, so as to require merely the ordinary water-dressing.

Case 5.—*Psoas Abscess.*—W. F., a man, aged 47, was admitted on October 28th, with a very large psoas abscess completely distending the triangular hollow of the thigh, and having the femoral vessels running over it. The patient stated that, about ten months previously, he had sustained a severe wrench of the spine in the lumbar region, and subsequently suffered so much pain at this part as to be confined for seven weeks to bed. Soon afterwards he observed a small swelling in his groin, which had gradually increased. On the 11th, under the protection of lint soaked with carbolic oil, I opened the abscess by a free incision, and discharged more than a pint of purulent matter. The carbolic paste, spread on tinfoil, was then applied over the wound. Not the slightest constitutional disturbance followed; and there was no discharge of matter from the cavity, there being merely a slight serous *oozing*, which gradually disappeared.

Not long ago, I should have declined to open this abscess, from feelin

very sure that the effect of doing so would be a profuse discharge of matter, rapid emaciation, hectic irritation, irritative fever, and death: but now, even when there is reason to believe that the matter proceeds from diseased bone, the confidence derived from experience has removed all such fears; and, caries being regarded as the effect instead of the cause of suppuration, patients are no longer left to their fate under circumstances that would formerly have been deemed entirely hopeless.

Case 6.—*Chronic Abscess of the Mamma.*—C. A., a married woman, aged 25, was admitted on December 2nd, for a deep-seated tumour of the mamma, which, she said, had existed for twelve months. Having ascertained that it was an abscess, I made a free incision, and discharged six or eight ounces of purulent matter. As it then appeared that a considerable arterial branch was bleeding, and could not conveniently be either tied or twisted, a needle was passed through the wall of the cavity so as to convey a thread round the vessel on each side of the wound, which, being tied, prevented any further hemorrhage. The wound having then been sponged with the carbolic lotion, was covered with the paste. On the 6th—that is, two days afterwards—the ligatures were removed, and neither then nor subsequently was there any discharge whatever, either serous or purulent. On the 9th, the patient was dismissed, and, at the end of a fortnight, she returned to show that the breast remained perfectly well. There are few surgeons who have not encountered great difficulty in treating abscesses of the breast, and who will not be able to appreciate the value of this extraordinary result.

Case 7.—*Acute Abscess.*—T. D., man, aged 59, was admitted on December 4th, with a very large abscess of the forearm extending from the elbow to the wrist, which, he said, had commenced about a fortnight before, and had been very painful. On the 5th, a free incision was made, under the protection of carbolic oil, and nearly a pint of purulent matter discharged; after which the paste was applied over the wound. On the 8th, there was a little serous oozing; on the 10th, it had nearly ceased; and on the 12th, the cavity was completely consolidated.

From such cases as those which have just been related, it is evident that there has taken place in surgical practice an improvement which promises to produce a great diminution of human suffering and danger. Some attempts have been made, anonymously and otherwise, to filch away from Mr. Lister the credit justly due to him for devising and establishing the antiseptic system, by representing the use of carbolic acid previously for other purposes as in anticipation of his treatment. But, although the agent was not new, the principles of its employment, the modes of its application, and the results of its effects, being so entirely original, I venture to hope that the members of my profession will no longer tacitly sanction such disingenuous and ungenerous conduct.—*British Medical Journal*, *Jan.* 4, 1868, *p.* 1.

37.—ON THE TREATMENT OF WOUNDS UPON THE ANTISEPTIC AND SUBCUTANEOUS PRINCIPLES.

By William Adams, Esq., Surgeon to the Great Northern and Royal Orthopædic Hospitals, &c.

[There is no novelty in the application of antiseptics to open wounds, and even the antiseptic which Professor Lister has selected, viz., carbolic acid, had been extensively employed by some few surgeons. There is, however, great novelty and originality in the method of applying the antiseptic adopted by Mr. Lister, as based upon the "germ theory" of M. Pasteur.]

The theory of M. Pasteur is simply this—that instead of regarding the septic or decomposing properties of the atmosphere as due to the oxygen and moisture, as generally believed; the decomposing properties are supposed to depend upon the universal diffusion through the atmosphere of minute organic molecules, which, by their development in the blood or serous exudation in wounds in which they are deposited, give rise to fermentative and putrefactive changes

According to this theory, then, minute organisms constantly floating in the air are the immediate cause of putrefaction, and putrefaction is regarded as the cause of suppuration. Mr. Lister observes:—"We know from the researches of Pasteur that the atmosphere does contain among its floating particles the spores of minute vegetations and infusoria, and in greater numbers where animal and vegetable life abound, as in crowded cities or under the shade of trees than where the opposite conditions prevail, as in unfrequented caves or on Alpine glaciers . . . and it is peculiarly in harmony with the extraordinary powers of self-diffusion and penetration exhibited by putrefaction that the chief agents in this process appear to be 'vibrios' endowed with the faculty of locomotion, so that they are able to make their way speedily along a layer of fluid such as serum or pus." And in a footnote to this Mr. Lister observes:—"I have seen vibrios, so minute as to be only just discernible with the highest powers of an excellent microscope, shoot across the field of view with a velocity that astonished me."

The best general account of Mr. Lister's views is given in a paper read by him at the annual meeting of the British Medical Association in Dublin on August 9, 1867, and reported in the British Medical Journal, September 21, 1867. In this paper he observes—"But when it had been shown by the researches of Pasteur that the septic property of the atmosphere depended not on the oxygen, or any gaseous constituent, but on minute organisms suspended in it, which owed their energy to their vitality, it occurred to me that decomposition in the injured part might be avoided *without excluding the air* by applying as a dressing some material capable of destroying the life of the floating particles." . . .

"The material which I have employed is carbolic or phenic acid—a volatile organic compound which appears to exercise a peculiarly destructive influence upon low forms of life, and hence is the most powerful antiseptic with which we are at present acquainted. . . .

"In conducting the treatment, the first object must be the destruction of any septic germs which may have been introduced into the wound, either at the moment of the accident or during the time which has since elapsed. This is done by introducing the acid of full strength into all accessible recesses of the wound by means of a piece of rag held in dressing forceps, and dipped in the liquid. This I did not venture to do in the earlier cases; but experience has shown that the compound which carbolic acid forms with the blood, and also any portions of tissue killed by its caustic action, including even parts of the bone, are disposed of by absorption or organisation, provided they are afterwards kept from decomposing."

Such, then, is the general outline of the "germ theory" of M. Pasteur, and the principles of the antiseptic treatment based upon it by Mr. Lister. Further details of the treatment are given by him, and a considerable number of favourable cases, including severe compound fractures, lacerated wounds, abscesses connected with carious bone, &c., are recorded by him in support of the treatment.

The facts placed on record by Mr. Lister are of course beyond question, and are, no doubt, a faithful record of cases treated upon the antiseptic principle advocated by him, and these results show undoubtedly a larger amount of success in the treatment of severe compound fractures and lacerated wounds than is generally met with. But still I do not feel inclined to accept Mr. Lister's explanation of these successful cases as based upon the "germ theory" and caustic antiseptics. I believe, on the other hand, that for these brilliant results Mr. Lister is mainly indebted to the adoption of the subcutaneous principle, the patients under the advantages of this system recovering even from the effects of caustic antiseptics in addition to the injuries sustained. The phenomena observed by Mr. Lister, and explained by him as depending upon the antiseptic principle, to my mind admit of being equally well explained upon the subcutaneous theory; and there can be no doubt that equally good results, as to the healing of compound fractures and large lacerated wounds, have been obtained by other surgeons who have adopted the subcutaneous principle, combined with weak and unirritating antiseptics.

One clinical fact which to my mind appears to be opposed to the "germ theory" is the frequency with which wounds, under apparently unfavourable circumstances, heal by the first intention, without inflammation or suppuration, which we should expect would more generally occur if suppuration depended upon organic molecules always floating in the air, and therefore always ready to enter wounds where they would germinate and promote putrefaction. We must be careful to avoid assuming too much credit for any artificial means employed, and also to avoid the common error of attaching too little importance to the *vis Medicatrix naturæ*, or the true reparative power dependent upon the healthy condition and constitutional powers of the patient.

The practical question is whether antiseptics are to be employed upon the "germ theory" of M. Pasteur, and applied of caustic strength to the deep recesses of recent lacerated and contused wounds of large size, such as in severe compound fractures, at the risk of causing further destruction of tissues and necrosis of bone; or whether they are to be used simply as antiseptics in a mild and unirritating form, with the sole purpose of preventing decomposition and putrefaction, such as would arise from exposure to the oxygen and moisture contained in the air, the healing of the wound being conducted essentially according to the subcutaneous principle—*i. e.*, the exclusion of the air by the mildest and least irritating means. My own opinion is strongly in favour of the latter view, and opposed to the application of caustic antiseptics.

This can only be determined when time has afforded other surgeons sufficient opportunities for deciding whether mild and unirritating antiseptics, applied to large wounds in combination with the subcutaneous principle of treatment, yield results equally as good as those reported by Mr. Lister as obtained by means of caustic antiseptic applications. I confidently anticipate they will do so, and observe that in a series of seven cases, including several severe accidents and injuries, treated by Mr. Syme upon the antiseptic principle, and recorded in the British Medical Journal of January 4 of the present year, no reference is made to the employment of caustic antiseptics. Mr. Syme observes:—"The preparations employed by Mr. Lister, which have been adopted here, may be denominated carbolic oil, carbolic lotion, and carbolic paste. The composition of the first is carbolic acid and boiled linseed or other fixed oil, in the proportion of one to five; that of the second, carbolic acid and water, in the proportion of one to thirty; and that of the third, carbolic acid with whitening, in the proportions requisite for the consistence of soft putty." In these cases the wounds were sponged with the weak carbolic lotion, which, in the cases of compound fracture, was also freely injected between the broken ends of the bones, and lint soaked in it applied over the wound, which was also covered with the carbolic paste. In some cases the carbolic oil was used instead of the lotion. The results were, in all cases, unexceptionally good, and I cannot but doubt that this practice will be generally followed, embodying, as it does, in a scientific manner the combination of the antiseptic with the subcutaneous principle.

To a certain extent it may be said that the practice is the same—viz., to exclude the air from the wound—whether this be done upon the "germ theory" or upon the theory of oxygen and moisture causing decomposition; but if the "germ theory" leads of necessity to the application of caustic antiseptics, then a wide difference in practice is at once established.

There can be no doubt that the greatest improvement in the treatment of wounds which has taken place in modern times, and that to which many lives are indebted for their existence, and by which many limbs have been saved, is the introduction of the antiseptic principle. Antiseptic lotions have now taken the place of water in the ordinary dressing, washing, and cleaning of wounds. We have, however, yet to determine by experience which shall prove to be the most efficient application, and amongst those generally in use are, the lotion of carbolic acid in the proportion of one part of the acid to thirty of water, or even weaker: Condy's fluid, which during the late wars proved so useful in army practice, and has long been in use in our general

hospitals: and lime-water, diluted with equal parts of water, which; in inflamed and suppurating wounds, I have been in the habit of using for many years.

Whilst fully admitting the importance of the antiseptic principle, with the exception only of the application of antiseptics strong enough to act as caustics, such as the strong carbolic acid applications sometimes used, I am desirous of submitting to the members of this society that there are other applications and methods of dressing wounds of known efficiency; some of these owe their efficiency more to the subcutaneous than the antiseptic principle, whilst others depend more upon their combination in equal proportions of the antiseptic with the subcutaneous principle.

As examples of the first class in which the subcutaneous principle is chiefly relied upon, I would mention, first, the method of dressing wounds simply with their own blood and dry lint. In this method the great object is to avoid exposure, or, at any rate, long exposure, of the cut surfaces to the air, and in this way, if no water be used, the wound being dressed only with dry lint, and a light pressure maintained by a bandage, incised wounds of considerable size, such as remain after the removal of moderate-sized tumours, amputation of fingers, &c., may be brought into a condition nearly as favourable to the reparative process as in true subcutaneous injuries and operations. All surgeons are familiar with the fact that large incised wounds, when treated in this way, as they sometimes are by patients in the absence of medical aid, frequently heal by the first intention, which would not have happened had the wounds been long exposed to the air, or if they had been washed with water.

Few practitioners have recognised the importance of excluding the air from open wounds, and still less have surgeons recognised the decomposing influence of water; but I may mention that for some time I have been in the habit of washing wounds with ether, instead of water, which I have found to contribute to the healing of many lacerated wounds, which appeared unlikely to heal by the first intention. Hippocrates, when speaking of the treatment of compound fracture, says:—"The wound is to be dressed in the summer with compresses soaked in wine; in the winter they should be dipped in oil, and the dressings will need renewal every day." I, for one, should certainly admit that, as a dressing for wounds, oil and wine are preferable to water.

Next to the simplest and most natural dressing of blood and dry lint is the method of dressing incised and lacerated wounds, even in compound fractures, with lint soaked in compound tincture of benzoin—the old Friar's balsam of well-deserved notoriety. This plan was based chiefly upon the subcutaneous principle, its object being by the exclusion of air to bring a compound fracture as nearly as possible into the same conditions as a simple fracture, but some of its advantages were undoubtedly due to the antiseptic properties of the tincture of benzoin. The late Mr. Bennion, of Oswestry, in Shropshire, adopted this plan, and obtained considerable reputation by his successful treatment of cases of compound fracture, accidents of frequent occurrence in this district. After setting the fracture, he covered the wound with a large a piece of lint saturated with compound tincture of benzoin. He never disturbed the first dressing unless urgent symptoms indicated the necessity of so doing, and if such symptoms did not appear, he would allow the first dressing to remain for a month.

[A simple method of treatment for wounds, and which owes its advantages chiefly to the subcutaneous, but in some degree to the antiseptic principle, is that of covering them with collodion. This remedy has been recently eclipsed by Dr. Richardson's new discovery of his colloid styptic.]

"The process of manufacture of the fluid," to quote from Dr. Richardson's lecture on the subject, "is tedious, but sufficiently easy. The object to be aimed at is to saturate ether entirely with tannin, and a colloidal substance, xyloidine, or gun cotton. In the first step of the process, the tannin, rendered as pure as it can be, is treated with absolute alcohol, and is made to digest in the alcohol for several days. Then the ether, also absolute, is added *until the* whole of the thick alcoholic mixture is rendered quite fluid. Next

the colloidal substance is put in until it ceases readily to dissolve. For the sake of its agreeable odour, a little tincture of benzoin is finally admixed. . . . .

"When the solution is brought into contact with an open surface of the body, the resultant phenomena are these: The heat of the body gradually volatilises the ether and the alcohol, and the tannin and cotton, as the ether leaves them, are thus left stranded on the surface in intimate combination. In proportion as the ether passes off, the blood or the secretion of the surface permeates the tannin and cotton; but tannin acts directly upon albumen, coagulating it, and transforming it into a kind of membrane, almost like leather. The cotton meanwhile unites the whole, gives substance to the mass, and adhesive quality. When all is solidified, the dressing becomes, in fact, a concrete, having a true organic hold or basis on the tissues; and as the tannin, if the solution be freely applied, is in excess, any new exudative matter or blood is for several hours taken up by it, and the annealing is made the more complete.

"Thus, by this dressing, the air is excluded from every possible point in every possible direction, not by a mere septum, but by the combination of the animal fluids with the remedy; and, because the air is excluded and fluid is absorbed, there is no decomposition—*i. e.*, no oxidation; and, because there is no oxidation, there is no irritation. . . .

"In cases of compound fracture, after the parts have been brought into apposition as far as is possible and fixed in the necessary position, the fluid should be poured slowly into the open cavity so as to fill it. Then the parts, externally, should be covered with a layer of cotton-wool saturated with the solution.

"On open cancer, and on suppurating or decomposing surfaces, the solution may be freely applied with the brush, and, afterwards, the parts may be covered with cotton-wool saturated with the fluid.

"In no case need there be any fear that irritation will follow the application of the solution. On the contrary, the action of it is so purely negative that it might be considered a sedative. It is not such in the technical sense of the term, but it so effectually covers the wounded and susceptible surfaces as to maintain what is virtually a sedative influence."

I have now, for more than a year, used Dr. Richardson's colloid styptic in a large number of cases of incised and lacerated wounds, some of formidable dimensions, with complete success in a large proportion of cases. In two-thirds of the cases so treated, I can with confidence assert that union by the first intention has been obtained; or that the reparative process has proceeded either without suppuration, even in bad cases, or with the suppurative process reduced to a very insignificant amount, and in no instance have I seen any injurious effects. In about one-third or one-fourth of the cases, the styptic had to be abandoned in consequence of suppuration occurring, and antiseptic lotion relied upon.

The largest operation in which I have applied it—or rather I should say that in this case Dr. Richardson himself was kind enough to apply it—was amputation of the foot, by Chopart's operation, in a young gentleman, aged 19 years, for extreme deformity of the foot, with anchylosis, after suppurative inflammation which had attacked the left foot, and also the right knee-joint, in consequence of purulent absorption; and I should mention that he was particularly prone to erysipelas.

In alluding to this case Dr. Richardson observes: "The operation was performed on Wednesday, February 13 of this year (1867), and as the cuboid bone and os calcis were anchylosed, the saw had to be freely used. Several vessels had to be tied, and the ligatures were left in the usual way suspended from the wound. When the lips of the wound had been brought together by wire sutures, I coated the wound freely with the fluid, and the bandage was applied. Three days later there was no fœtor, no discharge, and no general symptoms, but as Mr. Adams was anxious to see the condition of the wound I undressed it. To our delight we found it healed throughout, but, unfortunately, from the bandage adhering to one of the long ligatures, I, in removing it there, tore open the newly healed wound for the space of a

quarter inch. At this broken spot about a teaspoonful of purulent matter formed two days later; but this little break was very quickly reunited, and on the sixteenth day after the operation the patient was able to return to the country with complete healing by the first intention, and without having suffered from one symptom of a constitutional kind."

Another case was that of compound dislocation with fracture of the second phalanx of a finger, produced by a cricket ball at Lord's ground. The patient, Captain McN., came to me immediately after the accident, and after reducing the dislocation and applying three metal sutures I applied the colloid styptic and bandaged the finger to a straight splint. On the fourth day, finding no indication of inflammation, I allowed the dressing to remain, and did not remove it till six weeks after the accident, when I found the wound not only well cicatrised, but the callus was less than I expected, and some motion existed at the joint, which subsequently became quite free.

Another case was one of enchondromatous tumour, growing from the interior of the first phalanx of the third finger of the left hand of a young gentleman, Master K., upon whom I operated on April 3, 1867. My colleague, Mr. Gay, assisted me in the operation, and held the extensor tendon on one side, whilst I gouged out the growth from the interior of the bone, extending as closely as possible to the articulations at either extremity. I applied the colloid styptic to the interior of the bone, and, after applying metal sutures, painted the wound over with the styptic, and applied a little cotton-wool saturated with it as a dressing. No suppuration occurred, the boy recovering motion at both the articulations. Nothing could be more satisfactory.

Under my direction a large number of incised and lacerated wounds, including many of considerable size and many scalp wounds, have been treated by the application of styptic colloid at the Great Northern Hospital, by Mr. P. Hopgood, the House-Surgeon. The wounds were generally washed with ether instead of water, or with ether after water had been applied. I have before me the details of thirty-three of these cases, supplied by Mr. Hopgood, but will only read you in a few words the conclusions at which he has arrived. Mr. Hopgood observes:—"Out of the thirty-three cases of recent wounds treated with the styptic colloid, twenty healed by the first intention, requiring no further application than the first dressing; the remaining thirteen required the styptic to be removed, but still in nearly all some amount of adhesion had taken place, although suppuration had taken place in all, requiring the removal of the dressings. The styptic colloid, in my opinion, has these advantages:—It is certainly a great improvement on the 'pad and bandage' usually used in cases particularly of scalp wounds, and without doubt favours greatly, although not certainly, adhesion by the first intention, adjusting by its contractile powers (upon drying) the surfaces of wounds together, and effectually preventing the access of air. It appears to me, from the wounds I have treated with it, to be particularly applicable to clean-cut wounds where the surfaces can be brought accurately together; but the styptic is certainly often successful in obtaining union without suppuration in lacerated and often contused wounds, checking at all times, and often preventing suppuration in a wound which otherwise treated would be nearly sure to suppurate."

The testimony I have now given in favour of Dr. Richardson's colloid styptic as a material for the dressing of wounds, calculated to promote healing by the first intention or without suppuration, superseding the use of plaster, and embodying in the most scientific manner the antiseptic and subcutaneous principles, will, I hope, induce other surgeons to give this method a more extended trial. It is, I believe, only by such a combination of the antiseptic and subcutaneous principles that we can hope to prevent the more serious result of injuries with open wounds, such as exhaustive suppuration, diffuse inflammation, pyæmia, and death.

The methods of treatment and local applications to which I have referred, have mostly been considered in relation to their power of promoting union by *the first intention* in wounds of moderate size, or a reparative process without *suppuration* in wounds of greater magnitude. We have to deal, however,

with wounds in which suppuration is already established, and with open ulcers of a more or less chronic character. To these antiseptic lotions are especially applicable. But time will not permit of my entering upon this portion of our subject, which has of late derived additional interest from the study of the antiseptic principle and antiseptic remedies.

I would merely mention that amongst the most valuable antiseptic lotions are—carbolic lotion made in the proportion of one part of carbolic acid to thirty of water, or probably one part in a hundred would be found strong enough for ordinary purposes; Condy's fluid, weak iodine solutions, and weak solutions of chloride of zinc, and also a lotion which has hitherto received much less attention than it deserves—viz., lime water, as well as solutions of potash and soda. I may add that the alkaline solutions have for many years been favourites of mine, and that it has been my practice to apply lime-water constantly as a lotion to the surfaces of all ulcers and suppurating wounds; it cleanses the sore very quickly by chemically destroying all the pus cells and the secretion on the surface, and also diminishes the inflammatory congestion by its direct solvent action upon the fibrine of the blood in the distended capillary vessels, so that by endosmosis the condition of stasis of the red corpuscles in the capillary vessels is removed. I always use it of full strength in indolent and sloughy ulcers, in which it acts most efficiently in deodorising the ulcer, and as a slight stimulant; but in irritable sores I use it diluted with equal parts of water.

Lime water, in combination with oil, has long been a favourite remedy in cases of burns and scalds, and in combination with calomel as black wash, and with bichloride of mercury as yellow wash, has long been considered beneficial in many cases of specific ulceration; but, as I have generally considered the benefit to result from the lime-water rather than the mercury, I have used it simply as an alkaline antiseptic lotion in this class of cases.—*Medical Times and Gazette, March 7 and* 14, 1868, *pp.* 256, 282.

38.—ON THE USE OF CARBOLIC ACID IN THE TREATMENT OF ABSCESSES.

By Dr. James Martin.

I have lately found much benefit from the use of carbolic acid in the treatment of wounds and abscesses. One case in particular forces itself on my mind as exemplifying its value.

N. I., aged 18, a stout-looking young man, but of scrofulous diathesis, presented himself to me last February with an abscess, situate to the left of the junction of the first and second bones of the sternum. It was about the size of half a large orange, and had evidently formed under the aponeurosis of the pectoral.

I made an opening at a depending point with potassa c. calce, having, for that purpose, to make a very deep eschar. The abscess discharged freely pus of a thin watery character.

I treated him locally with poultices, stimulant applications, and tents, and generally with iron, quinine, and cod-liver oil. He improved greatly in appearance, but the abscess remained unhealthy. I could introduce the probe upwards and outwards fully 5 inches, and the integuments being sound and thick, I thought it rather a formidable operation to make such a wound as laying it open to heal from the bottom would involve.

Before doing so, I happily determined to try the effect of carbolic acid. So dipping a piece of cotton, on the end of a probe, in a solution of 5 grs. to 100 of water, I mopped out all the interior of the abscess. The improvement was rapid and decided, the character of the pus improved, and day by day the cavity contracted, until now it is at the point of being finally closed, the parts being well knit, and it being impossible to introduce the probe for the one-eighth of an inch.—*Medical Press and Circular, Dec.* 11, 1867, *p.* 530.

39.—ON THE REMOVAL OF TUMOURS BY ELECTROLYSIS.

By Dr. JULIUS ALTHAUS, Physician to the London Infirmary for Epilepsy and Paralysis.

The object of this paper is briefly to state the results hitherto obtained by the method of starving tumours by electrolysis. Let me premise that the electrolytic treatment acts in a threefold manner, viz., first, through mechanical disintegration of the tissues by the nascent hydrogen; second, through chemical destruction by free alkalies—potash, soda, and lime, which are evolved at the negative pole of the battery; and lastly, through a modification of nutrition, by the dynamic effects of the continuous galvanic current on the vasomotor nerves of the parts brought under its influence.

The entire number of cases I have up to the present time treated by electrolysis, amounts to fifty-eight, amongst which there were forty-seven non-malignant, and eleven of the malignant kind.

Eleven cases of nævus have been under my care. Of these, seven were cured; one patient was only seen once, and not heard of again; one discontinued the treatment before any appreciable result was obtained; two are still under treatment, and so much improved as to give the best hopes of eventual complete success. In none of these eleven cases was there, at any time during the progress of the treatment, any bad symptom whatsoever, and in most cases not a drop of blood was lost. In one case, the growth was so extensive that both Mr. Paget and Sir William Fergusson, who were consulted previously to the patient coming under my care, refused to interfere with the tumour. In another case, where the nævus was seated on the lower eyelid, Mr. Dixon had been previously consulted, and he advised to leave the growth alone, on account of its position, although the nævus was a source of great annoyance to the patient. Neither an awkward position, nor extreme size of a nævus, can ever be an impediment to the electrolytic treatment, which I believe to be suitable for all cases of nævus, indiscriminately.

Of bronchocele, I have had eight cases under treatment. Of these, two were cured, one was improved, five are still under treatment and progressing favourably. In several of these cases, Mr. Prescott Hewett, Mr. Paget, and Sir William Fergusson, had been previously consulted, and had pronounced any of the ordinary operations to be inadmissible. Most of these cases were of an enormous size, and on that account required a long continuance of the treatment; but I believe my experience justifies me in saying that any case of bronchocele, however large, may, in course of time, be cured by electrolysis.

Of sebaceous tumours of the scalp and face, I have treated fourteen cases. Of these, thirteen were cured; the fourteenth patient was obliged to leave town before the end of the treatment, but would from the change already caused in the tumour, have been cured by one or two more applications. In sebaceous tumours the treatment is short, as they never attain to any considerable size.

Of that hypertrophy of the skin which Mr. Wilson calls *ecphyma mollusciforme*, I have treated five cases. Two of these were seated on the upper eyelid, two on the cheek, and one on the back of the neck. Four of these cases were cured; the fifth was not heard of again after two applications, but from the change already caused in the tumour, I believe that it must have disappeared long ere this.

Of kelis, I have had one case under my care; but, as I saw the patient only once, I am unable to say whether any result has been obtained.

Of lipoma, three cases have come under my treatment. One of these is nearly cured; the other two patients discontinued the treatment before any appreciable result was obtained. One of these latter patients, a highly nervous gentleman, complained of pain and swelling of the tumour, after the application, and wrote to say that some matter was formed at one of the punctures. This is the only case where some limited suppuration seems to have taken place in consequence of the galvanic applications.

I have cured a cyst of the lower lip, in a gentleman, aged 53, and a ganglion on the wrist of a lady, aged 30, by the same method.

Of glandular tumours, I have treated three cases, all of which were much improved, but none continued the treatment until the entire removal of the enlargement. Thus of forty-seven cases of non-malignant tumours, twenty-nine were cured, ten improved, and in seven cases no result was obtained, or the result unknown.

In malignant disease, of which I have treated eleven cases, the results of the process are, as might have been expected, far less favourable. Wherever the cancer is one of considerable size, and growing rapidly, the pain may be relieved, and the further growth of the tumour be considerably checked by the electrolytic treatment; but I have not hitherto obtained a cure of such patients. In two cases of scirrhus of the breast, where the constitution was apparently not suffering, and where the tumour was only of the size, in one of a filbert, and in the other of a nutmeg, the swelling has entirely disappeared; although, of course, I am not prepared to say that a cure has been effected. For the healing of an open cancer, the treatment seems to answer very well; I must say, however, that in no case of malignant disease have I ever trusted to electrolysis alone, but have prescribed at the same time powerful remedies to be taken internally, for modifying the constitution of the patient, such as bromide of potassium, belladonna, and quinine. Perhaps in course of time a more continuous application of the electrolytic process may be discovered, by means of which we may be able to starve out even malignant tumours with more success than has hitherto been obtained.

The only part of the electrolytic operation which is at all unpleasant to the patient, is the introduction of the needles through the skin. I therefore, always use the ether spray, unless the patient himself objects to it. To some patients the smarting of the ether is more unpleasant than the prick of the needles, but in the majority of cases, the ether spray is preferred. The galvanism itself is almost, or even entirely painless, if judiciously applied. The length of the application should vary according to the nature of the case. For small tumours two or three minutes are frequently sufficient; for large ones fifteen to thirty minutes are preferable. In the latter, the best plan is to apply the current every day for some time; but I have also obtained satisfactory results where it was only done twice or three times a week. The instrument which I have found most useful for large tumours is the *serres-fines* conductor, made for me by Messrs. Weiss. With this, from 2 to 6 needles may be introduced singly into the depth of the tumour; their heads are then grasped by the *serres-fines*, which are connected with the negative pole of the battery by means of ramified conducting wires.

The safety of the treatment, which is its chief characteristic feature, seems to me in a great measure due to the fact of its belonging to the domain of subcutaneous surgery; for although there is a slight action on the skin, yet by far the greatest effect is produced in the substance of the tumour, owing to the superior electric conductivity of the soft and warm internal structures. Pyæmia and other much-feared consequences of surgical operations, therefore, do not follow the electrolytic applications. Add to this the painless nature of the proceeding, the absence of shock and of hemorrhage, the fact that, during the whole progress of the treatment, the patient may follow his avocations just as usual; and I hope it will be acknowledged that this method bids fair to effect a substantial improvement in surgical therapeutics.—*British Medical Journal*, *Dec.* 7, 1867, *p.* 521.

40.—ON THE TREATMENT OF TUMOURS BY ELECTRICITY.

By Dr. Maurice H. Collis, Surgeon to the Meath Hospital and Co. Dublin Infirmary.

Early in the present century, when galvanism was still new, it was applied to many things, with a sanguine hope of its proving a valuable curative agent. Partly from ignorance of its nature, partly from want of experience

as to its varying powers, and partly from impatience of the consequent failures, it was soon cast aside, and has been ever since, as I think, too much neglected. Its beneficial action in cases of paralysis and atrophy ought to have taught us that we possess in it an agent of great power; and the vast advances in our knowledge of its various forms and modes of action as a chemical agent ought to have led us to a more extended application of its powers as a curative agent. I must acknowledge having been led to study it in this latter respect by reading Dr. Althaus's paper on its use in the chemical decomposition of tumours. I tested his experiments, and found that there was literally no limit to the decomposing power of the continuous current derived from such batteries as Daniell's. Tumours and healthy tissue could be decomposed at a great pace—not without pain, certainly, but to an extent that might be desired, provided a battery of sufficient strength were employed. This mode of applying galvanism to tumours seemed to me very closely to resemble, as to its results, the action of other chemical decomposers, such as the strong caustics. Great immediate destruction of the morbid growth could, no doubt, be obtained, but a complete removal could not be promised, much less an immunity from relapse, or from extension of the growth by its periphery. Dr. Althaus's milder battery seems well adapted for some simple tumours, and I have seen good effects from it.

It occurred to me, however, that something could be done towards procuring the absorption of tumours, or perhaps towards checking their growth, by using galvanism simply as a stimulant. A slight occasional current, by its stimulus, will develope a wasted muscle; while the continuous use of the same current will, by over-stimulation, cause it again to waste. I therefore sought to apply this principle to tumours of various kinds, and to cause them to waste by keeping up a constant flow of electricity through them.

For reasons which I need not enter on, I tried various forms of battery; and finally I returned to the simple voltaic pile, composed of a dozen or more couples of zinc and copper an inch and a half square, or of small cylinders or plates of wood covered with felt, and wrapped round with zinc and copper wire. These simple batteries were excited by salt and water, or by sulphuric acid in the proportion of one to twenty parts of water.

The mode of application was as follows:—The tumour was covered with a plate of zinc, perforated zinc, silver foil, copper, or copper plated with silver. The positive pole was connected with this plate; the negative pole with a plate of copper, which was brought into contact with the skin of the back or other convenient part. The battery, tied up in gutta-percha paper, or oiled silk, lay on a table, or was tied round the waist of the patient.

The results were as follows:—

1. Complete removal of secondary cancerous deposit from a gland in the neck.

2. Rapid absorption of inflammatory deposit over and round an immense mass of strumous gland.

3. Slow removal of said gland.

4. Immediate check to the growth of a tumour composed of an aggregation of strumous glands.

5. Considerable cutaneous and subcutaneous inflammation in the same case, with softening and breaking up of the diseased glands.

6. Decided absorption of inflammatory effusions round primary scirrhus of mammary region.

7. Diminution and softening of a very firm fibrous growth attached to the periosteum.

All these results, except No. 3, were obtained in from three to six days; and all the cases had previously been submitted to a variety of treatment without benefit. Special results of a peculiar nature were observed as follows:

8. The zinc plate on the skin showed the usual tendency to decompose the skin and produce ulceration in a few hours.

9. Perforated zinc, employed to obviate this, produced phlyctenæ and pustules in the interspaces not covered by it.

10. Silver, whether as foil or plated on copper, produced redness of the skin, but with less rapid tendency to ulceration.

11. Copper plates were comparatively slow to act on skin in connexion with the negative pole; but the copper connected with the positive pole on one occasion blistered the skin severely when a very strong battery was used.

For the rest, the batteries of wire coiled on wood are much the lighter and more convenient in proportion to their strength. They preserve their activity sufficiently, and do not wear out so soon as the voltaic pile of zinc and copper plates. They have the further advantage, that as many or as few as are desired can be used without delay in arrangement. I believe that a very strong current, whether as to quantity or tension, is not required. A small quantity is sufficient to excite the nerves of the blood-vessels; and there should be just sufficient tension to ensure that the current pass through the part to be acted on. and not merely round by the skin. I believe that, without claiming any miraculous power for electricity thus applied, we have in it an agent of considerable energy, and capable of yielding results of sufficient value to warrant us in resorting to it more frequently than hitherto.—*British Med. Journal, Dec.* 7, 1867, *p.* 521.

41.—ON TAPPING WITH THE AID OF SIPHON-POWER.

By Dr. W. Roberts, Physician to the Manchester Infirmary.

The aid which may be obtained in the operation of tapping by the use of the soliciting force of a column of water in an elastic tube was first suggested to me in a case of ascites which was under the joint care of Dr. Eason Wilkinson, Mr. Hall, of Salford, and myself, in November, 1866. The disease was cirrhosis of the liver, and the necessity for tapping soon became apparent. It was agreed to try the method indicated; and Mr. Hall caused to be made a fine trocar and canula furnished with a short piece of metal pipe projecting beyond the hilt of the canula. The operation was performed in the following manner: The canula, armed with the trocar, was introduced in the usual way through the linea alba. The trocar was then withdrawn, and a piece of elastic tubing four feet long, previously filled with water, and tied in a knot at one end, was slipped over the projecting piece. The knotted end was then placed in a vessel on the floor, about thirty inches below the level of the puncture, and untied. The fluid immediately commenced to run, and continued to run for about an hour and a half, in which period several pints of ascitic fluid drained away. After this, Mr. Hall improved the instrument by adapting a screw to the projecting piece, and fixing a similar piece, also furnished with a screw, to one end of the elastic tube. The elastic tube could thus be immediately screwed on to the end of the canula on the withdrawal of the trocar. and the spilling of the fluid during the fixing of the elastic tube was almost entirely avoided.

This patient was subsequently tapped several times by the same method, and, so far as the mechanical arrangement was concerned, with cleanliness, convenience, and efficiency.

The principle of the method is easily understood. The column of liquid in the elastic tube (drawn into it beforehand by suction), acting in obedience to the law of gravity, operates as a soliciting force, and as it seeks the lower level of the vessel on the floor, it draws after it the contents of the cavity which is being tapped. When the patient lies horizontally in bed, the fluid first ascends in the canula, and then descends along the elastic tube, which thus acts after the manner of a siphon.—*Medical Times and Gazette, Jan.* 11, 1868, *p.* 34.

42.—ON THE TREATMENT OF HARELIP.

By THOMAS SMITH, Esq., Assistant-Surgeon to St. Bartholomew's and the Children's Hospitals.

At what age should an operation be undertaken for the cure of harelip?

The time of selection—*i.e.*, the best time, when one has the opportunity of choice—is between the third and sixth month of infant life. In recommending this period as the most favourable for the operation, I would not restrict it to any particular epoch, since many children are as robust and strong at the age of two months as others are at six. Rather consider that any age before the commencement of dentition is suitable, provided that the child is past the period of earliest infancy, is at least in fair health, and has shown by its progress since birth, and general condition, that there is a good prospect of its continued existence.

In the event of a child coming under treatment during the period of dentition, some interval between the eruption of the teeth must be selected for the performance of the operation.

It may sometimes happen that there is little choice of time for the operation—namely, in cases of newly-born infants who are unable to take the breast in consequence of the deformity, and where the performance of the operation may enable them so to do. Even in such a case, however, I would not undertake the operation unless requested by the mother. I would rather recommend that the child should be spoon-fed, either with its mother's milk or artificial food, until it had passed the tender age of earliest infancy.

I give you this advice for the following reasons:—

1st. The operation can be done more perfectly and artistically on a young child than on a newly-born infant, the parts being larger, more fleshy, and more easily handled.

2nd. A young child has, *cæteris paribus*, far more constitutional vigour and power of resistance to and recovery from injury than a newly-born infant; the younger child therefore is more likely to die under the operation.

3rd. Many infants with harelip die from causes unconnected with this deformity during the first few weeks of life, thus dispensing with the necessity of an operation.

M. Guersant, the present surgeon at the Children's Hospital, Paris, is against an early operation. Giraldès (his predecessor at the same hospital), who has had great experience in this deformity, recommends that the operation should be done in the first days of infant life, and in this opinion I am bound to tell you many coincide. The grounds of Giraldès' opinion are the following:—

1. The younger the child is the more quickly union takes place.

2. That in the case of cleft palate the union of the lip tends to bring the separated bones together, and to diminish the fissure in the hard palate.

3. That if the operation be postponed, infants get into a habit of thrusting their tongues into the hiatus between the bones, until at last the tongue acquires "*le droit de domicile*" in the palatine fissure. In a paper where this opinion is expressed, nine cases of Giraldès are quoted, and of these, three were under the age of ten days when subjected to the operation; two of them died.

On this question Sir William Fergusson, than whom no one has a larger experience, thus expresses himself: "The earlier the operation is performed the better; assuredly before teething. I decidedly prefer before the end of the first month in a healthy child, though the operation may be performed any time earlier to within a few hours of birth."

The same author, however, recommends that in the case of weakly infants, where the gap in the lip is large, and especially if the palate be cleft, the operation should be postponed for some months, until the child has acquired strength. I have myself operated on but one child under a month old. In this case union at first took place; symptoms of syphilis, however, supervened, the line of union ulcerated, and the child died.

I have seen very many infants with harelip die before the third month, without operation, of causes not connected with their deformity.

Mr. Cooper Forster recommends the performance of an operation "at the earliest possible period, remembering, however, that loss of blood in very early infancy may be attended with the worst consequences. I should," says Mr. Forster, "have no hesitation in operating immediately after birth, so as to avoid the shock to the mother occasioned by the sight of the child. The operation has been performed successfully within seven hours after birth. But the possibility of a fatal issue must not be forgotten."

Mr. Forster prefers to encounter the greater risk of the operation at this period, on the ground that the danger of the operation is less than that of imperfect nutrition from the deformity; and in support of this he relates two cases where death occurred, as he believes from this cause—the one at six weeks, the other at fifteen days after birth. But it is worth noticing that the death in the first case ensued from convulsions and diarrhœa the day after the operation for harelip had been performed; while in the second case the same operation had been done four days before death. Thus in both instances the operation may have had something to do with the fatal result.

The inability to suck, as far as my experience goes, is less frequently dependent on simple harelip than on fissure of the hard palate—an irremediable deformity at this tender age. In cases of harelip with cleft palate the early union of the lip certainly tends to bring the bones together; even in those cases, however, the loss of a month or two can make no great difference in this particular, especially if the mother be directed frequently to press the bones together from the first, or if the truss recommended by Sir William Fergusson be worn by the child.

There are certain imperfections in the result of the operation for harelip to which attention should be directed, since they are very likely to occur, and, when present, they seriously interfere with the completeness of the cure. These are: notchings of the free margin of the lip; a dissimilarity in the horizontal plane of the opposite edges of the wound; and the occurrence of suture scars. It is to the last only that we will now allude; since, to avoid repetition, it will be convenient to consider, once for all, the different kinds of suture that are employed in the treatment of harelip.

Sutures.—Of the various kinds of suture employed for the cure of harelip, the most time-honoured and the most generally used is the twisted suture; so that in speaking of sutures the term "twisted" and "harelip" are used as synonymous. The advantages of this kind of suture in the treatment of harelip are many and obvious: it secures firm, close, and accurate apposition; by its rigidity it keeps the exactly opposite and corresponding parts of the fissure in contact; and by the figure-of-8 thread twisted across the wound, the edges of the latter are pressed down and kept exactly on the same level. On the other hand, the following objections may be raised against its employment in certain cases—namely, that it cannot be allowed to remain in position long enough to secure the firm union of the wound without running some risk of leaving on the lip disfiguring and permanent traces of its presence. These traces may be either a small, depressed, livid spot at the points of entrance and exit of each pin, or, together with this, a transverse bar of cicatrix across the line of the wound. I by no means wish to imply that a permanent cicatrix of the kind mentioned above is a constant result of the operation for harelip when twisted sutures are employed; but that such an effect may occur, and is often not altogether avoidable, and that this results from the nature of the suture.

In applying this kind of suture when the edges of the wound are drawn together by tightening the thread beneath the needle-ends, the needle is raised above the level at which it was inserted, and pushed upwards towards the surface of the skin; while the twisting of the thread beneath the needle-ends and across the line of the wound completes the circle of constriction. Thus all the soft parts included in the suture are compressed between a rigid bar of steel below, and a more or less tight and unyielding thread above—a condition which often *sorely* tries their vitality and powers of endurance.

This compression does not exist to any serious extent where the edges of the wound approximate easily and without tension, as in wounds of the wal of the abdomen or the face, and indeed in some cases of harelip where ther is abundance of tissue; here the merits of the suture are very apparent. Bu it is otherwise in the treatment of bad cases of harelip in young children where the tension of the soft parts is often considerable, and where these bea pressure badly.

The stretching of the edges of the wound in serious cases of this deformity is more considerable than in wounds generally, both on account of the width of the wound left after the operation, and because the line of the wound falls across the crown of the alveolar arch, in these cases often abnormally prominent from the projection of the intermaxillary bone.

It may be stated as generally true that where there is much tension about the edges of the wound one runs a risk of causing a permanent cicatrix, if harelip pins remain longer in the lip than forty-eight hours after the operation—too short a time to admit of the wound being firmly united.

With a view to meet these objections in cases where otherwise the twisted suture would be used, I have frequently dispensed with the thread altogether, fastening the ordinary glass-headed harelip pin by clamping a perforated shot passed over the point so as to keep the edges of the wound in contact. On many occasions I have used a suture formed of a single strand of fishing-gut, passed deeply through the wound and clamped down on either side to the skin with a perforated shot. This has given me most excellent results, having never failed in my hands to secure union. It may be left *in situ* many days without cutting into the tissues. If there is a very scanty supply of soft parts, and much tension on the suture, it is liable to cause some eversion of the cutaneous edge of the wound; this may be remedied by tying the free ends of the gut over the wound after clamping it down on the sides. The knot across the wound may be cut on the second or third day afterwards, and the shotted portion may be allowed to remain as long as desirable.

Mr. Wood's button suture is much on the same principle, though silk is used instead of gut.

The same may be said of Mr. Pollock's metallic studs or buttons, which answer the same purpose as the shot in my suture, though perhaps more efficiently. Mr. Pollock passes a silver wire through the edges of the fissure, and fastens it on the sides by drawing it down into a slit in the stud made to receive it.

The *interrupted* suture is in every way well suited for harelip. It is used and recommended by both M. Giraldès and M. Guersant, the former of these gentlemen often allowing the threads to remain in for a fortnight or three weeks. In this kind of suture silk-thread, fishing-gut, horsehair, or silver wire may be used. Of these, the last three are to be preferred, as exciting less irritation than the first.

The *continuous* suture is admirably adapted for the treatment of harelip in every respect but one—that is, that it cannot be removed part at a time; and in this respect alone it is infefior to the interrupted suture, though for securing close and accurate contact it is to be preferred.

Whatever kind of suture be employed, it should pierce the skin a third of an inch or more from the margin of the wound, and should be passed deeply, even down to the mucous membrane, though it is well to avoid entering the mouth.—*Lancet, Dec.* 21. *p.* 762.

43.—TREATMENT OF HARELIP.

By Allen Duke, Esq., Dover.

Having read with considerable interest Mr. T. Smith's clinical paper on the treatment of harelip, and having considered every kind of suture recommended by him, and others to whom he alluded, with a view to prevent those ugly depressed scars and blemishes that almost invariably follow the intro-

duction of pins and sutures of every kind *through* the skin. I cannot forbear drawing his attention to, and urging him to try, a method I have repeatedly adopted (several successful cases of which I published in the Lancet of March 20th, 1858), as being, I think, superior to every proceeding that has been recommended. It consists in passing two or more curved needles, armed with silk sutures, from the raw edges immediately *under* the skin, carrying them through the whole thickness of the lip, and tying them *inside*. The principal advantage of this plan is that they may be allowed to remain any length of time, till the parts are firmly united.—*Lancet, Jan.* 4, 1868, *p.* 14.

ORGANS OF CIRCULATION.

44.—ON THE DIAGNOSTIC INDICATIONS AFFORDED BY BLOOD FROM THE URETHRA, RECTUM, OR STOMACH.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.

We will suppose a patient to come to you, suffering from hemorrhage from the urethra, his other symptoms affording no conclusive evidence as to the source of the blood: have you, then any means of finding whence it comes? I will try to answer this question for you. Firstly, urethral hemorrhage may be diagnosed by the fact that the blood precedes the urine. Should you be in doubt on this point, ask the patient to make water in your presence; if you see the blood escape before the urine, its origin must necessarily be in front of the bladder. This is so simple that I am afraid you will think I am reflecting on your rational powers in urging it; but, in spite of its simplicity, probably fifty out of a hundred medical men would not think of adopting the expedient, or would not choose to take the little necessary trouble. You next employ catheterism, and you find that the urine drawn direct from the bladder is clear (as is usual with urethral hemorrhage), or, very rarely, perhaps it may contain blood. Now comes the query—does urethral blood ever travel backwards into the bladder? I think not, or scarcely ever; since it seems so much more easy for it to run forwards to the meatus, in which direction there is nothing to oppose its escape, than for it to retrograde to the bladder, at the entrance to which is seated the firmly contracted sphincter muscle. You may start, then, with this fact—the blood from the urethra never enters the bladder; consequently blood in the bladder signifies hemorrhage from the kidney or bladder itself. Let us take some illustrations of urethral bleeding. First come those instances where blows upon the perineum are the cause. A man is riding his horse, and strikes his perineum suddenly on the pommel of the saddle, or another man falls unguardedly astride a bar of any description. Each will probably have a little pain at the time, and, not unlikely, may vomit, as if he had struck his testicle. A little blood, we will say, comes from the penis before the patient micturates; if such an observation be accurately made, it will at once settle the diagnosis. And as regards your prognosis, it is of the utmost importance to determine if the blood does come from the urethra or from the bladder. But it may come from any part of the urethra; and to determine the site with exactitude you press with your finger on the under surface of the penis from the rectum forwards, until you arrive at a spot where tenderness exists. Take another instance. A man jumps or falls from a great height, alights on his feet, and then falls. He at once experiences pain at the pubes, and on his going to pass water he notices blood upon his shirt, or when he has finished his micturation he sees that the chamber-vessel contains bloody urine. Now, what has here happened? The man has probably fractured the pubic portion of his pelvis; the fragments of bone have injured his urethra, and the blood is streaming into the canal at the damaged spot. But, if you have any doubt, pass a catheter, and draw off the urine. If it is clear, you may at once locate the site of injury in the urethra; and I will even go so far as to observe that with such a history the urethral bleeding is almost pathognomonic of fracture

of the pubic bone. Or, one boy (A) playing with another (B) pulls him up by the collar at the nape of his neck and by his breeches behind, so as to press forcibly upon his perineum: when B comes to make water he finds some blood on his shirt, and more is running from his urethra. I am not now drawing upon my imagination, for such a case occurred in my own practice. Subsequently, the boy had abscess in the perineum, which broke and led to urinary fistula. The pressure beneath the pubes had injured the urethra at that spot. Had I seen the boy at first, I should have thought it right to pass a catheter before he made water. Having found the urine clear, and having therefore concluded that there was no hemorrhage from the bladder, I should have ordered the catheter to be worn continuously until I deemed the urethral injury repaired, in order, during this period, to transfer the urine from the bladder to the exterior without allowing it to come in contact with the hole in the urethral wall. Such is the treatment you must adopt in a like case, if you would avoid extravasation of urine and its consequences. However, this boy subsequently wore a catheter for some time, during which the urinary fistula healed up, and he regained perfect health.

Violent and sudden straining, again, may induce urethral bleeding. If you see such a patient micturate, you will observe that the blood comes in front of the urine; it may be fluid or clotted, but it will assuredly come first. After over-exertion in sexual intercourse, should blood appear at the meatus, it is most frequently from the prostate. Manipulate carefully the prostate, the under surface of the penis, and the perineum, a day or two afterwards, and a little tenderness will surely express the exact seat of injury, and of the hemorrhage.

The following are curious cases of urethral bleeding, such as are not generally recorded in systematic works on surgery. A gentleman was running up stairs to catch his child, when he felt something "give way" in the right groin, accompanied by immediate pain, and presently blood trickled from the urethra. Another man was wheeling a barrow, when his course was suddenly arrested by an obstruction. He also felt something "give way" in his right groin, and arterial blood flowed almost immediately from the urethra. In a third instance, one of our old students who was skating, slipped, and suddenly felt an analogous painful sensation in his right groin; and he, like the two other patients, had hemorrhage from the urethra. In all these individual cases, being consulted, I at once passed a catheter, and in each instance found the urine from the bladder perfectly clear. According to the interpretation I adopted, the blood was the result of injury to the vas deferens; and the little artery which runs with that canal being likewise injured, had allowed extravasation of blood to occur into the vas, and so into the urethra. Such, at least, in these instances seemed probable from the fact that a day or two afterwards each had swelling and tenderness connected with the spermatic cord within the inguinal canal. I give you this explanation as, perhaps, accounting for bleeding in some obscure cases, but I do not lay much stress upon it. (For further details of these three cases, see Holmes' "System of Surgery," vol. ii., p. 502.)

But, to digress for a short while. A patient, we will suppose, comes before you with a discharge from his urethra; it is, of course, highly important that you should know whence the pus, or the matter constituting the discharge, is derived. That it is pus may be recognised by the microscope. If its source is in the urethra, and the patient makes water in your presence, you will see that the pus comes in the front of the urine. You may then be assured that it is urethral, and comes from some part anterior to the bladder; which part is unsound, since pus cannot be derived from a perfectly healthy mucous membrane. Then, from what part of the urethra? Well, excluding gonorrhœa, you will generally find such a discharge is from the prostate, where it may be connected with prostatic calculus. But Cowper's gland and duct may be the seat of abscess, which is to be felt in the perineum; or an abscess in the walls of the urethra or deep in the perineum may cause pus to escape from the urethra. Suppose the discharge comes from the prostate, if you pass your forefinger up the rectum, you will find tenderness when touch-

ing the prostate. Next, press upon the gland, and then with the finger of the other hand repeat the pressure along the middle line of the perineum and underneath the penis, until a little of the discharge is made to issue from the meatus, which you should inspect with the microscope. Now, although such proceedings flow from nothing but the most common-sense principles, yet it is surprising that simple expedients of this kind, by which diagnosis may be assisted and made perfect, are not more frequently chosen. I have several times made use of this method of examining a case, and nearly always with the happiest result. You do not want much of the discharged material for diagnosis. By this method I obtained from a patient, whose prostate had been inflamed and had suppurated, a small quantity of opaque milky fluid, which I took to the late Mr. Quekett, at the College of Surgeons. He examined the specimen microscopically, and at once declared it to be prostatic without being made acquainted with the history of the case. It contained microscopical prostatic calculi. A man, perhaps, comes to you saying, "I have had a little difficulty in passing my water, which is somewhat turbid; my doctor tells me that I am suffering from disease of the kidneys, and that I can never perfectly recover." The medical man has examined the urine, and, finding in it a little albumen, has jumped at the conclusion that the kidneys are at fault. But, do you examine the patient carefully; you will, perhaps, find a simple abscess in the perineum opening into the urethra, and giving rise to all the symptoms. If you gradually press the contents of the urethra forwards, you will probably find pus escape in a small quantity from the meatus.

I told you just now that blood from the urethra never travels backwards into the bladder: the exceptions are very few and special, and I believe you may safely act on that general assumption. If the third lobe of the prostate projecting into the bladder is the seat of cancerous or other disease, you may then have hemorrhage which partly enters the bladder. Again, if a catheter has tunnelled the prostate, making a false passage from the urethra into the bladder, then blood may be found in the latter cavity. Lastly, with paralysis of the bladder and urethra, the latter becomes simply a dead tube for the time being; should it then be injured, there is no power of the muscle of the urethra and of the sphincter of the bladder to prevent a retrograde motion of part (at any rate) of the blood into the bladder. But, with these few exceptions, you may lay it down as a rule that urethral hemorrhage does not travel backwards to the bladder. Carry out this observation a little further. You know that extravasated blood, if it continue undisturbed in any cavity inside the body, will in clotting assume the configuration of that cavity. And so in hemorrhage into the urethra; if the blood remains there undisturbed a little time, it forms clots which, being casts of the canal, are lengthened, and cylindrical or ovoid transversely. If there are several clots of cylindrical shape, escaping in front of the urine, you may with certainty say that they are from the urethra. But if you want to ascertain exactly their form for the purpose of diagnosis, take a little trouble about the matter, and always swim out the clots in clear cold water in a white dish or plate, giving them time to unravel themselves. If they are long and perfectly round, the site of their retention has been anterior to the prostate; if ovoid in their long axis, or like two triangles joined together at their bases, they were moulded in the prostatic portion of the urethra, and will probably be found grooved on their under surface. Fresh blood poured out rapidly will clot along the whole length of the urethra, like macaroni. But old clots from the urethra, numerous or single, may appear cylindrical, like pancakes rolled upon themselves, until swum out in water, when they perhaps unfold; this shows a slow and limited escape of blood into the urethra.

With hemorrhage into the bladder, the form of the clots is determined by the shape of the vesical cavity. When first passed from the urethra, such coagula may seem to be cylindrical, so that you are, perhaps, inclined to say they are urethral clots; but put them into a dish of water and they will then resume their flattened, circular true shape. If the bladder is full of clot, nothing but blood-stained serum and urine will, perhaps escape for days,

until the clot is broken up by a catheter. Coagula formed within the bladder are somewhat irregular in outline, mostly circular flattened masses, bevelled off and fimbriated. If they have remained long *in situ* their fringed edges are white, being formed of decolorised fibrine. Clots of this description evidently could not have formed in the urethra, nor in the ureter, nor in the pelvis of the kidney; and the blood forming them must have clotted within the bladder itself. If the clots are numerous and small, there must be many sources of the bleeding, or repeated escapes from the same source. To show you how I put these principles into practice, I will relate a case. Last Monday morning I was requested by a physician to visit a gentleman suffering from hematuria. On my entering the sick chamber three pots occupied by urine and coagula were handed to me: and I was asked to state the origin of the hemorrhage. Before examining the patient, I emptied the contents of one of the pots into a basin of water, and had the satisfaction of seeing the clots all come out with their physiognomies and faces well marked and all alike. They possessed the characters I have just described as pertaining to bladder clots. Then came the query—what was the cause of the bleeding? Without troubling you with the history of the case, I may state that it might in that gentleman have been stone, a fungus-growth, or ulceration of the bladder. I next passed a catheter, but could find no calculus. Upon examining by the rectum, however, I detected a firm growth, in fact, a little vascular tumour, seated above and behind the prostate, and which gave rise to this troublesome hemorrhage. The diagnosis was quickly complete; but it is not too much to say that the close observation of the shape of the clots of blood principally guided me to my conclusion. Coagula in the bladder may have come down from either kidney; but if so, there would most likely be pain in the corresponding loin, at least during the passage of the clots. Besides this extra source, hemorrhage into the bladder may result from ulceration, from stone, from a small fungus growing into the bladder, from the third lobe of the prostate, from injury to the bladder caused by accidents or by catheterism, and from cancer.

Take another outlet; let us pass to the consideration of hemorrhage from the anus or rectum. A patient comes to you complaining that he is losing blood from the bowel. Your first question should be—does the blood precede, accompany, or follow, or is it mixed with, the motion? If it comes before the motion, it probably arises from the lower part of the rectum. If it comes during the motion, it is probably caused by the feculent matter running across the plugged-up mouth of a wounded vessel, and displacing the plug. If it comes after the motion, it is evidently associated with tumour or something of the sort higher up the intestine. You should then ascertain whether the blood is fresh or clotted, and if the latter, whether the clots are new or old. If it is fresh fluid blood, or bright arterial and florid, it comes from somewhere near the anus. The patient may inform you that something seems to protrude at defecation: are you to be satisfied with this? No, it is your business to ascertain the source of the blood; and as honest, legitimate surgeons, you must, for the benefit of your patient, take some trouble to determine the exact nature of the malady, however disagreeable the necessary proceedings may be to you. In the case I have cited you must send your patient to the water-closet, tell him to bear down thoroughly, and examine the protruded parts, before letting him clean himself. In nine times out of ten you will see at once the source of the bleeding. The blood may be squirting on the sides of the pan of the water-closet, or on the thighs of the patient. It may be that all the hemorrhage comes from a small depressed ulcer seated near the anus, or it may come from several little raspberry-like prominences. As they are everted during defecation, it will be extremely easy to apply a ligature or the actual cautery or nitric acid to the bleeding part, according to your own choice. I wish you, moreover, to ascertain the cause of the patient's symptoms thoroughly, because, to put the matter on a lower ground, I believe that you will not then be harassed with so much doubt and anxiety in your *professional* careers.

The following are two instances which I practised the foregoing prin-

ples. A gentleman in the City, who for many months had looked exceedingly blanched and ill, came to me suffering from hemorrhage from the rectum, which had then continued for months. He was so nervous that he could not pass a motion in my presence. I accordingly ordered him an enema, and waited to see it administered. As the injection came away, the mucous membrane protruded, and an opened artery bled very freely from a punched-out ulcer, as big as a crow's quill. All this came into clear view. I ligatured the mucous membrane, including the ulcer, and the bleeding ceased from that time. He remained under treatment for weeks, whilst his health, which had been dreadfully shattered by the frequent hemorrhages, was being restored; but the bleeding never recurred, he gradually became quite well, and I now frequently see him, a strong, healthy man. The wife of a surgeon who had become extremely ill from recurring intestinal hemorrhage, and who had been attended professionally during a long period without benefit, consulted me. She was even thought to have malignant disease, in consequence of her yellow appearance; and it was supposed that she suffered from cancer of the stomach or liver. I saw that the clots which passed were common spherical masses of the ordinary red colour. From this I argued that the source of the hemorrhage must be close to the anus. For, if the blood be poured into the stomach, or intestines high up, it is acted on by the gastric juice or by the acid secretion of the cæcum. Feeling sure that the origin of the bleeding must be close at hand, I passed my finger up the rectum, and felt a tender spot, which I could also distinguish to be ulcerated, owing to the different sensation I there experienced. I then introduced a cylindrical speculum, which enabled me to see an ulcer freely issuing arterial blood. I passed a sharp-pointed knife beneath the ulcer, and divided the bleeding vessel as well as the ulcer itself; from that time the hemorrhage ceased, the patient rapidly recovered, and had no return of the bleeding, and is now, fifteen years after his occurrence, in good health.

To keep our observation on the rectum. We will suppose a patient passes one large single firm clot, not several coagula; it has not, perhaps, been acted upon by the gastric juice, nor by the acid of the cæcum. The hemorrhage in such case comes from one source, and from the large intestine not far from the outlet. The blood has rapidly escaped from the vessel, be it artery or vein; the arterial source furnishing the firmer coagulum. But a large clot may be a collection of smaller clots aggregated. Disturb it in water, and separate the component coagula, and observe their form for diagnostic purposes. Numerous clots mean repeated hemorrhages at intervals, and in no great quantity, from one source, or from several sources at the same time. Thus you may have the same amount of clot from the rectum of each of two patients. In the one case there shall be one clot only; in the other as many as fifty or sixty may be present, closely aggregated, so as to form but one. I had a good case illustrative of this point with the late Sir Benjamin Brodie some years ago. The patient was suffering from cancer of the rectum, and was passing masses of small coagula, which indicated repeated hemorrhage from the same spot, or from several spots. But the nature of the disease precluded us from tying any bleeding vessel or vessels. Ultimately, ice passed into the rectum arrested temporarily the bleeding. A lump of ice, I may observe, intended for such a purpose should be first placed in a warm hand, so that its sharp edges may be melted off.

If a patient vomits a large quantity of blood, the state in which it is brought up has reference to the relative quantities of blood extravasated, and of the gastric juice then in the stomach. Blood coagulates freely in the stomach, the quantity be large, though in a peculiar manner. The free acid of the gastric fluid coagulates the albumen, and the fibrine remains fluid; so that a stomach clot consists of defibrinated blood. The gastric fluid, moreover, converts the red colour of blood into a blackish-brown tint. If a large clot is vomited which has not been acted upon by the gastric juice, it has been caused by rapid effusion, and is probably from an artery. But blood coming from the stomach in large quantity and not coagulated evidently must be all fresh blood, and must have left the vessel immediately before coming up; whilst, if it be

grumous, nearly fluid, of a brown-bistre colour, and fragmentary, the effusion has probably been slow, and it has been acted upon by the gastric acid.

Surgeons are often accountable for the vomiting of blood, which fact you should bear in mind, or you are liable to unnecessary alarms. After operations on the lips, uvula, tonsils, or tongue, or after epistaxis, nothing is more common than for hematemesis to occur. A man perhaps receives a blow on the back of the head, and you have some suspicion, from the nervous symptoms he exhibits, that he has fractured the base of his skull. The diagnosis may be much helped if, on looking into his mouth, you discover blood trickling down the back of his pharynx, and afterwards being vomited from the stomach. He has probably sustained a "fracture of the base," which extends across the top of his pharynx, or across the back part of the roof of his nose.

From the foregoing observations you will see that it really is quite remarkable how frequently you may employ blood for the purpose of diagnosis.—*Guy's Hospital Reports*, 1868, *p.* 22.

45.—PLUGGING THE POSTERIOR NARES IN CASES OF PERSISTENT EPISTAXIS.

By Dr. J. Inglesby Mackenzie, Surgeon to the Sidmouth Dispensary.

I was sent for to a patient of full habit, subject to frequent attacks of rheumatic gout, who was suffering from epistaxis. Plugging the anterior nares had been adopted prior to my arrival, though ineffectually. This was repeated, the plugs being saturated with tincture of matico. Cold affusions were applied, but without success. I then proposed plugging from behind. I passed a No. 10 elastic catheter (in which I had cut an opening opposite to the usual one at its extremity) through the nostril, till I was able to seize it and draw it forward through the mouth, and passed the string to which my plug was attached through the eye made in the catheter, which, when withdrawn through the nasal orifice, enabled me, by tension on the string, to bring my plug into its required position. I fixed the end of the string to the forehead with strapping. The effect of this plugging was satisfactory; and it became necessary in three or four days to remove the plug.

I have failed to find any account of this part of the operation in any of the text-books at hand; but the plan adopted was a very simple one. I re-inserted the string in the eye of the catheter, and ran the catheter along the string up the nostril, detaching the plug, which had previously been loosened by injections of lukewarm water, caught the plug at the fauces with dressing-forceps, and simultaneously withdrew it and the catheter from the nostril.

The operation, minor as it is, is one requiring some little manual dexterity—the more especially if the patient is, as in the case to which I referred, highly nervous; which feeling was greatly increased by the sense of suffocation experienced during the operation. Bellocq's instrument is not always at hand; but there are few surgeons who do not carry an elastic catheter in their pocket-cases, and consequently have the means ready to give relief to a state which is to a patient at once distressing, if not terrifying.—*British Medical Journal*, *Feb.* 8, 1868, *p.* 115.

46.—CLOSURE OF ARTERIES BY TORSION.

By Prof. Syme, Edinburgh.

Having lately recommended the use of "torsion" on all occasions except where the great arterial trunks are concerned, I think it right to state that about a fortnight ago, having amputated the leg of a man in the hospital at the knee for disease of the joint, I twisted the popliteal artery with perfect ease and success. The stump being treated in accordance with the antiseptic system, there was no constitutional disturbance, and no suppuration. On the fourth day there was a slight serous discharge, but hardly any afterwards, and on the eighth day the wound was completely consolidated.

Although it would thus appear that arteries of the largest size are amenable to torsion, the expediency of trusting to this means for their closure, instead of employing ligatures, will admit of question until further experience ascertains how far it is safe to do so. But in the meantime there can now be hardly any hesitation in twisting all the vessels of ordinary capacity; and it may be mentioned that since I adopted this system there has not been a single case of secondary hemorrhage, either in public or private.

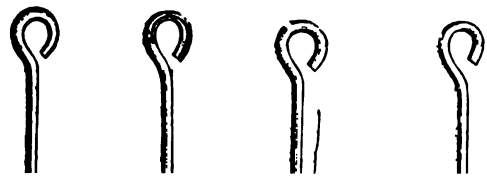
[The proper mode of applying torsion is to twist the end of the artery until it is twisted off. We believe it is to this that Mr. Syme refers. EDS.]—*Lancet*, *Jan.* 4, 1868, *p.* 26.

47.—ON ACUPRESSURE.

By J. COOPER FORSTER, Esq., Assistant Surgeon to Guy's Hospital.

[Being desirous of seeing acupressure thoroughly tried by Drs. Pirrie and

FIG. 1. FIG. 2. FIG. 3. FIG. 4.



Keith, and of hearing from their own lips the most successful modes of applying it, Mr. Forster undertook a journey to Aberdeen. The ease with which the needles were applied in the operations witnessed by him, together with the success attending their application, induced him on his return to London to try the plan thoroughly.]

Before detailing the cases in which acupressure has been adopted I would draw attention to the modes of procedure which I have found most advantageous. There are seven methods described in the work of Drs. Pirrie and Keith, but practically only three are constantly available; and one of these is much more useful than the others.

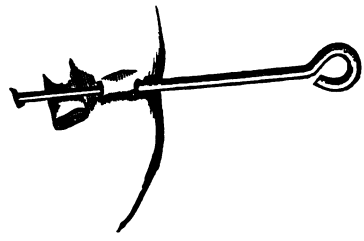
The methods to which I allude are the first, fourth, and fifth, and of these the fourth is the most useful. Let me now proceed to describe these plans, for I feel assured that if acupressure be generally adopted (as I sincerely trust it may be), one or other of them will be pursued.

The first method consists in passing the pin from without through the skin by one side of, and close to, the bleeding vessel, and pushing the point through the skin so that the artery lies acupressed between the pin and the skin. This is shown in fig. 1 in the plate. Where the vessel runs superficially it is a very satisfactory mode of acupressure: it is not, however, thought to be applicable to a large vessel, nor to one that requires much pressure—not being a very secure plan: still the vessels of the cord are amenable to it, and the arteries of the forearm.

The fourth method consists in passing the pin underneath the vessel in the wound, slipping a loop of wire over the point of the pin, and compressing the bleeding vessel between the double wire and the pin, fixing the wire tightly by coiling it around the pin again. The vessel appears as shown in fig. 2.

The fifth method (known as the Aberdeen twist) is practised without a wire, as the first, and consists in passing the pin by the side of the bleeding vessel, bringing the point out of the flesh, then turning the pin round a quarter of a circle, allowing its point to go over the vessel, and fixing the needle into the soft part beyond. It is difficult to show this in drawing, but figs. 4 and 5 will give a tolerably fair idea of the disposition of the pin in the two stages:

Fig. 5.



first, that of introduction of the pin, and secondly, that of fixation. It suffices to say that the vessel is compressed between the pin and the soft parts: these methods are demonstrated in a few minutes by any one who has adopted the plan, and rendered perfectly intelligible, but I doubt whether any description, however accurate, can render them so plain as to make them available by any one who has not seen the process.

The plan suggested by Mr. Durham consists in passing the pin as in the fourth method, and in using well waxed silk ligature or fishing-gut, which is better, instead of wire; this is looped around the point of the pin, crossed over the artery, soft parts, and pin, and tied underneath the latter on the opposite or head end of the pin; the vessel is thus compressed between the pin and the doubled silk or gut. This is seen in fig. 3.

[Mr. Forster then relates numerous cases in which these methods were used, and proceeds to say:]

Acupressure, to be satisfactorily performed, requires considerable practical experience. There is no doubt that the exact amount of pressure required to compress the vessels can only be learnt by practice (in two of the cases herein detailed hemorrhage occurred, apparently from want of that proper amount of pressure); hence there will always be some difficulty in instructing students and others in the plan, especially in its fourth method, the most useful. In some cases it appears that very little pressure, properly applied, will restrain the bleeding even from a large artery, as witnessed in the fifth method; in others, if the vessel be not exactly hit, or there is more muscle than there should be between the wire and the artery, or if more tissue intervenes between the artery and the pin than is desirable, the wire must be tightened with considerable force ere the hemorrhage is controlled. Should there, however, be any difficulty in arresting the bleeding from these causes, the pin can easily be taken out and re-introduced closer to the vessel, or a fresh wire applied. A little practice soon overcomes these obstacles.

By many it is said, "We do not doubt the possibility of restraining hemorrhage by this means, but in what respect is it superior to the ligature?"

Some reply, "In the avoidance of small sloughs." We believe much more importance than they deserve is attached to these small sloughs, which are said to be caused by the ligature strangulating the part of the vessel beyond the spot where the thread is tied; they (the sloughs) are said to be the cause of pyæmia, suppuration, and other evils: be it so, but the piece of tissue which is compressed between the pin and the wire has its vitality very materially damaged, if not destroyed, by the compression made use of, and though, as a rule, forty-eight hours is the longest time (and less than this is frequently allowed) for the pin to remain, I believe it is sufficiently long to seriously damage the life of the part beyond the pin and wire, and thus there is no particular advantage gained by acupressure on this score. Much remains to be known regarding the length of time during which the pin should be allowed to remain before the artery is secured, but whatever time elapses, the part must be squeezed rather severely between the pin and wire, if the latter be used.

Other objections are raised by inquiring what becomes of the accompanying nerves and veins when acupressure is employed: are they not seriously damaged?

In the case of the veins I can only say it is a great advantage to have their disposition to bleed arrested in so simple and efficient a manner. No damage seems to result from their compression, nor from compression of the nerves. I have not heard of any expression of pain at all greater than is common after amputation, when the ordinary ligature has been employed, and when the greatest care has been taken not to tie the slightest filament of a nerve: on this head therefore I believe the advantage to be considerably on the side of acupressure.

The records of Drs. Keith and Pirrie as regards pyæmia are very satisfactory, but that may be accidental. In the cases I have reported there have been four deaths from pyæmia after amputation—two of the thigh and two of the leg; but in justice it should be observed, that in one of these cases the patient was unfortunately in the next bed to a man in whom erysipelas appeared; in the other case, a double amputation was performed, the vessels being secured by ligatures in one leg, and by acupressure in the other; in another there was phlebitis previous to the operation. There remains then only one case (8) in which acupressure was employed, and, without any exceptional occurrence, pyæmia took place.

One great advantage promised by the advocates of acupressure is, that wounds in future will unite by adhesion; where now they heal by suppuration. On looking over the work of Drs. Pirrie and Keith before alluded to one is dazzled by the success they have obtained, and therein reported; it is totally at variance with my experience in the thirty-one cases I have recorded; it must be considered as exceptional. The sanitary condition of the Aberdeen Hospital, and the arrangement of the beds there, certainly did not strike me as peculiarly favourable to the adhesion of wounds. On the contrary, I look upon the scene of the labours of those surgeons as one that must frequently be the source of great annoyance to them, as likely to re-

the rapid convalescence of their patients. I need only mention the fact on one side of the wards there are no windows, nor any means of ventiɔn, except by doors opening into a dark passage, which runs along the .tre of the building (a particularly objectionable style of building, to my ıd). We must, therefore, look to other causes for the wonderful plastic wer from which such marvellous effects have resulted in the far north, and tribute it to the excellent health of the patients on admission, or to their ɔculiar diet—the porridge, Aberdeen bap and Finnan haddock indulged in ır breakfast, with goodly potations of the favourite stimulant of the country, r to the fresh air and sea breezes of Aberdeen, and not to acupressure alone: otherwise some measure of similar success might have fallen to Mr. Durham ınd myself. We have been most careful to arrest the hemorrhage from all the bleeding vessels, and to get the wounds to a certain extent glazed before bringing the edges together, yet, notwithstanding all the pains taken, perfect primary adhesion, without a drop of pus, has not in one instance rewarded our care thus far.

Regarding the probability of secondary hemorrhage after operations, of course it may occur both when vessels have been acupressed and when the bleeding has been arrested by any other means, but owing to the possibility of including several vessels at once in acupressure, it is less likely to take place than when a ligature is applied round the special vessel; and here acupressure undoubtedly becomes a great gain. That a smaller number of foreign bodies need be employed in acupressure than when the ligature is used, is evident from the twenty-first case, where but one pin was required to arrest the bleeding vessels of the arm in a child, where I was compelled to amputate; this could scarcely have been the case, if ligatures had been employed.

What great advantages, then, are offered by acupressure in securing bleeding vessels where adhesion of the surfaces of the wound is required? for, be it distinctly understood, where no adhesion is required, there is no necessity for acupressure.

1st. The ease with which one person can arrest bleeding. The surgeon is independent of the assistance of any one to tie the vessels while he is taking them up with the forceps: this is by no means to be despised in country practice, where assistants are not as plentiful as in a London hospital; perhaps the spring artery forceps may be considered by some as affording an equal advantage; I do not share that opinion.

2nd. The control of the venous hemorrhage at the same time with the artery. This could, of course, be accomplished by tying both artery and vein in one ligature, a plan certainly not to be recommended, although I never shared the fear that was formerly and is now sometimes expressed, of tying a vein; still it appears rather a clumsy way of arresting the bleeding of the two, to put one ligature around them, when a pin and wire for twenty-four hours will answer the same purpose.

3rd. The short time, forty-eight hours at the very longest, that a pin, or pin and wire, if both be used, need be left in the wound, constitutes to our mind one of the greatest advantages of acupressure; I believe that only those who have tried the plan, amid the obstacles which invariably present themselves to the adoption of any new course, can appreciate the satisfaction felt by the surgeon, who (in an amputation of the breast for instance) can, at the end of less than twenty-four hours, remove from the interior of the wound all foreign bodies; not alone by the surgeon is the satisfaction felt, for I have observed the patient to rejoice at the absence of something which she knows has been placed there for her safety, and has so speedily fulfilled its purpose. The surgeon can also honestly state that there is no obstacle, in the shape of an extraneous body, to the entire union of the wound after the pins have been removed.

There is a satisfaction about this removal of the pins, which, I repeat, can *only* be felt by those who have confidence in the proceedings, and who have given it a fair trial (as I have done). This alone would induce me to use it in preference to the ligature.

hemorrhage

4th. How often has the surgeon for several days enjoined especial care upon the dresser, and used the same himself, lest any untoward or accidental pull upon the ligatures should tear them away before the perfect securing of the vessel has been effected. All this caution is rendered unnecessary, or, at any rate, exists but for a few hours where acupressure is used.

5th. The offensive moist seton, in the shape of the ligature hanging from the wound, is gone; is not this in itself sufficient to recommend the use of the pin and wire? I think so, and have no hesitation in advising those to try it who at present condemn it as not being sufficiently advantageous to warrant a departure from the old custom.

I believe it to be necessary that acupressure should be seen carried out by some one tolerably perfect in it before it can be sufficiently understood to be practised; no mere verbal description will suffice. It was the inadequacy of description which induced me to see the plan in its entirety at Aberdeen, where undoubtedly (at least so far as I can understand) it is more generally in use than elsewhere. To my mind the difficulty of describing the plan is sufficient to account for the failures of which I have heard, and the deaths from hemorrhage put down to its use, which have unquestionably shaken the faith of more than one person in its efficacy as an hæmostatic agent.

It should never be forgotten that acupressure need only be employed where adhesion of the surfaces of the wound is required; nevertheless it may be used where any hemorrhage is taking place, if the vessel can be acupressed without much difficulty, and also when a speedy withdrawal of a foreign body is advisable. In secondary hemorrhage from the palm of the hand (as in Case 22, where I never saw anything more satisfactory) the usual practice would undoubtedly have been to have tied both ulnar and radial arteries, or, perhaps, the brachial, instead of which a pin was placed under the two former, and removed in forty-four hours, with the most perfect control of the hemorrhage, which did not again occur. It was quite impossible to secure the vessel at the seat of injury, as I believe the bleeding came from the deep palmar arch.

What are the disadvantages of acupressure? Suppuration along the track of the pin certainly may occur, but it is very rare. It is asserted by some of its advocates that so great is the tolerance of the flesh for metals, that foreign bodies of that nature may lie for weeks, nay months, or even years, in the body, without giving rise to anything more than a pain, and that, therefore, a pin will not cause suppuration along its track. One fact is worth a dozen conjectures. In Case No. 17 before mentioned it will be observed that considerable suppuration occurred along the course of the pin, and I may notice that the irritation did not subside for many days, yet the pin was only kept in for forty-eight hours—too long a time I quite admit, and which I should carefully avoid allowing to elapse before removal of the pin on another occasion.

Another objection sometimes raised is the protrusion of the pins and wire from the wound, giving the stump, when many pins are used, the appearance of a well-stocked pin-cushion. So frivolous an objection can hardly be entertained seriously, but, even were it worth considering, as the disagreeable condition lasts, at the longest, but forty-eight hours, the objection is, I think, of no practical importance.

To a patient the presence of the sharp points of the pins, if the first method be used, is objectionable, on account of their catching in the bedclothes; but, of course, the points can easily be protected.

It is said by some opponents of acupressure that the pain on the removal of the wire is a serious obstacle to its use, and undoubtedly some little irritation is very frequently felt when the pin is being withdrawn. At the subsequent withdrawal of the wire there is a momentary pain, but that does not always occur; still I will admit there is some objection to the plan on this score, but this is not the case when Mr. Durham's mode is adopted, as the soft silk or gut slips off more easily than the wire, and any one who gives these materials a trial will, I think, be very satisfied with them for this reason amongst others.

How acupressure *acts in* arresting the course of the blood in a vessel so

completely as that in a few hours the pin may occasionally be withdrawn—it is, I confess, a difficult matter to explain: and it is not the object of the present paper to discuss this question, but rather to assist in advocating the use of acupressure, instead of the ligatures, not by endeavouring to prove it a wonderful means of obviating all the dangers attending operative surgery, and of getting adhesion in wounds which would never otherwise unite; but rather by stating fairly, and without prejudice, what it does and what it does not do. For my own part I have quite decided in its favour (for the reasons I have given), and never intend again to use a ligature in any case where the edges of a wound are brought in apposition with a view to their adhesion. That some more simple plan than that of the pin and wire may be substituted I fully believe, and shall be most happy to cast them aside, as I now do the ligature, when such contrivances shall have been suggested; until that time arrives I shall continue to use acupressure as at present applied, in lieu of the ordinary ligature, with the same feeling that prompted the use of that means of arresting hemorrhage, instead of the hot iron, for I consider it as great an advance on the ligature as the ligature on the cautery.—*Guy's Hospital Reports*, 1868, *p.* 113.

48.—THE CLOSING OF BLEEDING BLOOD-VESSELS BY ARTERIAL FORCEPS.

By Dr. WILLIAM TAYLOR, Edinburgh.

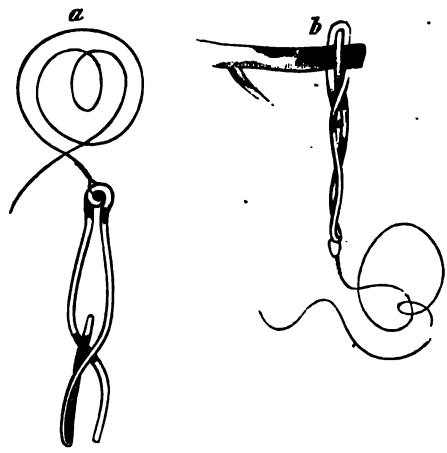
[The following article was elicited by that of Mr. Nunneley, of Leeds, given in our volume for last half-year. The experiments recorded in Mr. Nunneley's paper bear a close resemblance to a series in which Dr. Taylor was engaged when a pupil of Sir James Simpson two years ago. These were embodied in a thesis presented to the medical faculty of the University of Edinburgh in March, 1866. The following is an extract from this thesis.]

This may be regarded as emphatically the iron age in surgery. The knife has, in a great measure, supplanted the caustic holder; the metallic suture the silken seam; and now acupressure threatens the extinction of the ligature. We cannot have too many changes, provided that each be an improvement on that which went before; and, as acupressure is manifestly a step in the right direction, let us lend our aid in bringing it as near to perfection as possible. A healthy cicatrix is a far prettier sight than a suppurating sore, and a week's superintendence far preferable to a month's dressing; and if such benefit to our patient and such pleasure to ourselves can by any means be obtained, even once out of a hundred times, I hold it to be our duty to give him the full benefit of that chance. But the advocates of acupressure expect—and I think reasonably—far higher triumphs than these; they expect to see at least fifty per cent of their patients benefited by its use; and they have the pleasing reflection that those wounds which fail to heal by first intention under its application, belong to a class the healing of which is not by any means favoured by the ligature.

Although convinced of the supra-excellence of acupressure, I confess that, like many others, I have failed to see the possibility of its universal application, more especially to the deeper seated parts; and, although firmly believing that the ligature is most certainly doomed to disappear from our vision within a half a dozen years, still I think it probable that something besides the straight needle, but made to act on the same principle, will be required to aid in supplanting it.

Acting under these convictions, I set myself to devise a method whereby all the advantages of acupressure might be applied not only in regions where direct pressure from a straight needle would be unattainable, but also in parts that are fairly accessible. The result has been the construction of a self-acting wire-compress, which I have dignified with the name of "acupressor arteries." This minute piece of mechanism is so constructed as to retain a firm hold of

the artery, and, although hid from view by being enclosed within the divided tissues, it can be removed at pleasure any time after the operation. It is therefore tantamount to a metallic ligature, for the separation of which we



a. The acupressor arteriæ placed in readiness to grasp the artery.
b. The acupressor arteriæ closed upon the artery.

have not to wait for two, three, or four weeks, but which can be removed at any moment without the slightest trouble. It consists of a needle, or rather a piece of steel wire, bent upon itself in the middle and turned into a single spiral, which constitutes a spring; the extremities are so adjusted that by the aid of the spring, they lay hold of the artery to be compressed. The compress is then left in the wound, but connected with the spiral at the end is a small wire; this is allowed to hang out in the same manner as the ligature is at present. When the time has come for its removal, a slender silver tube is passed inwards over the wire until it reaches the acupressor arteriæ; the wire is then held firmly by the left hand, and the tube passed steadily inwards with the right. On reaching the converging arms of the instrument, they become compressed within it; the extremities then relax their grasp of the vessel, and the whole is gently withdrawn. (See illustration.) Its advantages are numerous.

1. It is perfectly secure. It will be noticed that, when in use, the reflected limb acts as a spring, retaining the other in its place.

2. It is easily applicable. Anybody could apply it with security, whereas often a well-tied ligature may give away.

3. In cases of necessity, it may be applied without the aid of an assistant, and would, therefore, be invaluable alike to the military surgeon and the country practitioner.

4. It can be applied in one-third of the time it takes to put on a ligature, and in any part where the ligature can be applied.

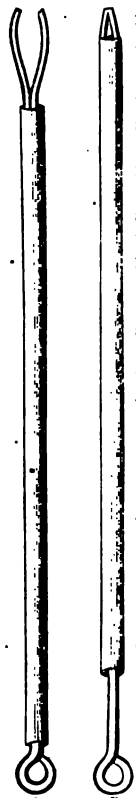
5. Its great simplicity and cheapness are a recommendation.

6. It is applicable to the treatment of aneurism. It may be applied at the Hunterian site; and in a few days will cause partial or complete occlusion; after which it may at once be removed.

7. It is perfectly applicable to deeply seated parts; wherever the knife goes, it can follow, even although the wound be small. For this purpose, it should be caused to gape by being partially drawn within the end of the tube; it is then applied by being pressed outwards from the interior of the tube, and the tube withdrawn.

I hope that soon all these statements respecting it may be confirmed, and that it may be found useful to the profession.

Forceps of the same kind and for the same purpose as those described by Mr. Nunneley, have, I know, been suggested by Dr. Hoffman and Mr. Staunton; but such instruments are far too cumbersome to admit of their being a benefit to surgery; while the cost would be so great as to prevent them from ever being universally adopted. The acupressor arteriæ, on the other hand, is simple in construction, and could be manufactured by the gross at a very small cost. It will, I think be found exactly to supply the defect which Mr. Nunneley confessedly feels in the use of his forceps, and of which he is now endeavouring to discover the remedy. It is, in fact, the realization of what he aims at when he says: "I think it probable that manufacturers of the safety-pin, or like handicraft workmen, may be able to supply a forceps sufficient for all purposes, at so small a price as to allow it to be used without any consideration of economy. This I am taking steps to ascertain. At one time, I thought of employing forceps whose blades are brought together by a screw motion or by a sliding tube; the latter idea had also occured to Mr. Birkett." &c.



A. B.

In the case of the acupressor arteriæ, the sliding tube is made use of for the liberation of the artery, or in cases where the instrument has to be applied to an otherwise inaccessible region. It forms no part of the instrument, but may be used for any number of them; thus the objection of costliness is obviated.

Mr. Nunneley says further: "I think it by no means improbable that, in some cases of aneurism, compression by similar forceps, of the artery, on some point of its course above the sac, for a greater or less length of time, may be successful. The sheath of the vessels should be merely punctured to an extent sufficient to allow of the forceps being glided upon the artery alone, no other part being included or disturbed," &c.

It will be seen, by the extract given above, that these ideas with regard to aneurism had also been anticipated in my thesis. —*British Medical Journal, Feb.* 1, 1868, *p.* 92,

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49.—ON A NEW METHOD OF ARRESTING HEMORRHAGE BY CANULA ARTERY FORCEPS.

By Dr. J. R. Wolfe, Ophthalmic Surgeon to the Aberdeen Royal Infirmary.

[Dr. Wolfe, who certainly at Aberdeen must have seen a good deal of acupressure, considers the several pins and loops of wire used in that process too cumbersome and formidable to recommend themselves to general use.]

Being in the habit of using extensively the *serres-fines*, and having daily opportunities of witnessing their superiority even over the metallic ligature, the idea suggested itself to me of applying the same to the securing of arteries. The *serres-fines* is exactly the self-acting forceps of Mr. Nunneley, published in this Journal of October 12th. But, owing to the difficulties I experience in withdrawing such an instrument from a deep wound, and to the danger of leaving it in an artery unguarded. I planned an instrument, which Mr. W. B. Hilliard of Glasgow constructed for me, and which I call the "canula artery forceps." It is so exceedingly simple, that the annexed drawing almost explains itself. A, shows the instrument open; and B shows the same closed. The sliding tube serves the purpose of a clamp. The largest does not exceed the size of a large acupressure-pin. Smaller sizes are used according to the depth of the wound, or the size of the vessel to be secured. The cost is only one shilling each.—*British Med. Journal, Nov.* 16, 1867, *p.* 444.

ALIMENTARY CANAL.

50.—POSITION IN THE REDUCTION OF INGUINAL HERNIA.

By Dr. Joseph R. Bond, Yarmouth, Nova Scotia.

Several years ago, I had a case in which the patient could not reduce an inguinal hernia while lying in bed either on his side or on his back, but as soon as he stood on his feet there was not the least difficulty. If, on removing his truss before going to bed, he neglected to apply his hand to the part and allow the rupture to protrude, he had always to get upon his feet before he could reduce it. Soon after this I was called to a case of strangulated inguinal hernia. After making every effort in the usual way to reduce it, I directed the patient to stand up; I placed myself (also standing) behind him, and encircled his body with both my arms, grasped the tumour with both hands, and effected in a few minutes what I had failed to accomplish in as many hours. Since then I have had many cases of inguinal hernia in my own practice, and several where I have been called in consultation, and have never failed to effect a reduction in a few minutes in the way I have described. I have never seen this means tried in the Hospital in Philadelphia nor in the London Hospital, although in both these institutions I have repeatedly seen all efforts fail to reduce an inguinal hernia without an operation. Nor have I ever seen it recommended in any surgical work.

My object in sending you this communication is to ask my medical brethren of the metropolis to give the erect posture in the reduction of inguinal hernia fair trial, and to publish the results. In femoral hernia the erect posture has never succeeded in my hands—in three cases I have been obliged to use the knife—in inguinal never. I will not attempt to account for the use of the erect posture in the reduction of inguinal hernia, nor for its failure in femoral. It may be thought that the erect posture favours reduction by causing syncope, but in only two cases do I remember that a feeling of faintness was complained of. In the last case (only a few days ago) the patient, an old man, fainted and fell as soon as the gurgling began to be felt, and I finished the reduction whilst he was prostrate.—*Medical Times and Gazette, Jan.* 4, 1868, *p.* 23.

51.—ON THE TREATMENT OF OBSTINATE CONSTIPATION BY FARADISATION OF THE BOWEL.

By Dr. Julius Althaus, Physician to the London Infirmary for Epilepsy and Paralysis.

[In the following paper it is the object of the writer to place on record a few of the numerous cases of obstinate constipation which he has found to yield readily to faradisation of the bowel, after the failure of other modes of treatment. The first case is that of a lady suffering from headache and other symptoms arising from habitual constipation, which Dr. Althaus had not succeeded in relieving by the use of ordinary purgatives.]

I now proposed to the patient the application of galvanism, for inducing a healthier action of the bowels: and, as she consented at once, I introduced an insulated metallic sound, the top of which only was free for about an inch, and which at the other extremity was connected with the negative pole of the primary faradic current, into the rectum, a moistened conductor connected with the positive pole being placed on the abdomen. A current of moderate power was then allowed to pass for fifteen minutes. The patient did not complain of any pain or discomfort from the application, but said she felt more lively and hopeful than she had done for a long time.

24th. She came to me in high spirits, saying that since the faradisation was used she had every day had a better motion than for many months past.

ordered her now to discontinue the colocynth pill entirely, and merely to take a wineglassful of Friedrichshall water thrice a day.

From that time forward the patient made an uninterrupted recovery. Faradisation was continued twice a week for a month, after which neither medicines nor mineral waters nor the galvanic stimulus were any longer required. The head symptoms disappeared gradually, in proportion as the action of the bowels was restored; and when I last saw the patient (July, 1867), she was perfectly well, excepting the somewhat copious menstruation, which was now the only trouble she experienced from the fibroid tumour of the uterus.

Case 2.—A married woman, aged 28, came to me in July 1866, with the complaint that her bowels were very much bound, and that she suffered from difficulty of walking and micturition, and a sense of distressing fulness in the pelvis. She said that her parents had made her take Morison's pills from a very early age for everything that ailed her, whether it might be a headache, a bad finger, or a sore throat. She had always had difficulty with the bowels after taking pills for some time. She was married in 1863, and had had two children, the last in January, 1866. After the first confinement, the bowels were very sluggish indeed, and required strong aperients to make them act. After the second delivery, this condition was, if possible, worse. She now went sometimes a whole week unrelieved, and, on making exertions for defecation, experienced intense pain about the diaphragm, so as to feel quite sick and giddy. She was restless at night, and generally thirsty, but the appetite was very feeble. The tongue was dry, the abdomen very full and tense, and painful on pressure. Pulse 90. On examining the rectum, I found it choked with hardened fæcal matters, some of which I scooped out at once. I then inserted the negative electrode of the primary current, as above, and sent the galvanic stream through the bowels for ten minutes. On the evening of the same day, a very large quantity of highly offensive fæcal matters came away, with a feeling of intense relief to the patient. Suitable rules as to diet and regimen having been given, and faradisation having been repeated four times more, the patient could be discharged cured. I saw her in May last, when she told me that she had continued perfectly well ever since.

Case 3.—A hard-working merchant, aged 38, first consulted me in June, 1865, for a troublesome form of dyspepsia, from which he had suffered for many years past. He complained of a feeling of heaviness and oppression after meals, especially after breakfast, coupled with eructations and flatus, which latter gave rise to acute pain until they were discharged. The bowels had been habitually costive since a journey the patient had undertaken to the west coast of Africa, in 1857, where he had suffered from a bad form of dysentery. Ever since that time he had been obliged to "assist the bowels" with purgatives. He had also used enemata of various kinds, but the rectum seemed to resent them, and the patient had taken a great dislike to their use. The tongue was dry and furred. The patient had lost flesh lately in consequence of being compelled to restrict his food to the smallest possible quantity. The urine was clear; the motion dark, hard, and ill-formed.

I carefully regulated the diet, and prescribed a tumblerful of Eger water night and morning, and five grains of carbonate of bismuth twice a day after meals. Digestion and defecation soon improved under this treatment, so that the patient was able to take more food than before, and he felt and looked much healthier and stronger than he had done for a considerable time past. About two months after I first saw him, business obliged him to return to Africa, and I did not see him again till March, 1866, when he was very much worse than he had ever been before. He had been obliged in the interval to take strong purgatives habitually, for ensuring sufficient action of the bowels. Indigestion and costiveness were now more troublesome than ever. The patient was emaciated and exhausted to the last degree. He was unable to do any work or to take any exercise, and refused all nourishing food, so that his family were extremely alarmed about his condition. Under these circumstances, an energetically tonic plan of treatment appeared indispensable. I prescribed liquor arsenicalis, with vinum ferri, a dose of pancreatic e-mul-

sion in rum and milk twice a day, a compound rhubarb pill, with the twenty-fourth part of a grain of strychnia at bedtime, and faradisation of the bowel twice a week. Under the influence of this treatment the patient rallied wonderfully. The beneficial effects of faradisation in inducing a healthy action of the muscular coat of the bowel were well shown by the circumstance that there was always a very good motion the evening after the application of the electricity, while on those days where it was not applied the action was rather sluggish. At the end of a month the aperient pill was discontinued; the arsenic was taken for another fortnight, and the emulsion for a month more. Faradisation was discontinued at the end of three months, when digestion and defecation were normal. Ever since that time the patient has been in good health, and taken no medicine whatever.—*Lancet, Nov.* 16, 1867, *p.* 607.

52.—FLATULENT DISTENSION OF THE COLON, WITH OR WITHOUT DIARRHŒA.

[We take the following short extract from a paper by Dr. Habershon, of Guy's Hospital, on "Functional Disease of the Colon."]

To check the flatulent distension of the colon from gaseous evolution, creosote and carbolic acid hold a deservedly prominent position; and when there is also diarrhœa, *carbolate of lime* may be given internally with marked benefit. The carbolate of lime as prepared by Mr. Squire, of Oxford-street, is perfectly white, and the smell is not so strong as the ordinary carbolate of lime used as a disinfecting agent. I have generally given it in one-grain doses combined with henbane; and I may remark that in the diarrhœa of phthisis, where there is evidence of fermentative changes in the colon, I have repeatedly used the carbolate of lime with good effect.—*Lancet, Jan.* 4, 1868, *p.* 8.

53.—DISEASE OF THE GUMS.

By —— Hilditch, Esq., Brighton.

The teeth should be washed night and morning, a moderately small and soft brush being used; after the morning ablution, pour on a second tooth-brush, slightly damped, a little of the following lotion, and apply it to the affected parts: Carbolic acid, one scruple; rectified spirits of wine, two drachms; distilled water, six ounces. Lister's brilliant inductions gave me the idea of using carbolic acid in these cases; by it suppurative action is kept under, and the gums soon get firmer and less tender.—*Lancet, March* 14, 1868, *p.* 365.

ORGANS OF URINE AND GENERATION.

54.—ON URINARY DISEASES.

By Sir Henry Thompson, Surgeon to University College Hospital.

[In forming a diagnosis in a case of urinary disease, and this indeed applies to most other diseases, we must adopt a pretty uniform mode of interrogating the patient. Our object is to collect *facts*, and our diagnosis consists of the inference which we draw from these facts.]

First of all with regard to questions. You may make out most cases of urinary disease—say five out of six—by four simple questions, including the minor extensions which arise out of them. I always ask the patient these four questions, and in the following order. The first is, "Have you any, and if any what, frequency in passing water?" Then, as a branch of that question, springing out of it, I ask whether the frequency is more by day or

by night, or influenced by any particular circumstances. How the question applies I will tell you afterwards. Then, secondly, I ask whether there is pain in passing urine, and whether *before*, or *after*, or *during* micturition; and whether at other times also, and if produced or aggravated by quick movements of the body. The locality of the pain is also to be ascertained. Then I ask as a third question, "Is the character of the urine altered in appearance? Is it turbid or clear?" Possibly the patient will tell you that it is turbid; but you find, on questioning further, that it was passed perfectly clear, and only become thick after cooling or standing. The fourth and last question is—whether blood has passed in any way, whether it is florid or dark, whether passed at the end or at the beginning of making water, or whether independently of micturition altogether; and such-like supplementary inquiries. These are the four questions, and let me remark that the answers you get will depend very much upon the way in which you put the questions. The patient is not always self-possessed, or he does not clearly understand the nature of the question you put. It is necessary to be very precise and very distinct in your questions if you wish to get accurate answers. In fact, there is no such difficult thing in all experience, whether in our profession or out of it, as to arrive at facts; and let me remind you again, that diagnosis consists of the acquisition of *facts*, and that it is impossible without them. Now you will say, how do I apply this to the list of diseases before you?

First, as to the frequency of passing water. These is no serious affection of the urinary organs, except one or two which I will name, in which you have not more or less frequency of passing water. For example: A man may have stricture to a considerable extent; the stream may be very narrow, and he may not for some years complain of frequency of passing water. I have classified these diseases, as you see, so that we may deal with them more easily. First of all, there are the inflammatory diseases—inflammation of the urethra, of the prostate, and of the bladder. In all these you have frequency in passing water. Not necessarily, however, in urethritis, until it reaches the distant part of the canal near the bladder. I do not propose to enter upon the subject of urethritis here, as you have frequent opportunities of studying it in the out-patient's room. I am now only referring to this symptom of frequency of passing water as existing more or less in all these three diseases at some time or another. In hypertrophy of the prostate you have it, and it is remarkable that it is more at night than in the day. In chronic prostatitis it is usually present to a small extent; in cystitis it is, of course, a characteristic symptom. In calculous diseases it is prominently met with, and generally its degree is in proportion to the amount of movement permitted to the patient. Tumours, of course, malignant and non-malignant, are attended by the same symptom. In pyelitis, and in almost all organic changes of the kidney, in Bright's disease, and in diabetes, there is frequency of making water. Whenever the natural characters of the urine are altered before it reaches the bladder, the secretion produces irritation. The fact is worth dwelling upon for a moment. Diluted or watery urine is often regarded as unirritating; on the contrary, it is not generally well retained by the bladder. The bladder is never so content as when it contains a urine of average, or more than average, specific gravity. Some persons, hysterical patients for example, will pass urine which is quite pale, almost like natural water, and the bladder is always more or less irritated by it. Of course in diabetes you have not only the character of the urine altered, but the quantity much increased.

The next question has reference to pain; and when you get answers as to the nature and seat of pain you will begin to see your way towards a diagnosis. In prostatitis there is usually pain at the end of passing water —less severe, but resembling somewhat that of stone; as the bladder contracts, when empty, on the tender prostate. In cystitis the pain is usually before micturition, because the inflamed bladder is sensitive on being distended, and is anxious to get rid of its contents. The pain is just above *the pubes*. When cystitis is acute, pain may be felt in the perineum also;

but in chronic or subacute cystitis it is supra-pubic, and not at the end but at the beginning of making water, unless the prostate is affected, and then the tender prostate gives a little pain at the end.

In stricture of the urethra there is often pain about the seat of the obstruction, an idea of which you may obtain by a simple experiment. If, when passing urine with a full stream, you suddenly narrow the passage with your finger, so as to diminish the stream one-half or more, you will experience an acute pain.

There may be pain with hypertrophy of the prostate, inasmuch as this is frequently associated with chronic cystitis, when the pain is before making water, and not afterwards—differing in that respect from stone. The bladder wants to get rid of its contents, and can do so but slowly, on account of the enlarged prostate, which stands as a barrier in the way. During its first contractions, which expel but little urine, there is pain above the pubes and deep in the perineum; but when a third or a half of the contents has issued, the patient is relieved.

I shall not dwell upon calculous disease of the urethra. The calculus is only a temporary lodger there, and as it can often be felt externally by the hand there is rarely any difficulty about the diagnosis. Calculous disease of the prostate is also rare. I shall not complicate what I wish to be a simple matter by dwelling upon it, but call your attention to the commoner condition of calculus in the bladder. Here the pain is quite distinct in its character; it is felt at the end of passing water, because the bladder being emptied, the rough surface of the stone is left in contact with the mucous membrane, doubtless that covering the neck of the bladder, which is unquestionably a sensitive spot. As soon as sufficient urine has trickled down into the bladder to separate the coats from the stone relief is obtained. Then the pain at the end of the penis, within an inch of it, about the base of the glans. Furthermore, the pain is increased by movement; in other complaints it is not necessarily so. Put a patient in a rough-going vehicle, or make him jump from a step, or perform any rapid movement, and instantly he feels severe pain, probably at the neck of the bladder, but also at the end of the penis. In prostatitis, inasmuch as the neck of the bladder is involved, there is usually some pain at the end of the penis, which is a reason why chronic inflammation of the prostate produces symptoms much resembling those of stone.

With regard to calculus of the kidney, I have little to say about it here. Of course you have pain referred to the locality, right or left, not often to both kidneys; there is tenderness also, and much increase of pain on movement. It is usually on one side only, and perhaps more frequently on the left than on the right side.

One cannot, perhaps, say much about any characteristic pain in connection with tumours. They may be situated in any part of the bladder; may obstruct the urine more or less, and accordingly as they produce cystitis and obstruct the flow of urine pain will be experienced.

The next question is as to the character of the urine itself. Now, suppose your patient has told you that he has frequency in passing water, pain at the end of the penis and at the neck of the bladder, and that the pain and frequency are aggravated by movement. You may begin to imagine, "Perhaps the man has stone in the bladder, and I shall have to sound him." Two questions only have already put this probability in your way, and you interrogate as to the character of the urine. See how this carries you a step further. We recommence our list as to this inquiry. A preliminary remark, however, about examining urine. I do not propose to teach you here a systematic mode of doing this. It is not in my department, and would only be repeating that which it will be your duty to learn elsewhere, and I hope you will do so thoroughly. But there is this hint which I may give with respect to it. Whenever you want a specimen from your patient to examine, do not tell him to send you a bottle of it passed in the usual way, or you will get a mixture of often doubtful value. What you require is the secretion of the kidneys, plus only anything there may be in the bladder; you do not want

mplicated with anything which may come from the urethra. Let the pass two or three tablespoonfuls through the urethra first, or as ep out whatever may be there, which may be put into a separate vessel. then you will get a specimen—at any rate one of which you will know source. You will have the renal secretion plus anything in the bladder. ppose the man has gleet or chronic prostatitis: there will then be a quan- y of muco-purulent matter in the urethra. If all this be carried into the essel with the urine, how will you determine the different products, and de- de by the microscope or by the eye what has come from the urethra, what om the prostate, and what from the kidneys? You cannot do it; but if ou get rid of the source of error by flushing the urethra, so to speak, and mptying the contents into a separate glass—say a wineglass,—then taking he bulk or remainder in a tumbler, you will generally have a sample of urine that you can do something with. If I felt disposed to indulge you with gossip, I could tell you stories of the gravest blunders committed by not at- tending to that simple point. I can at all events tell you that I have more than once known a learned practitioner treat a patient for pyelitis who had nothing but a profuse discharge from the urethra: how the urine had been sent twice a week in a scrupulously clean bottle, and because a quantity of pus was found in it, the patient, who had some symptoms corroborating that view, was treated during some months for pyelitis: how a surgeon at length found out that the whole of the matter came from the urethra, so that when the urethra was flushed into the first glass all the matter was there, and that the remaining urine was clear and healthy: and, finally, that the "pyelitis" soon disappeared under local treatment of the urethra. I do not know whether anyone else may tell you of that simple mode of determining this matter. I am always calling attention to it: and I will assume that in the future you will none of you make such a mistake as that I have mentioned.

Referring first to prostatitis, it is always associated more or less with shreds in the urine, which come from the prostatic part; and if you separate the urine as I have told you, you will find that the whole of the thick matter will be in the first glass, while what remains behind will be clear. How would it be with regard to calculus? You might have muco-pus in the first glass, but you would have more in the second from the bladder. Scarcely ever have you calculus in the bladder without having some muco-pus from the bladder itself. Occasionally, but very rarely, do I find a man with stone in the bladder having perfectly clear urine. Not commonly do I sound a man for stone who has clear urine, unless he has marked symptoms, because the presence of stone in the bladder almost always gives rise to a certain amount of cystitis, and there is deposit in consequence. If the patient passes shreds of thick matter in the first glass, and the urine left behind is clear, and he has symptoms like those of stone, rely upon it he has chronic prostatitis.

The character of the urine in chronic cystitis is well known. There is at the bottom of the vessel a thick mucilaginous deposit, which does not issue in a stream, but falls out in a mass. In acute cystitis, the urine is cloudy, and there is a considerable deposit of pus. In stricture of the urethra, unless chronic cystitis has been set up, there is no deposit from the urine. Here the character of the stream is important. If, when the patient is passing urine, you see a very thin, small, spluttering stream, or urine issuing only in drops, you will know that there is an obstruction, most likely stricture; because, although in hypertrophy of the prostate you may have the stream much diminished, it will be a stream which falls downwards from the organ. In stricture, force may be brought to bear on the stream, so that, however small it may be, it is often fairly propelled; but in hypertrophy, in which the expelling apparatus is involved, the muscles cannot act, and therefore, however large or small the stream, it generally falls perpendicularly. In cancerous disease of the bladder there is nothing to note about the nature of the stream. With regard to the débris of tumours found in the urine, the microscope sometimes, not often, throws light upon their nature. No doubt you may see cancer-cells in the urine, but it is most difficult to identify them. I have seen such cells declared to exist by good observers in cases

in which cancer was not present. Young pavement epithelium is easily mistaken for them. Going upwards from the bladder, we may note pyelitis, more or less chronic—a disease in which the condition of the urine is only one symptom among many others which must be observed before arriving at a conclusion.

The next question is, "Do you pass blood?" and this will bring you very near indeed to an opinion on most cases—not quite, because in any case you may require to sound. In prostatitis there is often a little blood at the end of micturition, as in stone; in cystitis there is not necessarily blood, unless it is acute and far advanced; in stricture of the urethra there is not necessarily blood, and in hypertrophy of the prostate not necessarily. You may have it or not, often only as the result of instruments. It tells most in the question of stone. Just as in phthisis, a large proportion of patients have hæmoptysis at some time or another; so in about the same number of cases, say four out of five, there is some blood with vesical calculus.

I want you to pay particular attention to these questions, because they underlie much of what I shall have to say. What I have to add with regard to observation by the eye, by the hand, and by instruments will come under each particular subject hereafter, and I will only briefly allude to it to-day. By the eye you observe mainly whether the bladder is distended or not, and you are assisted by palpitation and percussion. You examine the perineum and scrotum also, with a view to extravasation of urine, perineal abscess and fistula, &c. And now we come to the question of instruments. Suppose such a case as that to which I have already referred, in which there is frequency in passing urine, pain at the end of micturition, pain on any considerable movement, thickening of the urine, blood passing occasionally, but more on movement, you regard it as highly probable that the man has stone. You cannot arrive at a certainty without instruments. You may have almost all these conditions in certain changes of the kidney and in renal calculus, and you cannot distinguish them unless you skilfully explore the bladder with a sound. When I claim great value for this instrument, quite understand that I am by no means desirous that in the case of every patient who comes to you and complains of some frequency in making water, or pain in the act, you should say, "Lie down and let me pass an instrument." Perhaps the surgeon may be apt to abuse a little his power of passing instruments; it should never be done unless it is absolutely necessary. I hold that an instrument, *per se*, is an evil—a very small one, or a considerable one, according to the manner in which it is employed—and is never to be used unless there is good reason to believe that a greater evil is present which it may mitigate or cure. But when your patient has the symptoms named, you will be doing him an injury unless you resort to it. In cases of stricture the instrument is also necessary; and so in ascertaining the condition of the bladder, whether it is full or empty. A man may make water very frequently, strain hard, and be very certain that he has emptied the bladder, and be quite deceived. How can you tell? There may be a tumour stimulating a distended bladder. You cannot know whether the bladder is emptied unless you pass an instrument. Many a man has had an instrument passed into the bladder, and a quart of urine has been found to be left behind, when his own sensations led him to believe that he had expelled every drop. We shall see more of this when we come to the question of retention and hypertrophy of the prostate.

As we are now on the question of diagnosis, I take the opportunity of showing you that the eye may be assisted to a certain extent by what is called the endoscope, which is simply an instrument that we have long been in the habit of passing into all the cavities of the body—the ear, the vagina, the rectum—for the purpose of bringing reflected light to bear upon the interior of those cavities. For some years it has been attempted to do the same with the urethra. It is sixteen years ago since I first saw the endoscope applied to the urethra. This was in the hands of Mr. Avery, of the Charing-cross Hospital. As I was turning my attention somewhat to this subject, he asked me to see some of his patients, and a new instrument he was then making.

He showed me a long tube, precisely similar to this which I hold in my hand, with certain arrangements enabling one to see deep portions of the urethra. He showed me cases of stricture, but I do not think he looked into the bladder. He paid a great deal of attention to the subject, and the instrument was brought by him to a certain state of perfection; unhappily, however, his death occurred shortly afterwards, and the thing was lost sight of here. Various attempts have been made with the same object, long before and since, but I do not know that there is anything on this table very superior to what Mr. Avery showed. Within the last few years M. Desormeaux, of Paris, has paid great attention to the endoscope, and has perfected one of his own, consisting of a similar tube, but with different appliances. The various modes in which light is applied constitute the differences between the various kinds of endoscope. In all of them there is a tube of this description to pass into the cavity. Five years ago I had an endoscope of M. Desormeaux's, and exhibited it in the hospital—the instrument which you see here. Dr. Cruise, of Dublin, has brought it to greater perfection, and has produced a better instrument than we heretofore possessed. I may tell you at once, that if a man has a good and a tolerably practised hand, with a fair share of intelligence, I do not think he will gain a great deal by the endoscope; and if he has not, I think it will be of no use at all. There are some few cases in which he may find it of value; but do not expect that the endoscope is going to work any marvels in the diagnosis of surgical diseases of the urinary organs. In nineteen cases out of twenty you ought to be able to arrive at the necessary information without it. And it is not the easiest thing in the world to apply. As already remarked, a man should not be put unnecessarily to the pain and inconvenience of a sound or a catheter; but examination by the endoscope is a somewhat more irritating and tedious process. In certain exceptional cases, in which you are unable to arrive at a conclusion without it, you may employ it to some advantage. This patient before you had an exceedingly bad stricture of the urethra, which I cut internally last Tuesday week. He is now perfectly well. He could not pass a drop of urine before the operation, but now he is able to pass it naturally; and you will agree with me that a great deal must have been done last Wednesday week to make that change. I cut through the strictures deeply, and now we shall see whether we can find the cicatrices. I shall use Desormeaux's endoscope, illuminated by Dr. Cruise's lamp. You see we have now made a careful and prolonged examination. The urethra is of a more dusky red about the part which has been affected, but that is all which can be observed. Changes in the colour and texture of the mucous membrane of the urethra and bladder are those which are most easily seen, and which are of the most importance to note. The orifice of a stricture may be sometimes seen, but the result is without practical utility. A stone in the bladder may be easily seen, or rather the small portion of it upon which the end of the sound impinges; but I have never gained anything by the sight. A calculus smaller than a pea may be easily found by delicate sounding—an audible note elicited from it; more easily than you can see it through the endoscopic sound. I may mention that no one has yet been able by its means to identify the veru montanum, and if you cannot see the veru montanum I think it is quite possible that minute pathological changes will often escape you.

Mr. Baker has been good enough to send up another and very simple endoscope, designed by Mr. Warwick, for exhibition. It may be used with ordinary gaslight or with sunlight. It certainly seems, on comparison, to effect nearly as much as the larger and more elaborate instrument.

Stricture of the Urethra.—It is, of course, assumed that in treating a case by means of dilatation, whether continuous or simple, the instrument has fairly passed through the stricture, otherwise the stricture is not dilated. But all cases of stricture are not so easily disposed of. It often happens that at the first, second, or even third trial you do not get the instrument into the obstructed part, or only partially so; or it leaves the canal altogether and *goes into* a false passage. At all events the instrument does not go through the *stricture*, and onwards, as it should do, into the bladder. Now that is a co[illegible]

dition of difficulty which opens a new subject for us to-day. We have now to deal with more difficult cases, those in which all your care, and experience if you have any, are needed. It was said by Liston, that of all operations in surgery there were none so difficult, none which required so much patience and care, as the passing of a catheter in an obstinate and very narrow stricture. That statement is in his last edition of the "Practical Surgery" (p. 476), and you can scarcely have a higher authority for the fact there mentioned.

Now there is one word which is often used to describe stricture, to which I take a great objection, and I think the sooner it is expunged from the vocabulary of surgery the better. Such a stricture as that of which I now speak is often said to be "impermeable." What is impermeable stricture? Why, first, it does not denote a quality necessarily inherent in the stricture at all, but rather the quality of the surgeon. For you see, a stricture may be "impermeable" as regards A. but not "impermeable" as regards B, who may pass the instrument easily enough. But secondly, it is a contradiction in terms. Stricture is a *narrowing* of the canal; it is not an obliteration of it. There must be an opening, and if there be an opening, there must be room for an instrument; it cannot be "impermeable." It is only a question of the size of that instrument, and of skill or patience in the management of it. The stricture always admits urine, more or less in quantity, to pass through it, and I maintain the truth of the axiom first enunciated by Professor Syme, that whenever urine passes outwards through a stricture, an instrument ought with care and perseverance to be got in. I advise you to believe in that doctrine, not that it is true as regards yourselves at the present time, for I will assume that you have not sufficient experience to be able to pass an instrument through a stricture in all cases. If you are able to do so I can only say that you are quite out of place here, and need not come to learn. It is exceedingly difficult to pass an instrument in some cases, but after considerable amount of experience you will find that there are very few in which it cannot be accomplished. In the treatment of stricture, when you have really a difficult case before you, it makes all the difference whether you act under a belief that it is your own fault if you do not succeed, or whether, on the other hand, you hold the dogma that there are a certain number of cases which are "impermeable" to all surgeons. The man who holds the latter belief will be quite certain in some cases not to succeed, whereas, probably, the man who believes that in all cases an instrument may be passed with time and patience, will be very likely to succeed in all, or at all events will succeed better than the other. "Impermeable" stricture is not heard so much of now as it was twenty years ago. It was fully believed in then, and operations were frequently seen in the hospitals for it; but I will undertake to say that they are very rare now. The operation consisted in passing a large instrument down to the stricture, and opening the urethra upon it from the perineum, and then getting through the obstruction, or by it in some way, if the operator could, into the urethra beyond. It was very seldom that the true passage was followed, but somehow, by dint of cutting, a way was made for the instrument to go from the urethra before the stricture, to the urethra behind the stricture, and it was not a very successful proceeding. That was acknowledged in all books at that time as an operation for impermeable stricture, or as "perineal section." I have never had occasion to perform it, and I believe the necessity for it, if ever existing, is excessively rare. I have already given Professor Syme credit for having first enunciated that doctrine, and he has stoutly maintained it, to the great advantage of patients who suffer from stricture.

But you may have complete obliteration of the urethra, which, as before said, is not stricture. This is very rare: but it occasionally happens, and chiefly after injury in the perineum, as by the breaking of a vessel, or any wound there cutting into or across the urethra. If the opening remains pervious, and gives exit to all the urine, a cicatrix occurs involving the anterior opening of the divided urethra, which is then closed altogether, and so the canal is obliterated in front of the fistula,

Now how *are you to deal with* a really narrow and difficult stricture?

will assume that you have a case in which you have tried to pass an instrument three or four times, and have failed. The first thing to do is to see the patient make water. Your failure to pass the instrument may not have arisen from narrowing of the urethra; there may have been some false passage made. It may be that there is no stricture at all. No greater failures are made than with those patients who have little or no stricture, either from the surgeon not knowing well how to pass the instrument, or from there being a false passage into which he gets, so that he does not enter the bladder at all. You first of all see the stream of water, and judge by that what size of instrument is to be used. And always let the instrument correspond with the size of the stream which you see. It should correspond in this way, however,—it should be a little smaller. You know, of course, that when a current of water passes into a narrower passage than that in which it has been flowing, the stream is more rapid than before, and when the passage becomes larger it flows slowly again; so that the size of the stream as it flows from the orifice is not to be taken as the precise measure of the calibre of the narrowest part of the canal. You should, therefore, take an instrument somewhat smaller than the stream. There is a patient in No. 10 ward, whom some of you have seen, who does not pass a stream at all; the urine is only a succession of drops. How small then, in such a case must be the instrument that is employed. Then there is one very important thing to be considered in connexion with small instruments—namely, that no more dangerous weapon can be introduced than a very small catheter, unless corresponding care, delicacy, and gentleness are exercised; indeed, it is impossible to be too careful in employing it. You see how easy it is with such an instrument as that which I hold in my hand to get into one of the lacunæ or into any false passage, or to drive it through the soft walls of the urethra into the tissues outside; therefore it should always be used with the utmost lightness of hand. You must not hold this slender catheter tightly, resolving to get it through any obstruction; but it must be held so lightly that if it meets any undue resistance it will slip through the fingers directly—anything rather than wound the urethra. I certainly cannot advise you to try such an instrument until you have had some fair amount of practice with a larger one. In cases of difficulty, a small gum instrument is often not of great service. I have been advocating gum-elastic instruments as the rule; but if you have to deal with a very tight stricture, and fail to pass the flexible catheter after one or two trials, you must use a small silver instrument.

Now, one word upon the subject of using force. *Under no circumstances whatever should force be used in the introduction of an instrument through a stricture or into the bladder.* That is my dictum to-day. Years ago it was matter of discussion how much force should be employed; more years ago still you heard of people using instruments with great violence. Now I am perfectly satisfied, and I believe all modern surgeons will tell you, that no force is to be employed. It is very difficult to say what one means by force; but what you are to understand is, that no kind of weight or pressure is to be put on the instrument which can by any possibility carry it out of the canal; and very little force will do that. The more difficult the stricture the less are you to dream of using force. You will remember that the urethra is possibly of full size up to the point of narrowing, and hence it may be very difficult to find the opening. If you use force, you will perhaps perforate the soft walls on either side, and this done, you have increased your difficulties; because if you make a false passage, the point of the instrument is much more likely to be caught in it than to go through the strictured part.

Remember, also, that a stricture in the course of a canal does not necessarily follow the exact direction of that canal. It may be a little tortuous; may be on this side of the axis or on that, not necessarily in the middle. You may see this sometimes in the dead body; and you may infer it from experience on the living. When you have to deal with a very narrow stricture, take a small instrument which you intend to guide. Do not rely upon mere *groping* to find the orifice. First of all it is exceedingly dull work to be constantly groping. You should always adopt some method—any method

you please, provided that it shall seem to you exhaustive of the different modes of exploring the urethra. Some of you saw the plan I adopted with a patient to-day; and I believe that to be the best plan. You should go systematically to work, and slide your instrument from the orifice of the urethra down the one side or the other. This diagram will show you what I mean. If I go down on this side I shall probably not get into the stricture; but if I slide down gradually on the other I shall probably get the instrument in, because you see there is less obstruction on that side. Begin, then, by the roof. The roof is the firmest part, and, by following it, will be most likely to carry the point in. The floor, on the contrary, is the softest, loosest, and most spongy part: and will be most likely to yield to the instrument, and give way. If your first effort does not succeed, take the right side; if that does not do, take the left; if that does not do, take the floor. I know no other method so calculated to help you through a difficult stricture. If you are very careful, you may make the attempt in this manner for twenty or thirty minutes without doing any damage; but if you find the patient suffering much, if you are losing patience, give it up, or you will probably make a false passage, and increase the difficulty tenfold.

[Speaking of the diagnosis in cases of stricture, the writer observes:]

One rule is to be invariably followed: whenever you examine for stricture, take a fair-sized instrument, say No. 8 or 9. The patient may remonstrate, and will probably say, "That wont pass; it is useless to employ so large an instrument." Tell him that you do not propose to pass it, but only to discover where the obstruction is. For if you use a small instrument at first, it may pass through the stricture, if one exists, without detecting it; but if the large instrument goes on easily into the bladder, you have the satisfaction of telling your patient that he has no stricture, and you look further for the cause of the difficulty. Whatever, then, a man may tell you, and however small the stream may be, take an instrument not less than 8 or 9, pass it gently down the canal, and if there is a stricture the instrument is arrested, and you will find the exact position of the obstruction. Notice the point where it exists—four inches, or five, as the case may be, because it will be useful afterwards in dilating to know its exact locality.

Now, in passing the instrument, you may meet with circumstances which may mislead you. I have spoken to you of error on the part of the patient, and I am bound to say that the surgeon who is not much practised in these matters may also be deceived. What are the sources of fallacy to which he may be exposed? How is it that he sometimes fancies there is stricture when there is not? There are some kinds of practice in which you may be hereafter placed that do not afford the opportunity of often seeing this disease, and in such it is no great discredit to a man to think that he has found stricture where none exists. Not, of course, if he is a professed surgeon; then it will be a great discredit to him. But if he has very little to do with this kind of thing, he may encounter some difficulty in the urethra, and he may suppose, but erroneously, that it is due to stricture. Now, I want to guard you all against this; for though you may not all be operating surgeons, I want you not to leave any course of lectures which I may give, without knowing precisely what are the sources of fallacy, so that I may not hear of any of you hereafter making such a mistake as that to which I have referred. Let me remind you again of what was said about the urethra being a closed canal. I am not going to draw you a diagram of the urethra such as you see in anatomical books in which it is represented in section as an open passage; for it is never in that condition except in the act of micturition. First, close to the meatus is a source of fallacy—I mean the lacuna magna. Then five or six inches further on the bulb joins closely the membranous portion, and the canal, from being wide or dilatable, becomes very narrow. Lastly there is a source of difficulty at the neck of the bladder. Those are the three points at which persons may be mistaken in passing an instrument, and form erroneous notions in consequence respecting the presence of stricture.

Now whenever you pass an instrument, do not let your thoughts revert to those anatomical *diagrams* representing the urethra as an open tube; bear in

mind that it is simply a closed passage, so that nothing is easier in passing an instrument, if you are out of the line, than to find some obstruction in the folds or lacunæ of the mucous membrane, since it has more distensibility at some points than at others. Of course if it were simply an open tube there would be less difficulty in going on, but, as it is not so, the point is liable to hitch against the soft parts on one side or the other. First, as I have said, it is quite possible to get stopped at the very outset, which is embarrassing to a beginner, by engaging the point of the instrument in the lacuna magna. Whenever, then, you begin to pass an instrument, let your first thought be to keep on the floor, so as to avoid that obstacle. You wish, of course, to pass it well for the patient. Perhaps he has had instruments passed before, and you desire to succeed at least as well as the preceding operator. Now there is nothing which a patient appreciates so much as the easy passing of an instrument. It is a disagreeable operation, and if you pass it more easily than other persons, you will probably retain your patient as long as he requires assistance of that kind. If you stick and get into the lacuna magna at the outset, he infers you to be a bungler, and, perhaps, will not come to you again.

Now you see represented in this diagram the bulb of the urethra. The canal is more distensible at this point, and when you come to the deep perineal fascia the canal is much less distensible. Practically, therefore, it is much wider there than it is at the orifice, and when you get the instrument at that point it is apt to hitch. This is the place where almost all the false passages are made; the instrument is driven out of the canal down below the urethra, it being mainly at the floor that the canal is so distensible. The section of the corpus spongiosum is wider below than above; the texture is soft and spongy. The urethra corresponds in distensibility to the soft structure outside, and although the instrument gets smoothly down to this point it may not go on into the membranous portion. Now take care, at first, to get the point of the instrument so turned up as to avoid this lower part. Nothing is so good as a well curved instrument to avoid that obstacle. I am in the habit of doing this in the case of out-patients. I like to take a student who has never passed an instrument before, and I say to him, "Pass this bougie (a straight or slightly curved one) into the canal." He passes it, and invariably when he arrives at the membranous portion stops. I then take the same instrument, give its point a curved form, then it passes immediately into the bladder. As the instrument goes in it keeps close to the roof, instead of engaging itself in the distensible part of the bulb.

The last obstacle is at the neck of the bladder, and so common is it, that you often hear of "stricture of the neck of the bladder"—a thing which never exists. There never was a stricture even in the prostatic portion. "Stricture at the neck of the bladder" was a household word some years ago, and even now you sometimes hear of it; but there is no such thing. It is simply because there is sometimes difficulty in passing the neck of the bladder that it came to be regarded as a locality of stricture. In this case also a well-curved instrument is the best thing to go in. Let me recapitulate shortly the three sources of difficulty: First, the lacuna magna, which is avoided by keeping on the floor of the canal; then the narrow membranous portion at the bulb, which is avoided by keeping the point of the instrument well up; and the same with regard to the neck of the bladder.

I was going to read to you the notes of some cases of stricture in the ward upstairs, but I will not do so, because you have seen the cases, and know pretty well what they are; and I shall only refer to some of them as we go on. One has been treated by continuous dilatation, and another by internal cutting. Then we have several patients treated by common dilatation, and it is of common dilatation that I have to speak to you to-day. Suppose that you have passed a full-sized instrument, and found that it stops unmistakably, say, at about five inches, the most usual place; you will then, of course, *have to* pass smaller instruments to see what will go through it. You have *diagnosed* the fact of the stricture, and the situation; but you do not know *the calibre*. You ask the patient to make water. If he cannot do so, take

a small gum-elastic instrument, say No. 1 or 2. You pass No. 1, and that perhaps goes through easily; then No. 2, which goes not so easily; then No. 3, which goes in tightly. No. 3, then, is the calibre of the stricture. Now you have all the main facts about the stricture that you want to know. The man's general condition, the frequency with which he makes water, the amount of chronic cystitis—all these are questions to be considered; but it is only with the mechanical part that I have to deal to-day. Lastly, we will assume that he has only one stricture; for a patient may have two, but this is exceptional. Now, what is the treatment? First and foremost, dilatation—dilatation always—dilatation without exception, whenever it will succeed. It is always to be tried first, because it is the simplest and easiest mode. If you find a man with a stricture ever so narrow or tight, by no means think of operating till you have tried whether dilatation will succeed. What is dilatation? A mechanical process of stretching this organised lymph, which forms bands round the canal at the strictured point. Now, suppose you have been able to pass No. 3 bougie or catheter rather tightly through the stricture and into the bladder, to be perfectly sure that all has gone right; then you say to the patient, "That is enough for to-day: come again in two or three days' time for a larger instrument." Now I advise you on such second or third day not to commence with the largest instrument previously passed. Having passed Nos. 1, 2, and 3 on the first occasion, you should now take Nos. 2, 3, and 4; and on the third occasion 3, 4, and 5; and so on; always beginning below the point you had attained on the previous occasion, making the smaller instrument a sort of *avant courrier* for the larger one. Further, never let the instrument remain in the urethra; withdraw it at once: leaving it there simply increases irritation, and does not augment in the slightest degree the dilating power.

Now comes the question of the kind of instrument to be employed. The great principle which underlies all mechanical treatment of the urinary organs, whether for stricture or for hypertrophied prostate, for retention of the urine or for stone—the one great principle which must decide for us the question of the kind of instrument to be employed, is this: *all instruments are to be considered evils, more or less, never to be resorted to unless a greater evil be present.* The passage of an instrument of any kind into the healthy urethra must, *per se*, be a source of irritation. Try it yourself; and I advise you to do it, if you wish to pass an instrument well; for I hold that no man should pass an instrument for another until he has passed one for himself. Of course the amount of irritation will depend in great part on the manner in which it is passed, and on the kind of instrument employed. Let us consider, to use a commercial simile, that in the case of your patient there is a "debit and a credit side" in all treatment. You intend, beyond all doubt, to effect some real good—that is to the "credit side" of the account; but you cannot do it without making some irritation in order to gain your end—that is an entry on the "debit side." Be careful, then, that you constantly bear in mind the latter fact, and make it your business to diminish that "debit" as much as possible. Do not pass an instrument unless there is some good reason, unless there is some evil, for the sake of curing which it is worth while to incur a little irritation. Acting upon this principle, you will choose such an instrument as you know by experience or otherwise to produce the least possible irritation. And this leads me to the question of difference between solid and flexible instruments. Here I feel that I am treading on delicate ground; and I will tell you why. First of all, no one has been a greater advocate than I was some years ago of solid instruments as against soft ones, influenced by the traditions of this place, which are entirely in favour of the former. I can give you the reason for that. The great master-spirit of this place, who has been dead some twenty years or more, the man who gave the tone to the place, and educated almost all the elder men here—I mean Liston,—declared his preference for the solid instrument in very strong terms. It is just twenty-one years ago since I sat in this room and heard him deliver a lecture on that very subject. His powerful advocacy of the silver instrument, and the contempt he had for others, were matters of noto-

riety. Starting, then, with such views, and regarding him—as every one does to a certain extent the man who teaches him well and fairly what he learns—for a certain time as an oracle, I was strongly in favour of the solid instrument as against the flexible. But what is much more valuable than any oracle, whoever he may be, is a large personal experience; and this has taught me that, beyond all question, the flexible instrument is the best—if only you know how to use it—for the treatment of stricture, and for all maladies of the canal, whenever it is available. I am so certain of this that I have no hesitation in saying that a great part of the success of any man who has much to do with this subject will depend upon the use of flexible instruments as against solid. No patient will ever allow a surgeon to pass for him a solid instrument if you have passed for him a flexible one as easily as you are bound to do. It gives him so much less pain, and produces so much less irritation. To continue my commercial simile,—it puts so much less on the "debit side" of your patient's case; you get so much more of advantage, and so much less of disadvantage. I confess, then, to a considerable change of opinion even since I published my first work on the subject; and I do this without the slightest shame or the slightest repugnance. I hold that the end of life in this world is not achieved without change in our opinions. You may rely upon it, with regard to any subject whatever—whether politics, or religion, or our own proper profession,—if we hold the same opinions at forty years of age as we did at twenty, and, perhaps, looking forward, I may say, if we hold the same opinions at sixty as we do at forty, we live to very little purpose. It is an error to look for a life-long "consistency" in matters of opinion from men who think for themselves in whatever department their teaching may be. You must expect them to progress, or they will be bad teachers, just as I hope you are all progressing now. I have said this because I know that so much might be quoted from what I held fifteen years ago in contradiction to what I am now saying. If I did not state this, you might ask me why, having said so much in favour of the silver instrument, do I now say so much in favour of the other. You have my reason: it is simply because I have learned better.

There are two kinds of flexible instrument, the English and the French. Inasmuch as the French instrument possesses more flexibility than the English, I often prefer it. Perhaps it is right that I should qualify a little what I have said. I believe the flexible instruments are much better now than they were in Liston's time; and I think that, if we had had the good fortune to retain him so long (without taking credit for making a shrewd guess), he would have changed his opinion too. This is the kind of flexible instrument much used in his time. It is called a bougie; and properly, since it is simply a kind of wax candle, and is, in my opinion, a very imperfect instrument. You can bend it into any form by warming, but it is a very inferior instrument to what is generally used now. The gum-elastic or English flexible instrument is very valuable on account of one quality which it possesses, and which does not belong to the French instrument—*i. e.*, it will preserve any curve, when cooled, which you choose to apply to it under the influence of heat. If I want a small curve, I take the instrument, put it in warm water, give it the curve required, put it in cold water, and the curve is fixed or set. The French instrument is exceedingly flexible; you may wind it around your finger without difficulty. And it has another valuable character—namely, its peculiar tapering point. Now a tapering point *per se* is often undesirable in the urethra, because it is very liable to get into some lacuna. It would be an advantage if you could ensure that it did not do this, but this liability is provided against now very ingeniously by means of a little bulb at the end (see illustration). The long tapering extremity guarded by the *bulb insinuates* itself through the healthy urethra, or through one not greatly *contracted*, in a most certain and easy manner. Such an instrument as that *may be* passed by the patient himself without difficulty. Indeed, it may al-

most be said to be "surgery made too easy." The merest tyro can pass it in nine cases out of ten, although he might not manage the tenth. It is one of the most extraordinary instances of English conservatism that these instruments are found in so few hands. They are, however, at last being made here. For years it has been necessary to send to Paris for them; but a demand is arising now, and they are at length manufactured in this country. If you will try the experiment on yourselves, you will find the *bougie à boule* traverses the urethra almost without your knowing it; and it requires no knowledge of the canal in order to use it. Now it may appear to you very heterodox, but I advise you, in passing an instrument, to forget all about your anatomy. You are taught it over the way, and it is most important that you should know it; but in passing an instrument forget all about the different regions. Think nothing about the deep fascia, the membranous portion, or the compressor urethræ. A solid instrument is especially dangerous in the hands of an anatomist; he will push it the way he thinks right, as if all urethras were exactly of the same form, and did not vary as much as noses do, or other features. This used to be the pretext for preferring the solid instrument: it was said, "You want to know exactly your anatomy, and pass the instrument accordingly." I pity the patient who has a solid instrument thrust into his body by a knowing man at anatomy. You want an instrument that you can use most delicately, holding it lightly between the finger and thumb, withdrawing it or changing its direction as soon as you are able to perceive an obstruction. Your hand is to be educated for the power of delicately perceiving the characters of the passage by means of the instrument within it; and rarely, if ever, are you to push a solid instrument in any given direction preconceived to be the right one. If you wish to get the largest amount of dilatation and the minimum amount of irritation, this is unquestionably the instrument for you to use.

There is one other point to notice with reference to French and English instruments,—I mean the gauges. Our numbers are from 1 to 12. Here is No. 12; and you generally consider, when you have reached that size, that you have completed the dilatation. In England we cannot be said to have a uniform scale; all our measurements are very arbitrary. One maker has one scale, and another another; and the Scotch scale differs by one and a half from the English; so that the patient who takes No. 12 Scotch, takes only a 10½ English. Our more exact neighbours over the Channel adopt the millimetre, and the number represents the exact size, so that when I have named the size, I have named the exact calibre or magnitude of the urethra. Instead of 1 to 12, the French have 1 to 30. They begin lower, and go higher than we do, and the steps are more gradual. In this way irritation may be lessened. You may pass, for instance, a No. 4 English very easily and a No. 5 with great difficulty, or not at all, and an intermediate one might be the proper size. Their numbers 3 to 21 are about equal to our 1 to 12; showing you at once how much more gradual the range is. No. 1 is one millimetre in circumference; No. 2 two millimetres, and so on; so that the increase in size is uniform as well as gradual. If I have a patient who will take No. 21, I know that his urethra admits an instrument twenty-one millimetres in circumference, and of course seven in diameter. I advise you in this, as in other matters, to be cosmopolitan in your views, and to adopt improvements from all quarters.

I have told you that simple dilatation consists in passing every two or three days a larger instrument, until you reach the highest. In many cases all goes on smoothly from the beginning to the end. Then you teach the patient to pass the instrument for himself, and he does so afterwards once a month, or once in six weeks, to maintain a sufficient calibre.

I will touch but lightly on "continuous dilatation," or the tying in of the instrument. There is a patient upstairs, who is now undergoing it successfully. You have tried we will suppose, the simple dilatation, and have not made the amount of progress desired, or, perhaps, the patient's avocations may make it necessary to have a more speedy cure. In either case you may say, "If you can give me ten or fourteen days in your room,

not necessarily in bed, but on the sofa quietly at home, I can almost certainly bring you from the smallestn umber up to the highest,"—that is, by "continuous dilatation." In the first case the instrument is simply introduced, and at once withdrawn; in the last case you tie the instrument in, and allow it to remain for several days. You tie in a small catheter, which if possible is to be gum elastic, and so that it just enters the bladder. And you should always take care that it is small enough to pass easily, so that it lies loosely in the canal. These three conditions being granted, this is one of the safest and best modes of treating stricture. The patient upstairs has finished the process, and to-day the house-surgeon tells me that he has passed No. 11 with ease. The man has not now the slightest pain or frequency of making water, and he has not been so well as he is now for twenty years. He says he is as well as ever he was in his life; he came here in as bad condition as it is possible to conceive. He had been treated as an out-patient, and, making no progress, I advised him to come in, and try continuous dilatation. I repeat three conditions are necessary to success: You must have a flexible instrument; the point must not be far in the bladder, and it must not fill the stricture, because, remember, it is not a mechanical process; you do not want to distend the stricture as you would a lady's glove, but you let the foreign body lie in the passage. If you leave a No. 1 in for a sufficient length of time you will be able, when you take it out, to pass No. 10, without using the intermediate numbers. You do not leave it there because the stream of urine would soon wash it out, and it is necessary to put in a larger one to fill the canal a little more. If when you change the instrument you put in one quite as large as the stricture will admit, you produce pain and irritation, and infringe the principle I have laid down, so that the progress is less satisfactory. In continuous dilatation, as well as in simple dilatation you adopt the plan which gives the least amount of pain and irritation to the patient, putting as little as possible to the debit side, and as much as possible to the credit. The operative proceedings to be applied to the treatment of stricture I shall next speak of.

Now suppose, you have a false passage. Of course I will assume that you have not made one,—you will be much too careful for that; you are perhaps not the first surgeon who has seen the patient; some one else has seen him before you, and made a false passage.

You can easily conceive how difficult that false passage must make it to get the instrument in. It is only to be done by being very careful in avoiding the side on which the false passage is. A false passage commences usually on the floor, and no doubt for the reason mentioned, the structures below being looser than those above. When you examine a patient with a false passage, you probably find the instrument passing up to its very end, and no urine flows. Hence the notion that there is stricture at the neck of the bladder. When the instrument is thus passed, put your finger into the rectum, and you will know instantly whether there is a false passage; for if so, there are only the coats of the bowel, which are very thin, between your finger and the instrument, so that you feel it very distinctly. But if the instrument is in the right passage, you feel the whole thickness of the prostate, not always very considerable, between it and your finger, still quite enough to show that you are in the right path. It is almost always in the bulbous portion that it leaves the urethra and gets under the prostate. What you are to do, then, is to withdraw the catheter two inches or so, then pass it on again, keeping as close along the upper part of the urethra as you can, ascertaining by means of the finger that the instrument is not passing into the old route. It will be very likely to do so, because it is much more easy to get into a false passage than into the right one. I have devoted as much time to this subject as I dare. I have only given you these general hints; you will arrive at the rest yourselves by practice. There is very often a case of false passage in ward No. 10, and I wish you sometimes to verify the position of the catheter when it is in a false passage; to observe how very little tissue there is between the finger and the instrument. Then when it is

properly passed, you feel the thickness of the prostate intervening. You can only appreciate this by the touch, and will learn very little more by talking of it.

One word with regard to the injection of oil. When you have a very narrow stricture to deal with, instead of oiling the instrument, it is as well to throw half an ounce or an ounce of olive oil into the urethra, holding the syringe well round the meatus. It is easy to insinuate half an ounce or an ounce of oil through a very narrow stricture. It lubricates the parts, and sometimes the urethra is distended with the oil, so that, if you can cleverly retain it with the finger and thumb, you may introduce the instrument when you have been unable in any other way. This is worth noting. It does not do well when there is much bleeding or the tissues are torn, but when it is not so the plan is sometimes useful.

Now suppose that, following these hints, you have carried the instrument through the stricture; there is a sort of grasp which is quite unmistakable. That is a sensation which you are always content to have, because, feeling the catheter "held" by the stricture, you know you are through it. But that very "grasp," which you are so satisfied to have, makes it less easy to manipulate the point of the catheter after it is through the stricture, and you have sources of danger to encounter in the urethra beyond. Thus, the mucous membrane, being often reticulated, with dilated lacunæ, it is easy to engage the point of the instrument in one of those, and make a false passage. Be careful never to use force in this stage, and even after you are through, to get gently and quietly into the bladder.

Let us pursue the case one step further. Suppose you have got your catheter in at last after much difficulty. Do not pull it out. You will say: "I had trouble enough to get it in; and now I will tie it in at once." It is safe to do that, although it is a metal instrument; and you may keep it forty-eight or seventy-two hours before removing it. Do not be in a hurry to do that. You will be excessively disappointed to be obliged to repeat your task; and it will be very disagreeable to the patient. Keep it in about three days; and then you will be able to change it for a gum-elastic instrument. You then go on with "continuous dilatation" as already described, increasing the size of the gum catheter from time to time. You get up, perhaps, to No. 10, and tell the patient, "This is a great point gained;" but in ten days or so, to your great disappointment, the stricture will admit nothing larger than No. 2 or No. 3. Now it is clear that you have to do with what is called "resilient" or "contractile" stricture. It is not a question of calibre so much as a question of contractility. The narrowing has gone back almost to that condition in which it was at first. You can now only get in, say, No. 2. It is useless again to attempt dilatation. This is precisely that kind of exceptional case for which you reserve other means of treatment. Understand that you may have stricture very narrow, and be able to dilate it, and the dilatation shall be permanent. On the other hand, you may have stricture which will admit even No. 5 or No. 6 catheter easily, and yet the man will scarcely make a drop of water. We had an instance of that in the ward the other day. No. 6 instrument was passed; but the man could make no water until I had operated. These contractile strictures have been the plague of surgeons from time immemorial. If you go back to old records of surgery—how far back?—some hundreds of years—you will find that these cases have exhausted the wits of surgeons to the present day. All kinds of things have been used in order to overcome the difficulty. I cannot tell you half of the matters which have been put into the human urethra. I suppose the human stomach has been made to receive more abominable things than any other receptacle in or out of the human body. But if you consult the old surgical authors, or even some modern books, you will see that the urethra has been used nearly as roughly: and that is saying a good deal. Verdigris, savin, the salts of all sorts of metals, everything that could irritate, or that could be imagined to be disagreeable, has been employed to cure these unfortunate strictures. At the present moment I need not tell *you that some* surgeons have employed nitrate of silver and

caustic potash—not at all mild remedies either of them. Now the whole question of chemical irritants, as applied to stricture, I shall dismiss with the following words: I believe them to be unnecessary, undesirable, and dangerous. Most modern surgeons, both in this country and abroad, have pronounced against the use of caustics and chemical irritants in the treatment of stricture. I shall not pursue that question further.

Then what have we left? Several mechanical modes: we may rupture, or overdistend, or cut these unyielding and contractile fibres, which constitute the stricture. Urethrotomy, as it is called—division of the strictured urethra by some form of knife,—is, perhaps, all things considered, the most universally employed in such cases. Now there are two kinds of urethrotomy—external urethrotomy and internal urethrotomy: the external applied from the perineum; the internal, by means of the knife or some other instrument applied within the urethra.

I speak first of internal urethrotomy. There are two modes of doing it. You may cut the stricture from before backwards, or from behind forwards, the latter being the method most commonly adopted.

First, suppose you have a stricture at or near the orifice—a very undilatable part. You get in an instrument like that which I hold in my hand—a small bistourie cachée; project the blade and draw it outwards, dividing the obstruction. Nothing can be more simple. By means of a screw, you can raise the blade to a small or to a large extent. It should be used so as to make a rather free incision, which is perfectly safe. As a rule, the nearer the stricture is situated to the orifice of the urethra, the more necessary it is to cut, and the safer is it to do so. The further the stricture is from the external meatus, as a rule, the less necessary it is to cut, and there is rather more risk in doing so. All strictures at the meatus, and within, say, about three inches of it, which have resisted dilatation—and these generally do so—should be divided. You can dilate them, but the effect is temporary; and you can divide them with ease and perfect safety.

Secondly, with regard to strictures at a distance of five inches, the reason why it is somewhat less safe to cut them is on account of the large amount of erectile tissue at that point. In my own experience of sixteen or seventeen years of these operations, I have never lost a case by either internal or external urethrotomy, either in the hospital or out of it, and I have had a great many cases; I cannot say how many,—perhaps a hundred. Sometimes there may be a dangerous amount of fever, and, rarely, cystitis; much depends, I need not say, on the way in which the thing is done. External urethrotomy is usually known as Syme's operation. It is necessary always, in that case, to pass a small grooved staff or director through the stricture, to cut down upon the perineum, as in median lithotomy, and divide the stricture completely by cutting freely into the groove of the staff. That is very seldom done now. It was much done twelve or fifteen years ago; but other means have superseded it. Occasionally the operation is still performed, chiefly when there are perineal fistulæ which require to be laid open. After doing it, a catheter is tied in for forty-eight hours, and an instrument to maintain a good calibre must be passed from time to time afterwards. Now for internal urethrotomy in these cases of stricture in the bulbous urethra, this instrument which I hold in my hand is a very good one. I used it for a case here about a month ago, and the result was simply perfect. It has a small bulb at the end, concealing a small sharp blade within, which you pass through the stricture. You can then, by means of the bulb, feel exactly the position of the stricture. Having done this, you unsheath the blade about half an inch beyond the stricture, to the extent required, and draw it forward, dividing the contracted part freely. In that case, also, you should tie in a catheter forty-eight hours, and after that pass an instrument every third day, then every week, and so on. Here are instruments for cutting from before backwards, and of various kinds; but I prefer the others as a *rule*.

We next come to the mode of rupture; and here I will show you an instrument which bears the name of Mr. Holt, of the Westminster Hospital. He

has brought the instrument into notice, and the mode of using it is his own. It was invented by M. Perrève of Paris, more than twenty years ago. He used it mainly, but not altogether, for simple dilatation: Mr. Holt uses it otherwise. He passes it through the stricture, and then, instead of passing at different times a succession of tubes of gradually increasing calibre, he takes the largest tube at once, and forces it down the urethra along the central guide, so as at one stroke to split everything that happens to obstruct the passage of the instrument. Mr. Holt does not put in an instrument afterwards. When the operation was first introduced, I was strongly repelled by the violence of the proceeding; but I examined some of Mr. Holt's cases with him at the Westminster Hospital—now some ten years ago—and was surprised to find how few bad results followed. Hence I tried the plan, and use it occasionally for a urethra which has some two or three contracted points—a condition rarely met with,—so as to deal with them all with certainty. Rupture, however, is too rough a mode, in my opinion, for most cases; and I prefer to carry a fine blade carefully through the fibres of the stricture, and believe it to be the best, and the most enduring in results. But the popular sentiment about a knife cannot be ignored. The British public is not partial to a sharp edge, and is glad of almost any substitute—a feeling one can quite understand. So that it is not always possible to do the best thing, and we have to select that which is next best. You will, then, probably, find the proceeding by rupture a useful one when dilatation has failed. That operation suggested to me some years ago a different method—namely, one which I have called "overdistending" the stricture. It is simply this. Here are two blades, as in the instrument used by Mr. Holt, but these two blades can be separated for a considerable interval at one point, and as slowly as you please. In practice I do it very slowly, so as to rupture as little as possible, and to over-distend as much as possible, the structures which form the obstruction. There is this fact also to be noted: I wish you to remember that the bulbous part of the urethra—the usual position of stricture—is also the most distensible part in the natural condition of the canal. Supposing the external meatus to be about No. 12 (English scale) in calibre, the bulbous urethra admits at least No. 20 or 24. Hence it follows that no kind of dilatation or operation which is limited in extent by the size of the external meatus more than half restores the urethra which has a stricture at the bulb. It is on this account that I distend the contracted part to at least this, or even to a larger size, by means of the instrument in question. I have now used it many times, and it is certainly attended with good results. It is to be used only for strictures within the bulb. I have heard of its being employed for those within three inches of the meatus; it is a mistake to use it for such, which, as I said before, ought to be cut. This operation requires more care than the other. Mr. Holt's operation needs no care after the instrument is once in place; it is extremely easy to use it. If you once get the instrument in correctly, a single impulse of the hand forces the instrument through; and it is certainly tempting on that account. And for a considerable time afterwards you have good results from these operations. I think, however, that internal urethrotomy gives results which are more enduring than any other; but it is infinitely more difficult to perform properly, and, without doubt, it requires a practised hand.

Briefly let me, at the close of this subject, remind you that in all cases of impeded micturition, attention to the general health often aids in a considerable degree to mitigate the local troubles. Do not overlook the state of the digestion. If this is unsatisfactory, if the bowels are unduly constipated, the troubles of the bladder and urethra will be much increased; and frequently it happens that a mild mercurial, followed by a dose of Glauber's salts, or of Friedrichshall water in the morning, gently unloads the liver and bowels and greatly relieves the most distressing symptoms.

There is only one other word I have to say: do not be partisans of any single method. You hear one surgeon say: "I always follow such and such a method; there is nothing like it." Or another, that he always adopts the proceedings of M. Civiale, and a third that of M. Maisonneuve, and so on.

There has been great fertility in inventions of this description, especially in Paris, and you may see from several of them very excellent results. Do not confine your selection to any one method, whether I or any one else recommend it; but have every resource at your disposal. If you have much to do with stricture, or with such complaints, I can only tell you you will want all the resources within your reach. Consider them carefully, and select for each individual case that method which appears in your judgment to be the best adapted for it.

[It was stated on the great authority of Sir Benjamin Brodie, that "when the hair becomes grey and scanty.....the prostate gland usually, I might perhaps say invariably, becomes increased in size;" and that is the impression which a large portion of the profession have respecting it. Sir Henry Thompson examined two hundred cases of death above fifty-five years of age, and finds that the above maxim is very far from true.]

I found that about one in three exhibited after death some enlargement of the prostate. But of those do not suppose that anything like a large proportion manifested symptoms; for only about one in seven, not more, had symptoms of the complaint. So that you see it is not to more than one in (let us say) ten men who live beyond fifty-five years of age to whom you expect to be called to afford any relief for this affection. That, no doubt, is a large number. If you suppose that one man in every ten approaching sixty years of age has symptoms of enlarged prostate, you will see at once how often, if you have anything like a large practice, you may be called to attend to these cases.

We will now consider one or two anatomical points connected with enlarged prostate. This organ is, as you know, composed of two lobes and a median portion. Now, the part affected with hypertrophy very much influences the results in relation to the function of micturition. It is not necessary that there should be much enlargement of the prostate in order to produce very severe symptoms. On the other hand you may have a very large prostate, and may have almost no symptoms. Almost the largest I ever saw, as big as a small cocoa-nut, produced very little obstruction to the flow of the urine. Thus, if the median portion of the prostate is only slightly enlarged, there may be retention. Let this figure [diagram] represent the two lobes, and this the median portion. If there is a small nipple-like projection at the median portion, just filling the internal orifice of the urethra, that may be quite sufficient to prevent every drop of urine passing by the natural efforts. You will remember, then, that if you have occasion to examine a patient, and you find a very large prostate, it does not necessarily follow that he should have great difficulty in passing his water; and, on the other hand, although you may find inappreciable enlargement by rectal examination or otherwise, you may not therefore conclude that all his troubles, and they may be considerable, are not due to this complaint.

I speak next of symptoms. An elderly man comes to you and says that recently his water has not passed so easily, that it has issued in a dribbling stream, and he cannot propel it; that he requires to micturate a little more frequently, especially in the morning—probably two or three times while he is dressing; after which it becomes less troublesome, but at night it is rather more so than during the day. Then, if he does not say much about pain—which, of course, will excite your suspicion of calculus or some other complaint,—you will say, "This is probably a case of enlarged prostate." You do not necessarily proceed at once to pass the catheter, but you will ask the four questions already referred to. You will ask how frequently he makes water, and then, in connexion with frequency, whether the water ever passes without his being aware of it, or without his willing it. In many advanced cases you will find that some urine passes during a violent effort, such as coughing, or at night during sleep. Next you ask for pain, and if any whether before, during, or after the passing of the water. If before, and the patient is relieved by making water, it is probably hypertrophied prostate. *If it is after,* you may expect calculus, which comes into contact with the *mucous membrane* of the bladder when the water is gone; whereas if he has *a distended bladder,* as he is likely to have with enlarged prostate, it is pai

ful as distension occurs, and becomes less so as the urine passes off. Then you enquire about the character of the secretion, whether it is clear or cloudy. In most cases, at the commencement it is clear. In a great number of cases of prostatic enlargement, although the bladder has not been emptied for a month or even a year, the water is clear. On the other hand, if it is an advanced case the water will certainly be cloudy. And while you are talking of that you will also ask about the stream. You will generally find that it is a dribbling stream, different from the stream in cases of stricture. In stricture the stream is often propelled exceedingly well, although it is no larger than a thread, and so long as there is a stream he can act upon it by will, so as to make it stronger; whereas, strain as he may, in prostatic enlargement he often cannot influence the stream except for the worse. Then you ask the fourth question, "Do you pass blood?" Usually, in the early stages, the reply will be in the negative, although a little may appear after much exercise, so far suggesting stone.

Then you complete your diagnosis by mechanical means, and for this purpose you will first use a catheter. You should invariably make the patient pass water before you begin, because your object is not merely to ascertain whether enlarged prostate exists, but, what is much more important, how far it is a barrier to the exit of urine from the bladder. The great point to him and to you is, not the size, or condition, or shape of the prostate, but to what extent is it a barrier to the exit of urine. It is the quantity of urine left behind which will determine future treatment. I should advise you to use a gum catheter well curved, and certainly not a small one. As in stricture, always begin with a catheter of not less than 8 or 9, and of course without a stylet. In passing it keep the shaft well back in the groin, so as to maintain the curve. As soon as you have arrived at the bladder, carefully empty it, and note the quantity withdrawn. It may vary greatly, from an ounce up to almost anything you please. I have drawn off six pints, but that is a very large amount. You may find commonly from six to twenty ounces.

Now with regard to the employment of the instrument for patients with the symptoms described, do not forget that frequency of passing water, and still more the passing of it involuntarily, indicate the necessity for the use of the catheter. It is remarkable how common are the errors, not merely of patients but of practitioners on this point. They are apt to be misled by the fact that the patient insists, "I do not make too little water; I am making water too frequently, and too much of it, and I am sure my bladder must be empty. Tell me how to retain my water, and I shall be much obliged to you; don't think of drawing it off." It is surprising how that sometimes influences the practitioner. Nevertheless these are the very circumstances in which you should pass the catheter. Always bear this in mind, (and I wish, figuratively speaking, to render that sentence in the largest capitals,) that INVOLUNTARY *micturition indicates Retention, and not Incontinence.* There are a few exceptions to the rule, but very few. Most of the mistakes that are made on this point arise from the use, or as I shall show you, the abuse of the word "incontinence," which means of course, that the bladder is empty; and certainly when the bladder cannot hold its contents its condition is rightly described by the word incontinence. Now that happens only in very uncommon, but well-defined circumstances, such as in some cases of cerebral or cerebro-spinal paralysis, and in rare injuries to the neck of the bladder, and in these the urine runs off as fast as it comes from the ureters, the bladder having ceased to act as a reservoir. You see this one external physical sign is the same in these cases and in those in which the bladder is distended with urine; that is, there is urine dribbling off by the urethra. But mark how totally different are the two conditions in question. Whenever, then, you meet with this involuntary flow of urine, miscalled "incontinence," do not confound it with the condition in which the bladder is empty. Rely upon it the bladder is full, and the only way of relieving the patient is by the use of the catheter. I lay great stress upon this, because I have seen lives sacrificed to a forgetfulness of this point. I have made post-mortem examinations of persons who have died from the effects of reten-

tion undiscovered during life, misunderstood because the urine constantly passed off, as it was supposed, "so freely."

Now we know that our views of things, and our consequent acts, are very much determined by the manner in which we use and apply words respecting them, and it is impossible to be too clear and defined in all language, especially in that which relates to pathological conditions and surgical practice. I cannot express to you how strong my sense is of the importance of this matter; hence I have made it my constant business to point out the common misuse of terms in connexion with this subject.

First, then, the term incontinence, which means the bladder is empty, or "can't contain," should never be employed by you to denote the phenomenon that the patient's urine flows involuntarily; for, as we have seen, in that condition the bladder is generally full. It is better to use another term, viz., "overflow"—to denote that state; and then, remembering always my maxim, that "overflow indicates mostly retention, and not incontinence," you will never make the fatal blunder I have spoken of, and which I assert to be so common. This, too, assimilates our usage very nearly to that of French surgeons. The French, with their more logical use of language, speak of the bladder as "engorged," and "overflowing," but never as incontinent, except to denote that rare condition in which the bladder is always perfectly empty. I have, therefore, long been in the habit of denoting a bladder which is full, but allows surplus urine to run off little by little against the will of the patient, as an "engorged" bladder, and the phenomenon thus described as "overflow;" and I hope you will do so too. This brings us to another misapplication of terms. In this country the condition of the organ just alluded to is often called "paralysis" of the bladder, and the unfortunate word leads to mistakes in practice. The bladder is rarely paralysed. I know nothing of it except as an effect of spinal or cerebral changes. The bladder is never by itself the subject of paralysis, meaning, of course, an affection of the nerves either central or peripheral. It may be unable to expel its contents, because there is mechanical obstruction, as enlarged prostate, stricture, or impacted stone, or because the muscles have lost their power of contracting from long over-distension; but this latter condition is "atony." This inability is in neither case due to impaired nervous supply—a subject to be considered at some subsequent meeting.

After this digression, which its importance must excuse, we will go on to complete the diagnosis. While the patient lies on his back you place your finger in the rectum, and examine the size of the prostate, whether it is very tender, and whether the enlargement is more on the right or the left side. Of course you do this as gently as you can. The finger should be well oiled. The best position for the patient is lying on the back, because you can press the other hand above the pubes, and gentle pressure there brings the bladder and prostate near the finger, and you can ascertain whether the bladder is distended or not.

We shall now come to the treatment. The *medical* treatment of enlarged prostate may be dismissed in a few words. There is nothing to be done for it—that is, you cannot diminish the hypertrophy. There is often temporary enlargement from congestion; and that you can do something for. But hypertrophy cannot be diminished by any known means. All sorts of things have been tried: iodine in all its forms. And there is scarcely anything in the Pharmacopœia that presented a chance of doing good that has not been tried. We must simply say that, for the present, we know no means of combating the enlargement itself. But much may be done by way of palliative to the results of the complaint; and much of this treatment is mechanical. This will consist, first, in relieving the distension by the catheter. I have reserved for this place what I have to say about the instrument itself. The reason for preferring, as a rule, the gum catheter for this purpose is as follows:—Different curves are required for different patients; and the English gum catheter I will back against any other instrument, English or foreign, for general use. Unlike the French catheter, which is admirable for softness and elasticity, you have the power of making it assume any form you like—

lity, perhaps, not so often rendered available as it might be, but which mind is of immense service. The instrument-maker generally curves atheter in this way [diagram]: the point straight, and not well curved worst form in which you can put a catheter for use. For prostatic rement you require a catheter well curved *to its very point*. You should the instrument on an over-curved stylet for a month or so before em- ig it; and you will then find it easily assume the proper form, when ill pass it, as I need not say, without a stylet. If you require a stiff ument, a silver catheter should be selected; not a gum catheter with a in it. To return; you want the point of course to be carried over the uction formed by the enlarged prostate; and as the heat of the urethra s relaxes the curve, whatever it may be, by the time it arrives at the of the bladder, the ordinary gum catheter, as you have it from the r, becomes nearly straight, and will not pass over the enlargement. ram.] Now, when you have a catheter which has been well over-curved month or two, you remove the stylet and turn back the shaft, so as to the extreme curve and produce an ordinary one. And what happens you pass it? In spite of the heat of the urethra, the catheter has a ncy to curve more, instead of less, as it passes down the passage. And precisely the difference between success and non-success. That little euvre I regard as of extreme value. It is a very simple thing: keep the ter over-curved—not for stricture, but for enlarged prostate;—then back the shaft immediately before using. The curve gradually in- es as it goes onwards, and it passes over the enlargement into the blad- This is so simple that it seems scarcely worth making so much of; can only tell you that I know nothing of its kind that exceeds it in

n there is another thing; you may want a special curve for a particu- se. We have silver catheters with various curves. Here are several are very useful. But the English gum catheter possesses a quality , as I have before told you, is not found in any other; put it into water and bend it into any form you like; then dip it immediatley into water, and it will maintain the required form. But the best form so ced may easily be spoiled by your mode of using it. Of course the must not be altered while the instrument is passing through the ante- art of the canal, for it is at the posterior part that this form is required: aft of the catheter must be kept closely back in the groin, and the brought round the curve, so as to preserve the latter until it reaches ep urethra, when by well depressing the shaft the point will rise over bstruction into the bladder.

e general treatment is not to be disregarded; and here I shall defer a deal of what I might otherwise say until I come to speak of chronic is, which will occupy a subsequent lecture. Cystitis is associated with ny diseases of the urinary organs that I may as well refer to the treat- under that head, instead of taking it separately, and recapitulating it nexion with each disease. But as far as the general treatment of pros- enlargement goes, the main thing is to prevent local congestion. You the tell patient above all things to avoid anything like chills affecting the region, sitting on cold seats, exposure to cold; too much excitement, l or otherwise; long journeys, riding in jolting carriages,—all of which o produce pelvic congestion, and to interfere materially with the con- of the prostate.

other point has relation to the action of the bowels. You may make with enlarged prostate very comfortable if you keep the bowels in action. If he has constipation, and scybala lodge in the rectum, presence is often the source of great discomfort. Sometimes a simple of warm water gives instant relief; but, if necessary, the action of wels must be provided for by means of gentle laxatives, such as senna, a, bitartrate of potash, sulphur, or by sulphate of soda; anything which ot mildly, quickly, and without irritation, will keep him in a very different tion from that which is associated with habitual constipation.

I will devote the few minutes that remain to those cases of prostatic enlargement in which the difficulty of passing the catheter is considerable, and there is retention of urine. You may have a patient in whom prostatic enlargement has manifested itself rather suddenly; he may have had symptoms which he has not noticed, but he is suddenly attacked with congestion, he cannot make water, and he is in great distress in consequence. It is not a question how long a time is to elapse before the catheter is to be passed, you should relieve him at once. You find there is a distended bladder, evidenced by dulness of percussion above the pubes. Possibly, before you, others may have come, and you may be called to pass the catheter in circumstances of some danger to life. Now, in the first place, you should be careful with regard to position. I advise you to pass the instrument with the patient in the lying position first, if the bladder is very large; if it is not very large, perhaps it is as well, or better, to pass it standing. You can empty the bladder better in the standing position; but if you find that the bladder is very large, make the patient lie down before you draw off the water. I have known great mischief arise from drawing off a large quantity of water from a patient when in a standing position. I have even known death occur suddenly from this cause. If I had time, I could tell you of a case in which a charge of manslaughter was brought against a surgeon in a court of justice in relation to such an occurrence. The circumstances were all well known to me, for I was there to defend a brother practitioner. In that case a catheter had been passed in the upright position, and the patient fell dead from syncope, when six pints of urine had passed; just as a patient with ascites might do if you tapped the abdomen in that same position. No doubt it was an error, but nothing could be more monstrous than to make it a matter of action at law. It is a very instructive case, and I mention it to show that in cases where the bladder is large, especially in old men, you may have fatal syncope arising in the way I have described. Take care also, in these circumstances, to draw off only part of the urine; and after a quart or so has passed, wait a little before you empty the bladder. If you fail to pass a gum-elastic instrument, by all means use a silver one. The silver prostatic catheter is sometimes essential; that is, one which is much longer, and has a larger curve than the ordinary instrument. Sometimes, when these fail, a catheter with a short beak, like the sound or lithotrite, will pass easily. And always remember that anything like obstruction at the end can only denote that you are out of your path. No force should be employed. It is not a narrow passage; it may be a little close, perhaps, when you reach the prostate; but if you find any obstruction, you should withdraw and find another route, to the right or to the left. Never use force under any circumstances whatever.

If you have had much trouble in introducing a catheter you must sometimes leave it in. As a rule it is not good practice, but if an instrument must remain there, that made of vulcanised india-rubber is by far the best; it has generally to be mounted on a stylet for the purpose, which is withdrawn afterwards. It is the least irritating kind, and scarcely ever becomes coated with phosphates.

Subsequently, as the canal recovers, should the bladder not regain its tone and the catheter is therefore necessary to be used two or three times in the twenty-four hours, you will probably in the course of time teach the patient to relieve himself; and he will often, with a little tuition, succeed remarkably well. But it may happen that the catheter cannot be passed, and the subject of operation for the relief of the patient must be considered.

Retention comes before us in three typical forms, each requiring a different species of treatment. There may be some instances which cannot be absolutely so classed, and some the characters of which partake in part of two classes; still, for convenience, it is well to adopt this classification. First of all, you may have retention occurring in a young and healthy man who has no stricture; next, it may occur in an older man who is the subject of organic stricture; and, lastly, it may occur in a man who [illegible] who has no stricture, but has an enlarged pro-

ıte. Of the last I have nothing to say; we have already discussed that ıbject, and the mode of relieving retention in connexion with enlarged rostate. But I shall ask your attention to the two other conditions—name- r, retention from inflammatory swelling, and retention arising from organic :ricture.

With regard to the first kind, you will probably learn a history something ke the following. Within a month or six weeks perhaps, the patient, who generally a young man, has had gonorrhœa. He has obtained considerable lief from treatment, and has in consequence allowed himself some relaxa- on of the regimen to which he has been lately submitting. Thus, perhaps, e has indulged a little in the use of alcoholic stimulants, has taken some nusual amount of exercise, a game of cricket or the like, and, after being verheated, has been sitting on a cold stone or damp grass; or, lastly, he ıay have indulged in some emotional excitement. Under those circum- tances what is called "inflammatory stricture" may be produced. Now let ıe say, repeating myself slightly, that that condition has no right to the ame of stricture. First of all, the inflammation is probably at the neck of he bladder or in the prostate. It is difficult to ascertain that, except by in- erence, confirmed however by a rectal examination; for, happily, we very arely make post-mortems under such circumstances, as the patient does not uccumb to the complaint. But what almost to a certainty takes place is a egree of inflammation and swelling of the prostate, not in the least resem- ling stricture—that is, it is not a circumscribed narrowing at a particular art of the passage, but a tumefaction of the prostate gland, which prevents he expulsive apparatus of the bladder acting and discharging its contents. hat is usually the real condition in what is called inflammatory or spasmodic etention.

This condition of the prostate resembles that which affects the tonsils, and hich we call inflammatory sore-throat. Both complaints consist in the en- argement of glands which more or less surround narrow passages; and so nterfere with the functions of those passages; both occur rapidly, and may e produced by external cold.

Now, what are the early signs of inflammatory retention? First, there s usually some cessation of the gonorrhœal discharge. Just as in the case f orchitis, where the urethral inflammation is supposed to subside and to ttack one of the testes, the inflammation of the prostate is similarly asso- iated with diminished discharge, and if you examine by the rectum a very ender and swollen condition of the prostate will be discovered. Then the tream of urine grows smaller and smaller, and in a very short time the atient loses the power to relieve himself altogether. He is feverish, very estless, and suffers severe pain about the lower part of the abdomen and in he perineum. Those who are the subjects of stricture may have become in ome measure accustomed to difficulty, but when an active young fellow is hus attacked for the first time, he is in a state of extreme distress.

Now as to the treatment of such a case. The patient desires ardently to e relieved immediately, and declares he cannot endure his sufferings. You ee him bent nearly double in order to relieve the pressure of the abdominal nuscles on the bladder, and he is even breathing shortly and quickly to avoid heir action there. The old treatment in such cases, the classical treatment wenty years ago, was bleeding from the arm or perineum, repeated hot aths, large doses of opium, so as to enable the patient to bear the pain and et to dispense with the catheter. The reason assigned was that in an in- amed state of the canal you might do more harm than good with a catheter, nd that it was therefore better to mitigate pain by the means described. I ave told you that I dissent from that treatment altogether, although it is till employed to some extent. For, first, you must look at the after-conse- uences; and if you allow a young man to remain thirty-six or forty-eight hours with an unrelieved bladder because you fear to use an instrument, permanent mischief may be done. I have seen patients who for years have been unable to empty the bladder after treatment of this kind. Extreme and continued distension of that organ sometimes destroys or permanently in-

minishes its contractile power, and produces a condition which is properly termed "atony of the bladder." Therefore if you pass the catheter, even at the risk of doing a little mischief to the urethra, I am disposed to think you would be wise in incurring that little risk rather than expose the patient to the other danger. But then it ought to be done without such risk. For my own part, I always take a moderate-sized gum catheter—one not larger than a No. 6, as a large one gives in these circumstances unnecessary pain—which has been tolerably well curved in the way I have before described, since it has to enter over a swollen prostate. In this manner there is generally no difficulty in relieving the patient, who is exceedingly grateful for what you have done; whereas if you put him through that long process, and he relieves himself ultimately, he thanks you for little, comparatively speaking, and he runs the risk to which I have referred. But in the event of the gum catheter not passing, you should try a silver catheter of the same size. I believe one of the first persons to denounce the old plan of bleeding and hot bathing was Mr. Guthrie. If you turn to the racy writings of that experienced and practical surgeon, you will find an anecdote in connexion with this subject. He relates the account of a visit which he paid to a patient in the circumstances of retention I have described, and the reasons, in strong and graphic language, why he then gave up for ever the bath and bleeding practice, and passed the catheter at once for such cases in future.

So much for the inflammatory condition of the prostate producing obstruction to micturition. I need not refer at any length to spasmodic retention, which rarely happens. At the same time it may be as well to say that no doubt where an inflamed condition of the urethra exists, spasms of the muscles may coexist; but the precise share which each takes in contributing to the result will not influence the treatment.

Now I come to the second form of retention—viz., that depending upon organic stricture. Here we generally have to do with an older man, because it is rare to find a young one suffering from confirmed organic stricture. As a rule, such a patient mostly has stricture ten or twelve years before he gets complete retention. First of all you have to ascertain that it is stricture. You will probably find that he suffers less acutely than in the previous instance, but still a great deal; the progress of the case has been more gradual, and the derangement has not necessarily been brought on by any great or sudden imprudence. He has been passing water with difficulty for weeks or months, and at length some slight thing has produced a condition of absolute retention; the last ounce has broken the camel's back. Or it may not be absolute retention as before; there may be some dribbling, indeed the patient may have been relieving himself in that inefficient way for days, but the bladder is greatly distended, and to all intents and purposes it is a case of urgent retention. You find probably also that the patient is accustomed to instruments. Now what you have to do is to take an instrument of middle size, and pass it down to the seat of obstruction, to see where it is. You will probably find it four or five inches from the external meatus. You should then take the finest gum catheter and endeavour to insinuate it into the bladder, and if you are sufficiently fortunate to do that, you should tie it in at once, so that you may have no further trouble with it. But that is not a very difficult case of retention. Supposing you do not get the gum catheter in, I should then recommend a small silver one, either No. 5, or even smaller, and use it in the manner I have described to you in the lecture before last. Notwithstanding all your care and skill, and those, perhaps, of your friends whom you may have called in, the instrument is still not passed. There may have been false passages (they are easy to make), and there may be such difficulty that it is almost impossible for anyone to pass a catheter after your failure. We then come to the question, What is to be done next? Well then, first, much may be effected for these cases by opium and hot baths. Suppose the water is dribbling off, and you shrink from the *dernier ressort*—that is, puncturing the bladder, or other operation to relieve the retention of the urine,—a safe middle course may still exist for some of *these cases*. The patient up to this time may have been exposed to cold;

let him have the benefit of a warm bed and hot baths, with large doses of opium,—and you must be very liberal with opium if you use it at all,—so as materially to mitigate the involuntary straining, which he can no more help than he can help breathing, and which is utterly unsuccessful as regards the contents of the bladder, and often makes matters rather worse than better. The result may be that the water may dribble off more freely than before, and you may find after two or three days that it will come in a larger stream, and that then you can pass the instrument without much difficulty. The patient may often be saved an operation thus if there are grounds for declining to perform it. On the whole, however, I do not advise waiting very long: still it is better that the patient should be temporised with in that way than that he should be damaged by an unsafe hand either with knife or catheter. Most men, indeed, are quite sufficiently confident in their powers to rely on instrumental methods when they find that a patient cannot make water. Still if you are convinced that you are not doing any good with the catheter, still more that you are doing mischief, you can in most of these cases have recourse to opium, or to an inhalation of chloroform, with hot baths and fomentations, with success as regards the immediate and urgent condition.

But we will assume that you have done all that you can do in this way, and that the question of relief by some other means must be met. The bladder is increasing in size notwithstanding your treatment. You examine the supra-pubic region carefully, and find a tense and perhaps large tumour there, reaching to the umbilicus or nearly so, more like a uterus than a bladder. In some old cases of stricture there is not necessarily large supra-pubic dulness, for the bladder is thickened and contracted. Introducing the finger into the rectum you find there also a swelling produced by the distended bladder, and you then seek to obtain the sensation of fluctuation. If, placing my hand above the pubes, I feel a distinct wave communicated to my other index finger in the bowel, I know that to be a point at which the trocar can be inserted with safety. Also if I find a well-marked rounded tumour over the pubes which is dull on percussion, the bowel around it clear and distinct, I have reason to believe that an operation over the pubes would be successful. Again, the question arises, why not attempt to relieve the bladder by an operation on the urethra itself from the perineum, so as, if possible, to cure the stricture, and at the same time relieve the bladder? Might it not be wise in this manner, as it were, to kill two birds with one stone, and not be content with merely puncturing the bladder by the rectum or above the pubes.

At this point let me revert to the different practice and different experience of surgeons in relation to this matter. Let me give you the experience of Mr. Liston. He once said, from this chair, that during the whole of his connexion with the Royal Infirmary of Edinburgh, and subsequently with this hospital up to the time at which he spake—namely, three or four years before his death,—he had never punctured a bladder for retention of urine. On the other hand, there are men living in this town who have punctured a bladder fifty times or more. Mr. Liston meant to imply that a good surgeon ought rarely to find it necessary to resort to any other means than the catheter in circumstances of retention. But do not suppose for a moment that the gentleman I spoke of who has punctured a bladder fifty times does so because he fails to pass the catheter under those circumstances, but because he thinks it wiser to puncture the bladder than to persevere too much with the catheter. Both Lister and Guthrie occasionally performed the perineal operation just spoken of. From the perineum the urethra *may* be reached behind the stricture. Now, without entering into a long discussion on the subject, I may say that this mode has lost favour of late years. It is no easy thing to find the urethra behind the stricture; and a man may make an awkward wound in the perineum, and never hit the urethra at all. Then it does not follow that it should be desirable to divide the obstruction at all, so far as its cure is concerned; for the stricture, when the time comes to treat it, may be amenable to dilatation. The reason why puncturing through the rectum has been done so often by Mr. Cock, of Guy's Hospital,

is because he conceived it to be an excellent kind of treatment. He say[illegible]: "Let us withdraw the urine from the urethra altogether for a few days, a[illegible]d the urethra will recover itself, so that we may be able to cure the stricture with ease." And that is often true. He punctures the bladder by the re[illegible]c-tum under the circumstances I have mentioned; and this is his instrume[illegible]n[illegible] for doing it. In this way, the water not passing by the urethra at all, t[illegible] urethra is lying fallow, so to speak; and in a short time the instrument c[illegible] be passed—say No. 2, or 3, or 4 catheter, although before you could not p[illegible] No. 1. This, then, is a species of treatment of stricture which Mr. Cock h[illegible] introduced: and, at all events, he has proved how easily and safely the op[illegible] ation may be performed; he has, in fact, familiarised us with a proceedi[illegible] which before was often thought a very grave and serious affair.

If, then, you have failed to pass the catheter and the symptoms are urge[illegible] you have two proceedings before you: puncturing by the rectum, or abo[illegible] the pubes. By carrying into the bowel your finger—a reasonably long fin[illegible] —you arrive at a point just behind the prostate. The other hand is pla[illegible] above the pubes, that the wave of fluid, by its pressure, may be distinc[illegible] felt by the finger in the rectum. You are then quite certain of what you [illegible] going to do. Along this finger, kept firmly in place, your trocar is slip[illegible] and then boldly but carefully pushed into the bladder. This is always [illegible] anxious moment, because, if you have not hit the bladder, it is a ser[illegible]ious matter to have thrust this long instrument into the centre of the body [illegible]nd find no urine escaping. The best position of the patient is sitting on [illegible]he edge of a bed, resting his back on pillows behind, the legs apart, each o[illegible] a chair; and an assistant by him placing one hand on either side above [illegible]he pubes, so as to steady the bladder, and press it down towards the rectu[illegible]m. It is well to remember, subsequently, that if the canula slips out, you will not be able to get it into the same opening again. The muscular fibre of the bladder instantly closes, and you have to make another puncture: no[illegible] a matter of much consequence, but better avoided.

For the supra-pubic operation you divide the structures in the middle [illegible]ine until you reach the linea alba. Then carefully making your way dee[illegible]per, you will soon discover fluctuation; and having the bladder steadied as bef[illegible]ore, you will thrust the trocar slightly downwards. In this case you retain the silver canula for two or three days; but you may soon substitute a gum in-strument. Now supposing there is a probability that your patient may req[illegible]uire this artificial relief by tube for some time, you will, of course, prefer the situation above the pubes, because it is much more easy to wear the [illegible]ube there than it is in the rectum, where it interferes with the functions of the bowel, and is otherwise much in the way. I have known patients who [illegible]ave passed all the urine through a tube above the pubes from ten to fifteen y[illegible]ears, and who lead active and comfortable lives in consequence, the natural p[illegible]ass-age being completely obstructed. One of them, who had suffered gr[illegible]eatly before, and was now in perfect comfort, told me that "he did not know whether this mode of passing water was not preferable to the original one!" That, however, I conceive is quite a matter of taste.

[Speaking of extravasation of urine, Sir Henry Thompson observes:]

Suppose that from bad treatment, neglect, or otherwise, the patient has had no relief, and when you are called in find there is no question of puncturing, for the urethra has punctured itself, so to speak; that is, nature (as in the case of all maladies) has done something; often she acts in a clumsy way, but sometimes not more clumsily than the surgeon. There is no question that many who are the subjects of stricture or retention, and receive no surgical aid at all, will lose their lives in consequence; but they are sometimes saved by extravasation of urine taking place. What then happens is, that behind the stricture a portion of the urethra gives way, perhaps during some violent act of straining, and through the rent so made a quantity of urine is driven with great force into the cellular interspaces. Well, we know where that must go, from the anatomical disposition of the fascia—viz., into the scrotum, up into the groin above Poupart's ligament, and towards *the belly*. Occurring as it usually does in the bulbous part of the urethra,

where the walls of the canal are weak, the urine cannot pass backwards behind the scrotum, or the back part of the perineum: neither can it get into the thighs, because it is checked by Poupart's ligament. I have seen it rise as high as the chest, and I have made incisions to evacuate it, in a bad case of extravasation, as high as this point. Once taken place, it follows that at every action of the bladder more fluid is driven in with force, so that the cellular interspaces are separated, and the fluid usually finds its way upwards. Generally you ought to know at once by the appearance of the patient what has taken place, although it is possible sometimes to confound it with ordinary inflammatory œdema of the scrotum, for it may commence very gradually and insidiously. In ordinary cases you see a hard perineum, a large red, tense scrotum, the penis swollen, and a red blush perhaps rising over the pubes. In order to ascertain the true state of things, you must ask for the antecedents, and you will probably learn that there was great difficulty in passing urine, followed by rather sudden relief. When a man has had retention for some days, and extravasation takes place, instant relief is experienced—the frightful want to make water disappears as soon as the fluid finds its way into the scrotum; but he soon feels new pains, not necessarily very severe, but, what is worse, constitutional symptoms rapidly set in. The poisonous fluid quickly destroys the cellular tissue, so that sloughing soon begins. After forty-eight hours or so, gangrenous discolorations appear, and the urine may find its way into the corpus cavernosum, when a dark spot appears on the glans, showing that the structure of the penis itself is infiltrated. Without detailing the condition, which you must have seen for yourselves, and which may be seen now to some extent in a patient in the ward upstairs, let me say, Do not in such a case be afraid of the knife. You have no occasion for a catheter; the urine has found its way into the cellular tissue, and you must let it out as freely as possible. On each side of the perineum make a good deep incision. You need not limit the incision to two or even three inches; because you are really cutting into urine, not into flesh. The structure is so enormously distended that there is but little flesh to divide; and although the incision may appear very deep and long, when the water has run out it will be comparatively small. The incisions generally bleed rather freely. One may soon lose a pint of blood from three or four incisions. The urine runs out also, and as the distension goes off, the vessels are enabled to contract better; but if you see any little vessel spouting, tie it at once. An incision should be made on each side of the penis, because if it is made in the middle line there is not sufficient communication for the incisions on the one side to relieve the other. Do not be extravagant in these incisions; still it is better to err on the side of freedom than to be too niggardly in the use of the knife. The next day, if the case has done well, you will find the scrotum much less in size, and the parts altogether much less swollen and inflamed. You have now a direct communication through the cellular tissue from the bladder, and, with that rent behind the stricture, as a rule, you will be safer in letting the catheter alone, and permitting the water to drain off. What happens? Why just what happens after puncturing the bladder. When the water flows off by another passage the urethra begins to improve, and in three or four days you will probably have no difficulty in passing No. 3 or 4 catheter. With these patients, bad as they are, prostrated as they are when you see them, if the case has not gone too far and too much gangrene has not taken place, very striking and rapid recoveries often follow. The whole scrotum may slough away, and the testicles may be seen uncovered in the wound, and yet all may heal up soundly and well.

[After the exit of the urine by these artificial channels, some of them fail to heal and remain patent, and thus form urinary fistulæ. In all cases of simple urinary fistula,]

Whatever part of the canal it is connected with, it almost always heals if the stricture with which it associated is dilated. Dilate the stricture, and in nine cases out of ten the fistula will heal. Patients, especially in private practice, are often extremely anxious about the result of an opening in the

perineum or elsewhere through which the urine passes; and it is right that you should assure them for the most part, if the stricture is thoroughly dilated, the unnatural passage will heal of itself. But there is another point to be considered in connexion with this—viz., the quantity of urine passed by the fistula, compared with the quantity passed by the natural passage. Of course the gravity of the thing depends very much on the relative proportions passed by the two ways. Usually about three-fourths of the water will pass by the right way, and one-fourth or one-fifth by the wrong passage. If a large quantity—say three-fourths—passes by the unnatural opening, then probably a considerable amount of stricture is present. Nevertheless, as the stricture is dilated, you will see that the proportion passing through the fistula will gradually decrease until it ceases altogether, and the part soundly heals; but this latter result is achieved only on the condition that you maintain the stricture in a dilated condition.

Now we come to fistulæ which are associated with much inflammation and induration in the perineum. These may be multiple; in which case you may find five or six openings. I have known a man with a dozen, so that instead of the urine being discharged by one stream it flows as from a watering-pot. Even this condition, however, very much improves as the stricture is dilated, and may get quite well; but this is not always the case. Then there are some instances looking much less formidable, like those upstairs, with perhaps only two or three openings, through which the greater part of the water has passed for a long while. You recollect that we fully dilated the stricture in each case; but still no improvement as regards the flow of urine through the fistulæ took place. A No. 12 catheter was passed; but the patients did not get well. The condition of the perineum improved very much; but still more than one half of the urine obstinately held its erratic course through the perineal openings. Now, what is commonly done in such circumstances? Usually operative proceedings of some kind are resorted to; or, if these have been postponed or rejected, a rather tedious process has been employed. The principle laid down—and I have myself applied it successfully—is that it is necessary to take care that the external openings should be very free, ensuring this either by means of the knife, or by potassa fusa, or by some other means; so that the urine may not be detained in its way from the urethra to the external surface, causing fresh induration or thickening. Next, you may go on to excite adhesive inflammation in the track of the fistula by a hot wire, or by touching it with cantharides or a strong solution of nitrate of silver. No doubt this treatment sometimes succeeds; but it is at best a tedious process. Then it was sometimes attempted to cure such fistulæ by tying in a gum catheter for weeks, or even for months; but this always fails, and for this reason, that urine always finds its way from the bladder by the side of the catheter, along the urethra, and so into the fistulæ, by the force of capillary attraction, and thus the object supposed to be attainable, in reality never was and never could be accomplished. The practical surgeon soon discovers that tying in an instrument never ensures the transit of all the urine through it: some will always pass by the side and defeat your purpose. I have, therefore, adopted the plan of teaching the patient to pass the catheter himself; and that is by far the most rapid and the most certain method. With regard to the two cases upstairs, ten or fifteen years ago I should have applied potassa fusa or galvanic cautery, or something of that kind; but the fistulæ have soundly healed through insuring, by means of the catheter, not, as by the other process, that the urine should percolate rapidly through the perineum, but that it should not pass through at all—in fact, turning the current the other way. You first teach the man to pass a No. 7 or 8 gum catheter for himself—an easy matter enough. He then agrees to pass it every time he requires micturition, night and day. On no occasion is he to permit the urine to flow spontaneously—say during five or six weeks,—not even when he goes to stool; and this is avoided by always using the catheter immediately before. That plan has been followed in each one of the three cases in question, without difficulty

and with perfect success; for each man has a sound perineum, and has now relinquished the use of the instrument.

Now I come to the third form of fistula, that in which there is loss of substance. This class must be dismissed rather briefly, because its full consideration would involve tedious detail of many different surgical procedures. Where you have this loss of substance, a plastic operation of some kind is generally required to fill up the gap which exists. When the opening is small, you may contract it very materially by the heated wire or galvanic cautery, or by any mode which tends to produce a contraction of the tissues. You know that cicatrices which result from burns contract considerably, and you avail yourself of that action in this instance. Most commonly, however, if the soft parts have been largely destroyed, some plastic operation is required for the cure. Thus, on passing a silver catheter, when a portion of the urethra has sloughed away, you may see perhaps a quarter or a third or half an inch of the catheter exposed in the wound. The successful treatment of such cases demands much care and nice management. They do not often come under our notice, and less often do they get completely cured. I have had in the hospital but three or four such cases, in which, by means of plastic operations, the patients have been entirely cured. Some of you saw one last winter: a man who had just between the angle of the penis and the scrotum an opening, showing at least a third of an inch of the catheter, the whole of the floor of the urethra having sloughed away. The operation in that case was one of the most successful I ever saw. The first operation completed it, with the exception of an opening not larger than a pin-hole. What was done was to pare the edges all round, to get a large flap of skin from the scrotum below, which was brought up to cover in completely the wound, the margins being carefully attached by a number of little sutures. That fistula healed perfectly. And why did it heal? Here is the important point: there was one condition necessary, without which it would have failed. A week or two before this operation, I made the patient learn to pass the catheter habitually, so as to draw off every drop of urine; and finding him thoroughly expert at it, I performed the operation: and for the next month he never allowed a single drop of water to pass otherwise than by the catheter. Had I tied the catheter in, it would not have been sufficient, because the water always finds its way by the side sooner or later. Luckily, he performed his part of the compact to the letter for the stated term, so there was no reason why the wound should not heal there as well as anywhere else. The little tiny opening which remained was perfectly closed with the heated wire, and the urethra can now perform all its functions perfectly well. *Lancet, Jan.* 11, *Feb.* 1, *Jan.* 18, *Feb.* 8, *March* 7 *and* 21, *and April* 4, 1868, *pp.* 35, 154, 73, 183, 307, 367, 429.

55.—ON THE TREATMENT OF STRICTURE OF THE URETHRA BY THE STRICTURE DILATOR.

By Dr. Robert M'Donnell, F. R. S., Surgeon to Steevens' Hospital, Dublin.

[The question of the advantage of rupture of a chronic stricture by Holt's dilator is one certainly on "the debateable ground." Undoubtedly perfect recovery does not always follow this mode of treatment. The experience of Dr. M'Donnell is very favourable.]

In all the cases recorded in this communication the directions for the operation given by Mr. Holt were followed. In the instances in which the stricture was very close I dilated it with bougies for some days, until a No. 4 instrument could be passed. I always operated upon the patient standing his back firmly fixed against a wall. The instrument is more readily introduced in this position than when the patient is lying down. I may add that I have never found it necessary to use a dilator with the directing-rod hollow; if the patient has for a time retained his urine when the dilator is fairly introduced into the bladder, I have invariably found the water to flow along

between the blades of the instrument, not leaving any doubt of its bein[illegible] the bladder. In fact, I should lay it down as a rule, never to be departed f[illegible] that unless the water flows in this way, so as to make it a matter of cert[illegible] that the bladder has been fairly reached, the dilatation should not be [illegible] ceeded with.

Although in these pages I mention only those cases which I was able to follow carefully, owing to their being kept within prison walls for some years, yet I may say that besides these I have operated on a considerable number of others, both in hospital and private practice, yet I have never met with any untoward result: in two cases only did any considerable constitutional disturbance supervene, and this subsided after three or four days. When I compared this with the ill effects I have myself witnessed, as well in instances of internal as external section of strictures, and even following too hasty dilatation with bougies, it is, I must confess, perfectly astonishing. In short, were it not for the facts staring us in the face, one could not believe that the forcible splitting of an old stricture could do so much good and so little harm. It proves how essentially tentative everything in surgery is.

When operating on my first cases I had the benefit of Mr. Rawdon Macnamara's assistance. To this gentleman we are indebted for the introduction of this operation into Ireland, and his earnest advocacy has not only won for it a high place in public favour, but has in a great degree overcome the timidity of those practitioners who, like myself, at first regarded with fear a procedure which seemed so heroic.

[Dr. M'Donnell then gives a table of twelve cases operated upon by him at the Mountjoy Prison, and therefore under observation for long periods afterwards. One had a relapse four months after the operation, and one died of cholera; all the other cases were permanently cured.]

In all the foregoing cases I had recourse to an expedient which, I conceive, leaves no doubt that stricture actually did exist. I used a double-length catgut bougie, made with a gum elastic catheter to slide over it—an instrument devised, I believe, by the late Dr. Hutton, and much used by that eminent surgeon.

The catgut bougie (of small size) is first introduced into the bladder; being double the usual length, a long piece projects from the urethra: on this a catheter, open at each end, is slid: the end of the catgut being held, the catheter is slipt on, and is thus conducted along the urethra; if it comes to a dead stop at any point, it there has met with a stricture too close for it to pass. The narrowing permits the catgut to get through it: to the catheter it says no. According to the size of the catheter we know the size of the stricture. No lacuna, fold of membrane, false passage, or enlargement of the prostate can now deceive us. The conducting bougie would certainly steer the catheter past any such obstructions. I think this method may be considered a crucial test for the existence of stricture, and it was applied in each of the cases mentioned in the above table. I am then, I believe, justified in stating that stricture existed in every one of these patients, as in no one of them could I at first slip a No. 2 catheter along the conducting bougie into the bladder.

At the time (August, 1863) when I operated upon cases Nos. 1 and 2, Mr. Rawdon Macnamara and Dr. Cruise were present. They will remember, I do not doubt, that one of these patients made a considerable outcry, and that in his case the thrusting in of the dilator required an amount of force which to me, as a beginner, was alarming. The other patient (G. W.) merely said, "Oh, oh," and looked astonished at the unaccustomed sight of a large stream gushing from a large-sized silver catheter. Very little force was necessary, and the introduction of the dilator was accompanied by a slight snap, as if a thread tied round the urethra had given way. This patient was surprised when he was directed to go to bed, as he said the operation had cost him no more pain than the introduction of an ordinary instrument. This case (No. 2. G. W.) is a most important one. Two months before this he had been in hospital, under treatment by gradual dilatation; six weeks after *leaving* hospital, with a urethra able to admit a No. 9 bougie, I could not

pass No. 2 along the railroad catgut. Yet this stricture, which, under ordinary treatment, has such a tendency to return, is split, with little or no pain, and four years after he is reported as having but slight stricture.

Case No. 5 (P. H.)—Was examined by Dr. Cruise with the endoscope some months after he had been operated upon; indeed this is one of the cases alluded to by Dr. Cruise in his classical paper on the Endoscope in No. 78 of this journal. We perceived a transverse slit, somewhat crescentic in form, at the spot where the constriction had been.

Nos. 7 and 8 relate to the same individual. This is the only case in which anything of rapid return of the stricture took place, and as the patient died of cholera within a month after the second operation, and as an opportunity was afforded of examining the urethra, the case is one of importance.

I exhibited the bladder and urethra at the Pathological Society on February 15th, 1867.

The following is the report as published in the proceedings of the Society:——

"Dr. R. M'Donnell brought before the Society the bladder and urethra of a patient who had suffered from a stricture, which, shortly before the individual's death, had been treated by the method called 'bursting.' The following was the history of the case:—J. M'C., aged 36 years, was admitted to the hospital of the Mountjoy Convict Prison, on July 23, 1866. He stated that he had been suffering from stricture of the urethra for six years. He could only pass water in a very small stream, or in drops. At first only a small catgut bougie could be introduced; after dilatation up to No. 4 was accomplished, Holt's dilator was introduced, and the stricture was burst. Immediately after a No. 10 silver catheter was introduced into the bladder. All went on well, and four days later the urine was drawn off through a No. 10 silver instrument. He was discharged from hospital on August 3rd. On November 6th he was again admitted to hospital stating that the stream had become as small as ever. On examination it appeared that it was not possible to introduce into the bladder an instrument larger than a No. 1 catheter. At his own request the operation of bursting was again performed as before. He was dismissed from hospital on December 7th. He died suddenly of cholera on December 22nd. Dr. M'Donnell removed the bladder and urethra, and they were examined carefully by Dr. Cruise, Mr. William Stokes, and himself, soon after removal. The appearance, however, at that time was not materially different from what the members had now an opportunity of seeing. A No. 9 catheter could readily be passed along the urethra. Except for the hypertrophied condition of the muscular coat of the bladder, and the dilatated state of the portion of the urethra behind where the stricture had been, there was no other sign of the disease having existed. There certainly did not appear to be any granular condition of the mucous membrane in the vicinity of the triangular ligament where the stricture had existed, neither was there any marked induration of the submucous tissue. Dr. M'Donnell had at first thought that there was an appearance indicating that the dense fibrous, submucous tissue had been split by the process of dilatation, while the mucous membrane itself had been only slightly, if at all, lacerated. On inspecting, however, other urethræ he found a similar appearance. The interest of this case was, perhaps, in the fact that it showed so little sign of disease remaining. The case was one in which the stricture had unquestionably returned in a comparatively short time after being burst."

Besides the cases given in the table, I met with two cases among the convicts in the Mountjoy Prison, in which the immediate plan failed to afford even temporary benefit. As they were cases of quite an exceptional character, I do not think it would be fair to group them with ordinary cases of stricture.

A military prisoner, committed for an offence of the most serious character, gave the following account of his case:—Some years before he had a bad stricture, which became complicated by perineal abscess. A surgeon with difficulty succeeded in getting a gum elastic catheter into his bladder, and having succeeded he left it there for a month. When it came to be with-

drawn it had to be dragged out with great force, and, as the patient described it, it was encrusted with calculous matter, so that the end of it was as thick as his little finger. Of course the urethra was lacerated from one end to the other, and when he came under my notice the urethra beneath the penis, as far back as the scrotum, was gone. I could, with difficulty, introduce a small silver probe into the orifice, from which the water came. Indeed I could not do so until the patient, holding up the penis, forced out some urine from the little orifice in the angle between the penis and scrotum.

The whole urethra had, in truth, been destroyed, and there only remained a fistulous trajet from the bladder, where the urethra formerly had been. I gradually dilated this channel with bougies, but it narrowed up again with extreme rapidity. At last, in despair, I introduced the dilator, and burst it. It did no harm, but it did no good; the tendency to narrow was as great as ever. The patient was obliged to keep his disease at bay by the daily use of a bougie kept in for half an hour.

The second case to which I allude was a very similar one, in which almost the entire urethra had been made into a fistulous channel by a strong injection of nitrate of silver used to cure gonorrhœa.

No portion of the urethra had ever entirely sloughed away, but abscess had formed, and the fistula and the whole length of the urethra was of almost cartilaginous firmness. In this case there was great difficulty in dilating the urethra, and it narrowed again at once. I now also tried the dilator. Here also it did no harm, but it did not at all check the tendency to close again. After one day had elapsed it was impossible to introduce so large an instrument as on the day before.

I am clearly of opinion that it is not, in general, good practice in the treatment of stricture to introduce very large instruments, as No. 12 or 13. I have satisfied myself by observations made by injecting the bladder and urethra with wax, that even in subjects of large size, the portion of the urethra begirt by the triangular ligament rarely exceeds the dimensions of a No. 10 catheter. To dilate, therefore, beyond No. 10 or 11 is, then, to dilate beyond the normal calibre of the canal; the urethra is kept in a state of irritation by this, and generally resents such treatment. I am convinced by experience that when the regular introduction of an instrument is necessary in order to keep a stricture at bay, it is, as a rule, better not to go beyond what may be considered the normal size of the canal at the triangular ligament, and, as I have said, this does not generally exceed No. 10.

In three cases only of those which I have tabulated was there any decided tendency in the disease to return observed, and in only one to return promptly. To what was this tendency to return so quickly due? This is a question which I am at a loss to answer. I must content myself by recording the fact. Dr. Cruise, whose frequent use of the endoscope has made him familiar with the pathological changes in the urethra, is inclined to believe that the immediate plan will be found best suited for cases of true organic stricture, and that the obstruction arising from chronic inflammatory constriction, produced by the granular state of the canal, is less likely to be benefited by it. Yet we could find no evidence by the careful examination of the urethra in this case, that granulations had existed; but then it must be recollected that the stricture had been treated a second time, and, for so far, with success, at the time when the opportunity of examining it had occurred—*Dublin Quarterly Journal, Feb.* 1868, *p.* 53.

56.—ON THE USE OF CAUSTICS IN STRICTURE OF THE URETHRA.

By F. Le Gros Clark, Esq., London.

[The following is elicited by the condemnation of the use of caustics contained in a preceding paper (Art. 54) by Sir Henry Thompson.]

I have been in the habit of advocating and practising in public the treatment thus unsparingly denounced, and the results of my experience for many years as a hospital surgeon are entirely at variance with the observation and expressed conviction on this subject of Sir H. Thompson.

I have, of course, often performed perineal section in suitable cases: but have never divided a stricture within the urethra, because I have never found it necessary or desirable to do so. It has occurred to me that some danger might attend this operation; but I do not venture to assert that such the case, as I have not tried it. But I have for many years been in the habit of employing potassa fusa in the treatment of old and impenetrable stures, with the greatest advantage, and without in a single instance ing dangerous consequences. A suitable instrument, and care in using gether with a little patience, are requisite; but these are elements of ss in every operation. Many of my dressers can bear witness to the d successful application of this caustic, especially in bleeding stric- and I have from time to time recorded such cases in my published lectures.

Apology for this comment on Sir H. Thompson's lecture is, the desire to rescue a valuable remedy in the treatment of stricture of the urefrom being expunged, without protest, from the chapter on diseases of inary organs.—*Lancet, Feb.* 22, 1868, *p.* 271.

57.—VULCANISED INDIA-RUBBER CATHETERS.

In the case of old men affected with retention of urine without stricture, M. Richet, Maisonneuve, &c., employ, by preference, vulcanised India-rubber sounds, as do many of our own surgeons. With this flexible and simple sound, says Dr. Victor Reveillout, catheterism, so to speak, performs itself, even in cases where there exists considerable enlargement of the prostate. It seems to find its own way in the urethra with the utmost facility. It causes less irritation than any other when left in the urethra.—*British Medical Journal, Nov.* 23, 1867, *p.* 474.

58.—ON STONE IN THE BLADDER.

By Sir Henry Thompson.

It is stated in books that stone in the bladder is most common in children. [This is not the case, it is most common in individuals from fifty to seventy.] How do you sound? You should employ an instrument like this, with a small, short, curved beak, because it can be turned in any direction. If you use an instrument with a large curve, like a catheter, you are unable to rotate it in the bladder, and hence it does not explore sufficiently.

When I entered this room I asked for the hospital sounds, for I knew I should find among them a good example of what a sound ought *not* to be. Here is one, for example, which no one could rotate, or ever find a small stone behind an enlarged prostate with, except by sheer accident. You will ask, naturally enough, "Why are such sounds here, and who has used them?" They were used formerly, and found a good many stones, too, in the hands of our illustrious predecessors. But I will answer for it, they have missed a good many stones also; and this is precisely what I want you not to do. I have no hesitation whatever in saying that more stones are missed in sounding than are found, by the ordinary methods adopted in this country; and that must be the case if a sound of the form of the common catheter is relied on for the purpose. But with an instrument which has this small beak at the end of it, you can search in every direction. If there is a large stone, of course you can find it with anything; but our great object is to find the small stones. Anybody can find a big stone; the art consists in finding a small one. It is more important to find a small stone, because it

will grow large, and may be very formidable to deal with; whereas, when it is small, it is a far less formidable matter. You may promise the patient, in the case of a small stone, that it may be removed without risking his life; whereas, in the case of a large stone, there is always some risk, often considerable danger.

But you have something else to do besides merely discovering the presence of stone. It is necessary to have other particulars respecting it, because the nature of the operation to be performed will depend on these. First of all, it is essential to know what the size of the stone is before you decide on what you will do with it. From the note elicited by merely striking it you can get some indication of its size. It is often sufficient for practical purposes to use a sound (which I have long used myself, and have recently introduced) provided with a little slide on the shaft, which by proper manipulation enables you to ascertain very nearly the size of the stone, as you have frequently seen in the wards.

Then there is another way. You may introduce a lithotrite (which gives, however, a little more disturbance to the patient) and seize the stone in two or three directions, so as to ascertain its diameters.

At the same time you ascertain its nature. A phosphatic stone gives a very different sound from the others. The specimen before me is dry, and therefore will not give the sound to which I refer. When wet, it is spongy and soft, with a rough surface, and always gives a dull note when struck; whereas the uric-acid stone gives a hard ring. Then you will judge partly by the condition of the urine. If the urine is acid, and if, also, uric acid is thrown down, you may conclude that the patient has a uric-acid stone. If so, it is likely he has passed small calculi before. If the urine is very alkaline and there is a great deal of phosphatic matter, you may conclude that it is a phosphatic stone, or, at all events, it is covered with phosphates.

The number of stones is the next thing. Usually there is only one stone, but occasionally there are more. There is a patient here, whom I shall lithotrise to morrow, who has two rather large uric-acid stones in the bladder. The way to determine that point is this: Having seized one in the lithotrite, you move it gently in every direction as a sound for others. If then you encounter one on one side and one on the other, you know that there must be at least three stones.

I have spoken of uric-acid and of phosphatic calculi. But it may happen that you have an oxalate-of-lime stone—a very important thing to ascertain. You examine the urine, and see if there is much oxalate of lime thrown down. The patient may have passed a small mass of oxalate of lime before, and you may infer that an oxalate-of-lime calculus exists now; but then it may be covered up with phosphates, and so deceive you.

Having got all these data, the next important question is, what are you to do? Are you to cut, or to crush? You know there are only two modes of removing the stone. You must either make an opening sufficiently large to admit of its withdrawal, or you must crush the stone into very small fragments, so that they may be expelled by the natural passage. It was less important to make a diagnosis of all these points when we had but one operation—namely, that of cutting. Formerly, whether the stone was large or small, the patient was always cut. There was no other way of removing it. Now that we have two operations, it is very important that we should choose the right one, because, let me tell you, if you do not determine pretty accurately the characters of the stone, and select the right operation, you may do more harm than if you cut every patient. If you crush the very large stone, and cut for the very small one, you will have greater mortality than if you simply resorted to the one operation of cutting in all cases. When lithotrity was first introduced, it was rather a clumsy operation; and when the cases were not judiciously selected, when surgeons crushed without making a diagnosis of all these points—crushed stones that ought really to have been cut, and left for cutting stones which might have been crushed,—the entire mortality resulting from operations for stone was greater than

previously, when every case was cut. I cannot give you a stronger argument for the necessity of apportioning the operations judiciously.

Now, without taking up your time too much, I will lay down what you will understand to be the axioms which should direct you, in a general way, in making your selection. First of all, I will say that all stones, under puberty, with very few exceptions, are to be cut. Under fourteen or fifteen years of age, stones occurring in the male are to be cut, unless they are very small, and can be crushed, say, in one operation; because lithotrity is not a very easy or successful operation in children, the urethra being small and the bladder very irritable: whereas, as is well known, lithotomy is a very successful operation in these cases. We do not want a better operation, comparatively speaking, and one may be content to let well alone. Not more than one death in fifteen or sixteen cases occurs from lithotomy in children. I do not think, therefore, we can do better than to cut in these cases, as a rule.

That leaves us all the cases above puberty. Then I will say, in general terms, that all the cases above puberty are to be crushed, with certain rare exceptions. The first exception is in a case of an oxalate-of-lime calculus, which is, let us say, an inch in diameter. Under an inch in diameter you may crush an oxalate-of-lime calculus. I have crushed four or five in my time. The cases are very rare. Two of them were in this hospital. An oxalate-of-lime stone, from the size of a bean up to an inch in diameter, can usually be crushed; above that size no instrument can deal with it, and the fragments will be so hard that the operation might be of doubtful value, even if we succeeded in crushing. That, then, is the first exception to the general law that all cases in adults are to be crushed.

Secondly, a large stone of uric acid, or a phosphatic stone of *very large size*, had generally better be cut than crushed. Mechanically speaking, it is possible to crush any stone, whether uric or phosphatic: but considering the number of sittings required, and the amount of irritation produced, the single operation of cutting will be the better of the two when the stone is, shall I say, full two inches in diameter. A stone which is two inches in diameter, either phosphatic or uric acid, had perhaps better be cut. No doubt a rather larger phosphatic stone may be crushed. Here is a very large one, two ounces and a half in weight. The phosphatic stone is very friable, and you may deal with rather larger phosphatic stones than uric acid stones by lithotrity. So much for the exceptions to cutting, regarding the characters of the *stone* itself.

Now what are the conditions on the part of the *organs* which will make it necessary for you to cut instead of crush? If you have a bad stricture of the urethra you cannot crush; if you have a very highly diseased condition of the bladder probably you cannot crush; but the exceptions are very few indeed. First of all I will tell you what are not exceptions, but which are stated to be so in books, and are generally considered as exceptions; because lithotrity has advanced since the time of most books. I have recently crushed a uric-acid stone in a case of organic stricture with small instruments made for the purpose; but the stricture was not a very narrow one. Hypertrophy of the prostate was said to be an exception; it was said that you could not crush under those circumstances. I make no difference whatever in respect of that matter; I would as soon crush in the case of hypertrophied prostate as in any other. It is only a question of delicate manipulation. If the hypertrophied prostate occurs in a man who has had instruments passed, he will have become habituated to them, and is therefore a better subject than a healthy one who has not been so accustomed. Next, it was said that when the bladder could not empty its contents by its own power, and the urine had to be drawn off by means of a catheter, lithotrity was contraindicated, inasmuch as the fragments could not be passed. On the contrary, I rather prefer such a case, for the reason just assigned—that is, the bladder and the urethra are habituated to instruments; and as not to removing the fragments, there is no difficulty at all in removing the very last fragment, thanks to the improved methods now employed. It was said also that great irritability of the bladder was a reason why we could not

crush. It was said that if the bladder could not hold above three or four or five ounces of urine there would be no room for the lithotrite to work, and therefore the surgeon must cut. I make no objection on that ground, because the irritation of the bladder is due to the presence of stone; and as soon as you begin to get away the stone the irritation diminishes. Besides, it is not necessary to have four ounces of water in the bladder: one ounce is ample. There is no occasion to have four or five ounces in order to perform the operation of lithotrity. It might have been so with the old, clumsy instrument; but with modern instruments there is no necessity to protect the bladder from contact with them. When instruments were used that were to catch the coats of the bladder it was no doubt desirable to have a quantity of water in the bladder; but with modern instruments, which will not lay hold of the coats of the bladder, there is no difficulty whatever in crushing with a single ounce of water. I do not care whether the bladder is empty or contains a large quantity of water, provided only that it is not too full. Nothing is worse than too much water, because the stone rolls about, and you must figuratively speaking, play a game of hide and seek to catch it. It is better to have an empty bladder than a bladder with half a pint of water in it.

You see then that the exceptions are very few; indeed there are very few adult cases which cannot be crushed, provided you give proper care and attention. If the present generation of surgeons now growing up, progress, as they must, and become more intelligent and more careful than those who have gone before—if they are better acquainted with the subject, as they must in the nature of things be, as our sons will be wiser than ourselves, and our grandsons wiser than they,—there will be fewer and fewer exceptions; because if the stone is discovered when sufficiently small, *it can always be crushed with an almost certain chance of success;* so that lithotomy must at some day disappear, except for cases which have been strangely neglected by the patients themselves, or have been overlooked by the medical attendant.—*Lancet, April* 25, 1868, *p.* 521.

59.—THE BEAKED SOUND

By John Gay, Esq., London.

A reference to the comparative advantage of the modern over the old sound in the course of Sir Henry Thompson's lecture on stone, in this day's number of your journal, recalls my attention to the fact that in almost all works in which allusion is made to this instrument, its origination is accredited to those to whom, I believe, it does not justly belong.

The volume of your journal for, I think, 1845 contains a letter from me in which I detailed some observations which I had just then made on the bladder in the dead subject, for the purpose of showing what Sir Henry has forcibly reiterated—viz., that with the sound, *prior to that period*, it is impossible satisfactorily to search the bladder, inasmuch as the beak is too long to admit of that freedom of movement within the viscus that is essential to its complete exploration. After trying curves and beaks of varying sizes, I suggested the sound now in use, so far as its figure is concerned, and had a set made by Mr. Ferguson, of Giltspur-street, several of which I have still by me. I believe these were the first sounds that were ever made, or, at all events, used in this country, of the modern shape. The bulb at the end was a great improvement; but at whose suggestion it was made, I know not. It has been, I believe, accorded to Sir William Fergusson.

Had not the device of this "famous beak" been assigned to other surgeons *by name*, I should not have broken silence on the subject. As it is, I think I may be excused for seeking to give publicity to its true history, without incurring the charge of being over-nice about what some may deem "an inconsiderate trifle."—*Lancet, May* 2, 1868, *p.* 575.

60.—LITHOTOMY BY A SEMILUNAR EXTERNAL INCISION.

By Sir William Fergusson, F. R. S., Serjeant-Surgeon to the Queen.

[The operation of lithotomy, in consequence of the improvements in lithotrity, is now less frequently performed than formerly, but on this account it is not the less interesting. The interest in it indeed increases, for it is more dangerous, being confined to the worst cases, where lithotrity is inapplicable.]

Without discussing the applicability of the one or other of the two operations to the young, it may be affirmed as a maxim that between puberty and the age of fifty lithotrity will be the most suitable and the selected operation. Even above the last-named age a majority of cases will be most suitably treated by this method; and if any between puberty and extreme old age be rejected or set aside for lithotomy, it must be for certain unfavorable conditions and indications, which if likely to increase the dangers of lithotrity, will most assuredly have a similar effect in lithotomy. In other terms, it may be said that all the cases of stone in the bladder in the adult, with complications, are now set aside for lithotomy; whilst all the simpler cases most promising of success are treated by lithotrity. Hence, then, it comes that the use of the knife in this disease seems more fatal than ever. A surgeon who keeps to lithotomy alone in the present day is hardly to be tolerated; he is behind the age, and his contempt or ignorance of lithotrity renders his opinion of little value. But the accomplished lithotritist knows and feels that he has in a certain number of cases to declare that some are not favourable for that mode of treatment, and these, still seeking relief from suffering, are consigned to the dangerous ordeal of the knife. It is true that some of these cases do well, and it has perhaps been fortunate that they have been rejected from lithotrity; but as a rule, there is great danger in meddling with them. That which may be bad for lithotrity is equally so for lithotomy. The common example of the stone being too large to be crushed is one where cutting and extracting are equally, even more, dangerous. If lithotrity is thought or found impracticable, the patient's life need not be placed in hazard; but lithotomy once performed, sets him in a condition of jeopardy which may baffle the utmost skill.

These views have gradually acquired force in my mind, as experience in both operations has increased. Simple cases are selected for lithotrity—severe are set aside for lithotomy; hence the latter proceeding is more hazardous to life, and seemingly less successful than ever. That these conditions will acquire even greater prominence in time I feel strongly convinced; and as I am equally of opinion that, with our present knowledge, lithotomy will still remain a great and important agent in the hands of man to give relief from extreme suffering, it may be said to be more incumbent than ever upon the surgeon to endeavour to render the proceeding, if possible, less difficult and less dangerous.

[The oldest record we have of any lithotomy operations is that of Celsus, and it is a revival of this operation which is aimed at by Sir William Fergusson. The haphazard incisions of Celsus about the neck of the bladder are of course out of the question—it is to the external incisions alone to which reference is made. The medinal operation of Allarton is merely a modification of the Celsian, the means of dilating being alone somewhat different.]

From the time of Marianus to the present day the Celsian incisions have been referred to seemingly more for the sake of the curious than for the sake of instruction—more to deter than to attract. There is no evidence that Cheselden, who was perhaps one of the boldest experimenters among the lithotomists, had ever tried the method; and even Dupuytren's semilunar cutaneous incision was perhaps more to test the double-bladed lithotome than to show any feature of the operation of the Celsian date.

Of this I am sure, that throughout my own personal experience I have never heard a proposal to revive any of the steps of this most celebrated operation. *Cutting on the gripe* has been nearly obsolete from the time of

Marianus, and the only instance I can think of as modern is that related by Samuel Cooper in his "Surgical Dictionary." With the all but universal neglect, if not condemnation, of the operation, no one seems to have drawn any distinction between the external lunated incision in the skin and the formidable wound in the neck of the bladder, devoid as much of anatomical knowledge as the gripe was of mechanical skill. The anatomist can scarcely contemplate the deep incision in the Celsian operation without a shudder; yet if he considers the superficial wound, it may be difficult to point out a defect. My own impression regarding this is so strong that it constitutes the real object I have in view in offering these remarks.

In almost every instance in which I have observed difficulty in the operation of lithotomy, whether in cutting, in seizing the stone, or in extracting it, a short external incision has apparently been the cause. The cutting instrument has not been easily used in the deep part of the wound; the point of the left forefinger has not been easily passed into the bladder; the same with the forceps, and the blades have not been readily opened; nor has it been possible to extract without swaying the wound in the skin as it has been stretched over the instrument and, in the last movements, over the stone. Much of the latter part of lithotomy is done with the forefinger of the left hand, and when the external incision is short its margins will appear to encircle the finger, so that there seems scarcely room to introduce the knife for the deep incisions. It is under such circumstances that the prostate is likely to be pushed before the point of the finger, and that the operator is likely to fail in reaching the bladder. When the perineum is deep, the further progress of the finger is arrested at the cleft between it and the middle one, and additional pressure merely pushes the skin towards the deeper parts. If it so happens, however, that the external incision is free, the finger readily passes deeper, so that the knuckle gets into the wound—a thing impossible in the short incision. When the finger and instruments can be easily passed into the deep parts, the latter and nicer steps of lithotomy can be more readily accomplished, and therefore, in my opinion, with greater safety to the patient. Although fully aware that the operation has been most satisfactorily accomplished through an opening more like a stab or a round hole than an incision, I nevertheless consider the style very objectionable, and its happy results as exceptions to the general rule.

I have an impression that such palpable objection to a limited external incision had much influence in leading to a change from the Marian operation. It does not, however, appear that Jacques's incision at the side of the perineum was made with a view to length, but gradually, as the lateral operation became developed, a free opening in the skin in front, at the side, and below or behind the anus, was made an important feature; and the modern surgical anatomist has specially indicated that the knife may be very freely used between the anus and the tuberosity of the ischium to a considerable depth, as no important tissue lies in this locality. There is, however, a limit here to which the knife may be used with impunity, for the rectum, particularly if it be capacious, is in jeopardy if the incision be free and deep. Yet since Jacques's period, and particularly since the precise anatomical epoch of Cheselden, the lateral incision in the skin of the perineum has held its ground with but slight exception, and it may emphatically be called *the* mode of lithotomy of the present day.

Although I have practised the operation all my life, with a few exceptions, entirely in accordance with the views of Cheselden, I have from an early period had doubts if we were even yet on the right line for perfection. I have doubted if, after all, the side way has been the best either into or out of the bladder; and I have ever had in view the indications of Nature in this region. The contents of the bladder, of the vesiculæ seminales, although coming from the side, find their exit through the mesial line, and the *débris* from the bowels takes departure in like manner. But, above all, the ingress *and egress* to and from the uterus seem to me to indicate Nature's intentions *as to a middle* or central passage. The vagina must be allowed to be the *via sacra* between the urethra and rectum in the female; and although the

analogy I mean to draw does not hold good as regards the ends of the vagina, or, in other words, the os uteri and the vulva, still I imagine that I may claim a propriety in the difference between the surgeon's operations and those of Nature, and at this distinction my lecture aims.

The grand immediate objects of lithotomy may be said to be an easy entrance to, and an easy exit from, the bladder, both being secured without unreasonable damage or risk to the patient. The old mesial or Marian operation seems to have been so far destitute of these qualities as to have been in time utterly condemned and given up. With all deference and with all respect to such of my contemporaries as have practised or advocated the revival of this operation.—for I call it nothing else in its main features.—I make bold to say that the same fate must befal this recent innovation. From personal observation, and from what I have read and heard, I believe that more mishaps have occurred proportionately in this operation than by the ordinary lateral, with which all practical surgeons are unhappily too familiar.

The theory of the lateral operation is, upon the whole, excellent. The external wound is in a thoroughly safe line as far as regards damage to important structures; and the same may be said as regards the deeper passage to the bladder. The bulb of the urethra, and the rectum on the mesial line, the common pudic artery on the outer side, are intended to be thereby avoided; and the gliding between the accelerator urinæ and erector penis, without damage to either, seems a feat to gladden the heart of the physiological surgeon. True the transversus perinei must suffer, and so must some of the levator ani; but then, again, the division of the lateral lobe of the prostate in its long axis, about midway between the reflected portion of the pelvic fascia and the vesiculæ seminales, indicates that perfect precision in which the surgical anatomist delights. That such theory is generally realised in the lateral operation I thoroughly believe; yet I have my doubts if this really be the easiest and safest way to and from the bladder. Although I have followed the practice, as a rule, ever since I began to perform lithotomy —a period extending over thirty-five years—yet I have long been sceptical on this subject; and, as a proof of this, I may refer to my temporary adoption of Dr. Willis's proposal of lithectasy in 1842; and, again, to my proposal in that operation for an incision, the upper line to be in a part of the raphé, and the sloping lines on each side of the anus. Neither of these proposals seemed to take with the surgical mind in Britain; although the latter has been extensively and successfully used by Dr. Eve, of Nashville, as I have lately been told by that gentleman himself. Even with the experience I had acquired twenty years ago, I felt diffident about seeming to question the perfection of Cheselden's operation, and so I went on as best I could with the time-honoured lateral. The old feeling, however, cropped up again in me, and still leaning to the central part of the perineum, I ventured on another innovation. Instead of beginning the lateral incision on the left side of the raphé, I ventured more and more to the right, beginning a little to the right of the raphé, and then a little more; until, possibly, had I gone on, I might have tried a transverse wound between bulb and anus, from tuberosity to tuberosity. I have been told that these views, good or bad though they may be, are not original—that this gentleman and that gentleman made such and such incisions. But I have failed to find any record of the practice, nor am I disposed to say much about it; although I have fully satisfied myself that crossing the border from one side of the raphé to the other was in no wise detrimental, the early impression on my mind (erroneous as it proved) being to that effect. But a direct transverse line did not satisfy my imagination so much as a longer oblique one, and at last I boldly ventured on doing what I had never seen or read of in modern surgery: I performed lateral lithotomy as regards the deep parts of the wound and prostate, beginning with the famous, but antiquated if not despised, semilunar incision of Celsus. On the 16th February, 1867, I cut a boy six years old, at King's College Hospital, in this manner. The operation was simple and easy; I may almost say, more so than is usual with myself. The recovery *was perfect and without* a bad symptom, although the wound healed

more slowly than is common after the lateral operation at this age. On t
1st June following I had an opportunity of performing similarly on t
adult—on a patient forty-eight years of age, in whom lithotrity had failed
on that account a case of extra danger, in accordance with the views
forth in the early part of this lecture. The result was highly satisfacto
The particulars of these two cases were published by the reporter for
Lancet in the number of that journal for July 13th, and since then I h
had a third successful case in the hospital, on a patient aged 64. An exa
in private practice, on a patient aged 59, with most unpromising adju
has also been crowned with a successful issue; and with this experienc
one under puberty and three in the adult, with questionable data—I ven
to bring this subject in this public manner before my professional brethren.

In all these cases the wound seemed to me to heal more slowly than in successful lateral lithotomy; but whether this was a coincidence in each, or the result of the peculiar line of incision, I cannot take it upon me to say. My impression is that it was only chance, but I should not quarrel with any who think otherwise, nor do I deem it a point of much importance, provided there be advantages otherwise.

It may be said that four successful cases are not sufficient to display any advantages to induce a trial of a new operation, and in some respects I willingly admit this; but it must be borne in mind that I write with an experience of nearly 200 lithotomies by my own hands, and it may be admitted that I should be allowed an opinion as to the peculiarities or features of different methods which I have myself tried. On this score I can say unhesitatingly that I have been greatly pleased with the proceedings I refer to.

Although I speak of the Celsian operation, I beg it to be observed that this differs as essentially from that method as did the later operations of Jaques with those he first performed in Paris. When he began he cut at hap-hazard, and without a guide, into the bladder; but latterly, after being driven in ignominy from the capital of France owing to want of success, he used a guide—a staff as it is now called—into the bladder, and then it is supposed that his greatest triumphs came. The operations of Raw and Cheselden included this important feature, but it nowhere appears that there was any guide into the bladder in the Celsian operation, excepting the uncertain bulging of the stone as pulled and pressed downwards by the fingers of the surgeon introduced into the rectum. I have heard of Mr. So-and-So again, as in the oblique and transverse incisions, having made the Celsian incision in the skin with a staff in the bladder, but I know of no record of any such thing, and hence I am induced to call the attention of my brethren to the subject.

It may be said that Dupuytren's operation embodied all that I seem to intimate as new in this paper—the staff and the semilunar incision; and in some respects this may be admitted, but the double incision in the prostate and the use of the lithotome caché seem to have made little, if any, impression on the minds of modern lithotomists. Neither the double-bladed lithotome of this distinguished surgeon nor the single-bladed instrument of Frère Côme has taken with English surgeons, probably in consequence of the decided preference of the operation of Cheselden.

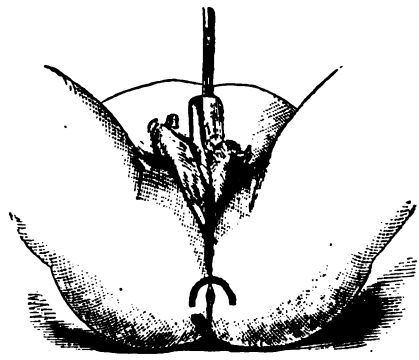
The proposal that I now make is more in accordance with that celebrated proceeding, and the chief difference is in the external incision. I have already referred to the seeming safety of that incision, and also to some of its dangers, but something more may be said on these points. When the perineum is presented in position for the commencement of lithotomy, the raphé usually appears as a hollow (excepting in very fat subjects) bounded on each side by the ischium. The external incision, being intended to run midway between that bone and the mesial line, is carried along the highest part of the surface—for the operator dare not go near the ischium, knowing the danger to the common pudic incurred thereby, and so he begins at the furthest *distance* or highest eminence from the bladder. In other words, it may be *stated that* a wound here, to reach the neck of the bladder, must be deeper *than one in* the middle of the perineum. The deeper the wound, the more difficult,

and I may add the more dangerous, the operation; and, most notable of all, the seizing and the extraction of the stone are more likely to be difficult through a deep wound than a shallow one, particularly when that wound or passage for the stone is limited to one side of the perineum. The objection which I have always felt to the Marian or mesial incision is that, being a straight line from over the bulb of the urethra to the front of the anus, it has been too limited to give facilities for the deeper part of the operation, and incapable of giving a fair and safe exit to a stone above average size. I admit, as a practised lithotomist, that skin and other tissues may be compressed or pushed towards the bladder in making way towards that organ, and again that when the stone is between the blades of the forceps the neck of the bladder may be drawn towards the skin, so as to make the perineum appear comparatively shallow; but I believe that both processes are highly objectionable in lithotomy, and the easier that the bladder is entered, and the easier that the stone is removed from it, the better. This leads me again to advert to the midway of Nature. When great space is required, as in egress of the head of the fœtus, the expansion takes place towards the ischia and the coccyx, and such should be the aim of the surgeon in dealing with a large stone. I am disposed to think that a semilunar incision facilitates this in a more efficient and satisfactory manner than any other. Here it might be said that the mesial incision carried into the rectum would fulfil this proposition, and so I believe it would; but all experience goes to show that such a wound of the sphincter and front of the rectum as this implies, is highly objectionable. There is no evil in the semilunar incision greater than that of the lateral; on the contrary, there seems to me less. For in this central locality there is in reality no vessel, nerve, or part to demand the surgeon's caution; the bulb and rectum are certainly in no greater danger, and assuredly the common pudic and its perineal or scrotal branches are in less. The great peculiarity of this semilunar incision, however, is that it gives such an extensive external opening beyond what in this locality any single straight line would. The gap opens something like the clasp of a purse's mouth; and the points of two or three fingers can be placed in it at once, so as to depress the anus and lower part of the rectum, and permit the knife to be freely used between the bowel and bulb and on the lower part of the triangular ligament. With sweep after sweep of the knife in the semilunar direction, a close proximity with the bladder will have been made; and now the operator may take his choice as to the steps of the future stages of the operation. Years ago I thought of using a blade of this kind after making an opening into the membranous portion of the urethra:—a sort of miniature double gorget, intended to be carried with its probe point along a groove on the convexity of the staff, to notch both sides of the prostate; but being unwilling to change the cutting instrument, I resolved to try with one, and so, using the same blade which I employ in the ordinary lateral, to make the semilunar cut. I have at this stage aimed at the membranous portion of the urethra, and thereafter cut into the lateral left lobe of the prostate with the same blade in the usual way.

Thus I have combined the time-hallowed, or rather time-neglected, semilunar incision described by Celsus for cutting on the gripe and the modern lithotomy of Cheselden of cutting the left side of the prostate on a staff. All parties since the time of Jacques, who is said to have used in his latest operations a guide, director, or staff, to render his internal incision more precise, have agreed on the value of this great addition to lithotomy, and I need scarcely add that the staff is an essential feature in the operation to which I now draw attention.

The performance differs from the ordinary method only in the external incision, but a short description may be desired. The staff being introduced, the patient is to be bound and held in the ordinary way. The usual lithotomy knife, like a scalpel in a strong handle, is to be held in the right hand with its cutting edge directly upwards; the point is then to be introduced into the *skin on the* right side of the perineum, midway between the *anus and the tuberosity* of the ischium and a little lower than the anus. It

may be inserted a quarter or half an inch deep; and, by a pushing or gliding movement upwards, the right side or end of the semilunar incision is made, then the curve, and then the slope on the left, so that a line like that indicated



here (see illustration) is completed. During the movement the knife is gradually turned round with the hand, whose radial margin, from being first turned downwards, is ultimately upwards. A second similar sweep, with the extreme ends not so deep, should next be made, when the fore and middle fingers of the left hand should be pushed into the wound between the bulb and rectum, with the intention of separating those parts: it will then be found that the anus and rectum can be depressed with peculiar facility. Now, too, with possibly a touch or two in the middle part of the wound, it will be found that the finger readily slips into the space between the erector penis and compressor urethræ; and the staff may be felt through the tissue between them and the membranous portions of the urethra. By a dexterous push, the point of the knife may be introduced into the groove, or perhaps a little further cutting may be used to divide a portion of the lower margin of the triangular ligament and part of the levator ani, or these may be divided in withdrawing the knife. The point of the blade should be pushed along, say, one-half of the membranous portion of the urethra, into the prostatic portion, so as to notch or cut freely the left lobe of this organ, or the latter may be done in withdrawal. Should the gorget be preferred, it may be used after the opening has been made in the membranous portion. The subsequent steps are the same as in the ordinary lateral operation, excepting that the forceps may be held more to the right, and withdrawn through the middle of the perineum, instead of the left side, as in the common way. Should the stone be large, the right side of the prostate can be more readily reached through this wound than through the lateral, and the good rule of pulling obliquely downwards in extracting can be more efficiently accomplished than through a wound in the side.

The after-treatment is in all respects as in the usual operation, a tube being used or not, in accordance with fancy or fashion. The skin in front of the perineum immediately behind the scrotum being left entire, the wound is scarcely observable as the patient lies on his back. In one of my cases a surgeon of experience on looking at the perineum would scarcely believe that lithotomy had been performed until he made a closer examination.

With the limited experience of only four cases, although three of them were adults, and comparatively unfavourable for operative interference, it would be wrong and beyond my wish to refer to this proceeding as specially superior to lateral lithotomy; yet I think it worth while to call the attention of my professional brethren to it from the conviction that it deserves trial, and with a hope that it may in some degree simplify, in regard to performance and danger, one of the most formidable and interesting operations in surgery.– *Lancet, Jan.* 4, 1868, p. 1.

61.—ON LITHOTOMY BY A SEMILUNAR EXTERNAL INCISION.

By Sir Henry Thompson.

I venture to think that the following facts, very briefly related, will form a not unimportant contribution to the subject discussed in the interesting communications of Sir Wm. Fergusson and Mr. Erichsen.

Sir Wm. Fergusson has adopted, in four cases of lithotomy the external incision of Dupuytren's bilateral operation, combined with the internal incision of the ordinary lateral operation.

Mr. Erichsen has adopted in one case the original operation of Dupuytren—that is, the external semilunar incision, with an internal one, similar in form and direction, made with the "lithotome caché," in both lobes of the prostate.

Now the most important point, it must be conceded, is the *internal* incision; that the external one, wherever it is, should be free, is agreed on all hands. Mr. Erichsen argues in favour of the division of both prostatic lobes, in order to obtain room for a large stone.

That incision I have now practised for several years, on these very grounds—viz., that the internal opening in median lithotomy is insufficient for any stone above a moderate size (and such are almost always crushed), and that the internal opening in lateral lithotomy is less safe than that made by the lithotome caché. The first time I used the lithotome caché for the bilateral incision of the prostate was November 19th, 1861, making the external incision in the median line. Mr. Erichsen kindly held the staff for me on that occasion.

During the last three years I have operated *in the same manner*, on ten more adults, in all eleven cases, and of the respective ages of 43, 71, 73, 68, 80, 69, 55, 78, 45, 63, and 67 years. The three largest calculi so removed weighed respectively 3½ oz., 2¾ oz., and 2½ oz. The first and second of these three cases are still living, and were, like several of the others, cut in University College Hospital. It is true that any experience of eleven adults reckons for little in an estimate of mortality; still, it is the largest at present existing in this country, and the results with it, the ages considered, are satisfactory.—*Lancet, Jan.* 25, 1868, *p.* 132.

62.—ON MEDIO-BILATERAL LITHOTOMY.

By Vincent Jackson, Esq., Wolverhampton.

[The following article explains a plan adopted by Mr. Jackson for the removal of stone from the bladder of the adult in cases unfitted for crushing.]

On October 10th, 1867, I performed at Wolverhampton General Hospital lithotomy in two cases; one, a boy, cut laterally; and the other an elderly man, aged sixty-seven years, from whom I removed a large flat stone after the following method:—A large curved staff, deeply and widely grooved on the convexity, being introduced into the bladder, and the patient tied up, and placed in the usual lithotomy position, I inserted my finger into the rectum, and placing it firmly in front of the prostate, I entered a sharp-pointed straight-backed knife as near above the anus as the safety of the rectum permitted; and pushing it on to the groove of the staff, I freely divided the urethra, and as it was withdrawn I incised upwards in the median line the skin and superficial structures as extensively as prudence allowed. The left forefinger, now removed from the rectum, was passed into the wound, and its point (nail downwards) lodged in the groove of the staff, sufficing as a guide to a broadish straight gorget, with a central projecting probe point, and a limited bilateral cutting edge. This was easily and gently pushed on into the bladder, incising as it advanced the prostate bilaterally from before backwards, and along its upper hollowed-out surface the finger slipped into that viscus. The former was now removed, and as small a pair of forceps

as could be effectually used being introduced, the stone was seized and extracted.

The above operation I have had in my mind for many years (I practised it frequently in the dead-house of University College Hospital when I was house-surgeon, dissecting carefully the parts afterwards, and I have also in my possession some recent dissections); and although I have had many opportunities, I never resorted to it previously, having been satisfied hitherto with the lateral method in all cutting operations. But the above case being in many ways a very unfavourable one for operative interference, cystitis to a considerable extent existing, and the man's health being completely broken down with a long life of drinking, and of late years with much suffering, I resorted to it, trusting to reduce the risk to a minimum, and also bearing in mind the greater facility which, in my opinion, the median section of the perineum offers to arrest venous bleeding, if it occurs, by a more complete and perfect plugging of the wound, either around a tube or upon a catheter; and in this particular case I anticipated some trouble from this danger.

In conclusion, I would remark that, however excellent the architect, and however good the workmanship, time and accumulated experience must settle the worth of a thing; so we have yet to look to the future to determine the question whether a better or a safer operation can be devised than the lateral method of Cheselden.—*Lancet*, *March* 7, 1868. *p.* 330.

63.—MEDIAN LITHOTOMY WITH BILATERAL SECTION OF THE PROSTATE.

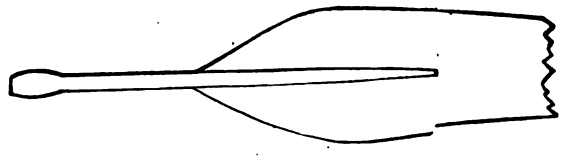
By Jonathan Hutchinson, Esq.

[On reading the preceding paper by Mr. Vincent Jackson, Mr. Hutchinson discovered that he had been working in the same direction as Mr. Jackson with regard to improvements in lithotomy instruments.]

During the last ten years I have had made, and have repeatedly used, both on the dead and the living, various modifications of the double-cutting gorget, with probe-point, and designed for use with a median incision. The latest which I have had made is, I should suppose, almost exactly like that contrived by Mr. Jackson, and I am very glad to read his testimony as to the easy use and the neatness of the section of the prostate which it makes. I believe it will prove a very valuable instrument, and I was only waiting for further experience before bringing it under the notice of the profession.

Although our knives, or rather gorgets, are, I expect, alike, and although we both propose a median external incision, yet I am sanguine that I have got a little further than Mr. Jackson in one or two points. My staff is rectangular, and at the end of the groove is a little thickening to receive and retain

Fig. 1.

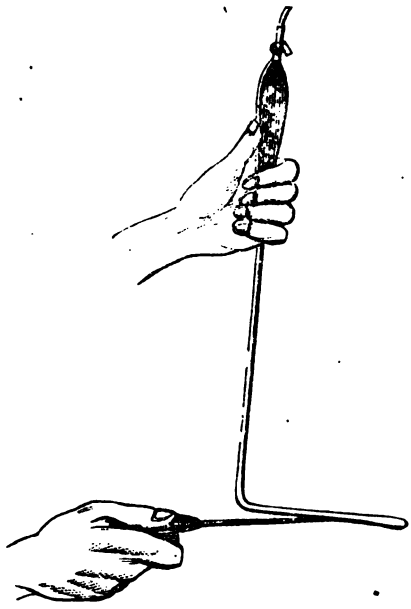


the probe-point of the gorget. Instead of making the gorget move on a staff which is fixed, I pass the probe end of the former fairly into the thickening, and having thus locked both firmly together I then give the staff from my assistant, and pass both together forwards and fairly into the bladder. Thus the great danger of the cutting gorget, that it may slip from the staff, is obviated. With a rectangular staff the whole of the move is quite straight, and the probe finds its way in much more easily than I can describe. There is not the slightest difficulty in introducing it.

accompaying woodcuts will explain my instruments.

ig. 1 the flat surface of the gorget is shown, and the proportionate of its probe end. It is taken from one made for me by Krohne about ago.

Fig. 2.



ig. 2 the gorget and staff are seen together as they would be just prior final thrust. The dotted line indicates the part where the probe joins de. The whole of the probe is concealed in the thimble and groove staff. The blade is of course seen edgewise.

advantages of the proposed method are: 1. Median incision of exter- ts, and diminished risk of hemorrhage. 2. Ease, certainty, and celer- performance. 3. A bilateral section of the prostate of determinate size, to afford plenty of room, and yet certain not to wound the capsule. certainty that no injury will be inflicted on the important parts in or of the prostatic urethra.

merit of the invention of the rectangular staff, of course, rests with uchanan, of Glasgow. He, however, proposed its employment for a kind of operation. I have never myself performed his operation, hink very highly of the convenience of the rectangular form of staff. uchanan thought the instrument not suited for the ordinary lateral op- , and depreciated in strong terms its use for that purpose. I have not his prognostications verified. I believe I have performed upwards of ateral lithotomies on a rectangular catheter staff, and certainly have ntion to go back to the curved one.

lst on this subject, may I be permitted to express my conviction as extreme importance of the use of a tube after the operation. If this eneral, I believe it would prevent many deaths. Especially is it le in the adult, in whom the long, deep wound causes risk of infil- of urine. The tube which I use was described in the columns of the about eighteen months ago. It consists of a large silver canula, at the end. Through its whole length is passed a continuous piece of , which, projecting into the bladder, soaks up the urine, and conveys with a degree of certainty which a mere tube would not afford. In- f silver, an india-rubber tube, of a quarter-inch bore, might be used,

with, of course, the sponge passed throughout, and projecting well at each end. The outer end of the tube may be received into an india-rubber bag or larger tube, and thus the patient's bed kept dry during the first few days. Of my last eight lithotomies *in the adult* (most of them in old men), all but one have made good recoveries; and I attribute the success very largely to the use of the tube. The man who died was believed to be the subject of diseased kidneys at the time of the operation.—*Lancet, March* 21, 1863, *p.* 391.

64.—ON LITHOTOMY BY A SEMILUNAR EXTERNAL INCISION

By JOHN E. ERICHSEN, Esq., Senior Surgeon to University College Hospital.

[Mr. Erichsen agrees in the main with all that is contained in the preceding paper by Sir W. Fergusson. As, last summer, he removed a large calculus through a semilunar external incision, he reports the case, now that an interest in the subject has been excited by Sir W. Fergusson's paper.]

The operation was performed on the 7th of August. The patient having been placed under the influence of chloroform, Mr. Erichsen passed a grooved rectangular staff into the bladder; and the stone being at once felt, he proceeded to perform the operation after the bilateral fashion. A transverse crescentic incision was made in the perineum. The centre of the incision was half an inch above the anus, and each extremity of it about the same distance from the tuber ischii. The dissection was carried down in the line of the superficial incision to the central point of the perineum, so as to separate the bulb from the rectum. The scalpel was then thrust onwards into the groove of the staff, through the membranous urethra, and just in front of the prostate, its blade being directed upwards. The knife was now drawn backwards out of the wound, cutting upwards as it was withdrawn so as to form a vertical incision in the superficial structure, of about an inch in extent, in the middle line, and communicating with the centre of the crescentic cut originally made. The knife being withdrawn, a lithotome caché was introduced along the groove of the staff; and by the withdrawal of this (the handle of the instrument being at the same time depressed) both lateral lobes of the prostate were divided to the extent of about three quarters of an inch—the distance to which the instrument had been previously adjusted. The lithotomy forceps was then introduced into the bladder, and the stone was at once seized. The withdrawal of the forceps was effected with some little difficulty, owing to the large size of the stone, and to the necessity of changing the direction of its axis in their blades. The calculus on removal was found to be of an oval form, with a long diameter of about two inches and three quarters, and nearly two inches in a transverse direction, and from its surface several irregular nodules projected. It weighed three ounces and one drachm. Its external surface was composed of uric acid. A tube was passed into the wound, and left for about forty hours, the urine coming away freely through it.

Aug. 10. The report continues as follows; Pulse 84; the patient going on well. From this time he progressed uninterruptedly (with the exception of a slight attack of diarrhœa, which was easily checked,) taking his food well, sleeping well, and suffering no pain whatever.

On the 22nd the urine ceased to flow through the wound, and came entirely by the penis.

The wound granulated healthily, and, occasionally being touched with nitrate of silver, was almost entirely healed when he left the hospital on the 11th of September.

It will be seen by a reference to this case that I performed the operation by Dupuytren's method. This plan, as is well known, is the original "bilateral" operation by means of the "lithotome caché," a most beautiful and ingenious instrument devised expressly for it. Civiale modified this operation by making the external cut perpendicular in the mesial line, but retaining the double section of the prostate; and Sir W. Fergusson has still further

modified it by confining the section in ordinary cases to the left lateral lobe of the prostate.

The principle of all these three operations appears to be the same—namely, to reach the neck of the bladder through the mesial line, where it is nearest to the surface. The difference in the details is, however, great, and I think most important.

So far as *external* incision is concerned, Sir W. Fergusson adopts Dupuytren's method—the crescentic transverse; thus getting wide space, enabling the rectum to be fairly separated from the bulb and well depressed, and cutting below or rather behind the arteries of the perineum. In all these respects I think this incision is preferable to the perpendicular external cut of Civiale and Allarton.

In the internal incision—that through the prostate and neck of the bladder—Civiale nearly follows Dupuytren, making the cut equally into both lateral lobes by means of a lithotome caché; whilst Sir William confines his cut, except in certain cases, to the left lateral lobe, and makes it with the scalpel.

To my mind the double cut appears the preferable method, as by it an equal extent of incision can be made in the prostate with less danger of passing beyond its limit. Suppose, for the sake of argument, that to extract a calculus it is desirable to make a cut eight lines long into the prostate. If one lobe only is incised, the cut must go to the full extent in it. If both lobes are cut, only four lines need be divided in each of them; and so proportionately to any extent. As the whole under-surface of the prostate is exposed by the transverse external cut, it is as easy to divide both lobes as one. This may be done with the scalpel, and, in such dexterous hands as those of Sir William Fergusson, with perfect safety; but for a less expert master of our art I think the lithotome caché a safer instrument, as by it the extent of incision can, without possibility of error, be regulated to a hair's breadth.

There is one practical point in all these mesial operations of lithotomy—whether Allarton's, Civiale's, or Dupuytren's—which I consider of some little importance. It is the use of the rectangular in preference to the ordinary curved staff. I have now used it several times in Allarton's, and Civiale's, and look upon it as possessing three very decided advantages over the curved staff. The first is, that its angle can be placed directly in the membranous part of the urethra, and held there until the knife enters its groove. Thus it becomes an unerring guide to the exact part of the urinary canal that we wish to open. The second advantage is, that it carries the urethra away from, instead of, as the curved staff does, down against the rectum. And the third is, that from the angle onwards the course of the groove is straight and direct, so that the beak of the lithotome or the point of the scalpel is carried on in a straight instead of a curved direction.—*Lancet, Jan.* 11, 1868, *p.* 41.

65—ON LITHOTOMY BY A SEMILUNAR EXTERNAL INCISION.

By Dr. J. Ogden Fletcher, Manchester.

[The following is elicited by Sir Wm. Fergusson's paper.]

During the years 1846–8, the late J. A. Ransome, Esq., very constantly operated, at the Manchester Infirmary, in the way suggested by Sir W. Fergusson, with the trifling difference of extending the left crus of the semilunar incision about two lines lower down, between the rectum and ischium on the left side, "to allow the urine a more easy escape."

The reasons given by Mr. Ransome for adopting this combination of the two acknowledged operations were those so well set forth by Sir W. Fergusson. I have notes of more than twelve cases operated on in this way by Mr. Ransome with very good results. And if my memory serves me well, Mr. Jordan has many times operated in this way; but, as I have not any

notes of his cases, this I give from memory alone.—*Lancet, Jan.* 8, 1868, *p.* 105.

66.—ON THE EXTERNAL USE OF DIGITALIS IN SUPPRESSION OF URINE.

By J. D. Brown, Esq., Haverfordwest.

Case 1.—Mr. H., a healthy young farmer, aged 23, was suddenly seized with severe pain in the bowels and back. I saw him at the end of seven days. Bold treatment had been enforced by Dr. John Thomas, of Narberth, such as bleeding, sweating, blistering, warm baths, but in vain. A catheter was passed on my arrival, and about a teaspoonful of urine was removed, highly albuminous. His condition was nearly hopeless when we met the next day. Vomiting and nausea prevailed, with heavy dull pains, and he was evidently sinking. It was now the ninth day, when we agreed to try the effects of digitalis. It was useless to administer it by mouth, and there was no time to lose. It was the month of May, and digitalis was plentiful. A poultice of leaves, bruised and warmed in boiling water, was applied at 12 a. m. We left, saying that if no urine came away by six in the evening a fresh poultice was to be applied. We had no sort of hope of the patient's recovery, and communicated our opinion to the friends on leaving. By 6 p. m. no urine. A fresh poultice was applied to the abdomen. About 10 that night urine passed. At 4 a. m. I was sent for, and Mr. Thomas, who lived nearest, got to the house by 6 or 7. The messenger, to my utter surprise, said that they could not stop him making water. He had then made eight ordinary-sized chamber vessels full, and was still making it when he left. We met at 10 a. m., but he never rallied; the drain was too much. Digitalis had been badly handled by us. We left no guide, no rule, and too much was absorbed. It did well, but went beyond its work. He lived till night.

Case 2.—A. R., servant, aged 45, subject of renal calculus and gout, was suddenly seized with suppression, but had no great amount of pain beyond what he usually suffered in passing small stones. I saw him on the fourth day, in consultation. All the usual remedies had been tried in vain. It was winter, and digitalis was given, but not in bold and sufficient doses at first. At the end of the sixth day it was boldly given in large doses, and a poultice applied on the seventh day; urine passed freely, and all went well. He still lives in his usual health.

Case 3.—Mrs. L., age 50, subject of renal calculus. Suppression came on, with vomiting and the usual symptoms. At the end of four days, every other plan having been tried, I recommended the poultice. It was winter, but leaves were obtained and applied as usual. I returned in six hours, when two chamber vessels full of urine had been passed. All the symptoms gave way, and for two days she continued to do well. Again suppression came on; she was neglected by her attendants, and the poultice was not again applied. She sank at the end of the twelfth day, no water being passed except for the three days when digitalis was in the field.

Case 4.—Mr. G., a gentleman about 40, subject of renal calculus, having passed one year ago, in good health up to Friday morning, when he was suddenly seized whilst dressing with intense agony in the right renal region. His medical attendant, Dr. Rowlands, of Carmarthen, saw him immediately. subdued the pains, but, to his surprise and vexation, no urine passed. A catheter was passed; there was no urine in the bladder. The usual treatment (baths, leeches, and opium) was actively employed without avail. Dr. Lewis, Carmarthen, was then called, who persisted, in conjunction with Dr. Rowlands, with the remedies. On Monday, I saw him with Dr. Lewis. I told him of my success with digitalis; he was surprised, but did not like to undertake its management unless in concert with Dr. Rowlands, who was then out of town. We agreed to give it in one-grain doses every four hours *until Tuesday* morning, when we again met. We then agreed to apply the *poultice.* It was May, and we procured plenty of fresh leaves. Dr. Ro-

lands and myself made and applied it, He undertook to watch the pulse, which we took for our guide. It was 109 at 11.30; catheter passed no urine; few drops of blood; poultice applied over abdomen; in ninety minutes, pulse 75; poultice taken off. A call for the chamber vessel; a good stream of water; in twenty-two hours, seventy-five ounces were passed. Specific gravity 1·16. Acid, slightly albuminous, a little blood, and casts. Improvement kept pace. Friday: Symptoms of stone again, which gave way to the usual treatment. Saturday: Plenty of urine; recovery complete.

Case 5.—Mr. R., aged 54, subject of renal calculi, was suddenly seized with symptoms of renal calculi passing off in December, 1867. Mr. Hicks, of St. David's, and Mr. Howell saw him, and used every available means that skill and practice could command for two days, but in vain. Mr. G., of Carmarthen, the subject of the preceding case, being in the neighbourhood, hastened to the house, told the doctor of his cure, and begged them to try it immediately and to send for me. Fresh leaves were collected, and a poultice applied, but having no guide nor experience in its use, it was taken off too soon. I arrived at 4 a. m. Being sixteen miles distant, much time was lost. This was the third day. A catheter was passed, to satisfy ourselves as to the state of the bladder. No urine. I had brought some dried leaves and tincture with me. A poultice was made of ℥ss of tincture, with fresh and dried leaves; poultice applied at 5 a. m.; pulse about 80. At 8.30 a. m.: Pulse reduced about 15, and about ten ounces of pale, clear, slightly albuminous urine came off. A fresh poultice applied at 10, and I left, with directions to take it off when the pulse came down to 60. Plenty of urine continued to be secreted, and from that time he has continued to improve. Urine is now slightly albuminous and alkaline, in spite of our remedies. This gentleman was much out of health, and had been for two years the subject of renal calculi, nausea, and dyspepsia, but he is much better, and improving under phosphoric acid and iron.

Case 6.—Mrs.——, aged about forty, was suddenly seized with suppression of urine. In spite of every remedy, to the fourth day the suppression continued. Severe symptoms were now developed, and a tendency to stupor was amongst them, pulse indicating mischief. It was now decided that they should try the digitalis plan. One ounce of the tincture was mixed with a warm linseed poultice, and kept on overnight. The next morning a large quantity of urine passed. The poultice was kept on the abdomen all next day; urine was plentifully secreted; and from that time she improved and got well.

There may be a difficulty in obtaining leaves collected before seeding time, which appears to me to be the period of most vigour, judging from the fact that Nature concentrates her powers for the multiplication of species, and which at seeding time would pass into the young, leaving all other parts more or less exhausted. I believe from experience the winter leaf is very deficient in power. The tincture, mixed with linseed meal, succeeded in Messrs. Rowe and Phillips's case (Case 6); the fresh powder or dried leaves would be equally efficient. The rules of management must depend on the pulse. I have seen no good results till the pulse fell in number; it matters not from what figure: fall it must before any change occurs. In Mr. G.'s case it fell from 109 to 70 in ninety minutes; in Mr. R.'s, from 80 to 65 in three hours and a half. I would strongly advise 60 as a standard from a high number; 40 or 50 from a lower figure—say from 80. Judging from the effects on the circulation, we cannot lose sight of the fact that the arrest of secretion depends on capillary congestion, which in turn might, by pressure, paralyse the nerves. The fact, however, remains that we compel the kidney to resume its functions by diminishing the force of the circulation, lessening the quantity of blood by allowing a much longer interval between each new arrival. Strange, too, it is that in four cases the attack commenced suddenly like a fit of stone, and, in reality, stone came away in each case.

These cases are reported with the object of calling attention to the effects of digitalis in that dangerous disease, and of inducing the medical world to give it a trial. *It is not* supposed that it will succeed in all cases of that

mysterious disease; but it is clear that it has a powerful influence over t
renal secretions, and if carefully watched, taking the pulse as a guide,
mischief need be feared. I own there was a want of caution in the mana
ment of my first cases—indeed, it was only used as a hopeless remedy—but
the truth broke upon me, it was clearly seen there was a power in use I had
clearly foreseen, and more skill and caution was enforced in its managem
—*Medical Times and Gazette, Jan.* 25, 1868, *p.* 86.

DISEASES OF THE EYE AND EAR.

67.—ON A NEW METHOD OF EXTRACTING IN CASES OF CATARACT.

By Dr. CHARLES TAYLOR, Honorary Surgeon to the Nottingham and Midland Eye Infirmary.

[The writer states that he was induced to practise the mode of operating recommended in the following paper owing to his being dissatisfied with the results of some cases in which the lens had been extracted by Schufte's method. There is such a feeling of utter powerlessness when any accident occurs during the performance of the ordinary flap operation. The point aimed at by Dr. Taylor is to extract through a wound so small, that there is no risk in such manipulation of the eyeball, as are necessary to facilitate the various steps of the operation, and yet large enough to permit the exit of the lens without the introduction of any instrument into the eye, or consequent risk of those disastrous results which sometimes follow the lifting out of the lens with Schufte's spoons.]

I propose to call this mode of operating "Extraction by Linear Flap," as I think the term expresses its peculiarities better than any other, and will serve to distinguish it from Von Gräfe's strictly linear method, more especially in those points in which it essentially differs, and, in my opinion, possesses some advantages. I must mention that I had fully worked out this method, adopted it in my practice, and demonstrated its advantages to my medical friends, before I had heard of Prof. Von Gräfe's modification of linear extraction, and before any account of his experiments had been published. On first hearing of his modified linear extraction, I was under the impression that the two operations were almost identical; but having seen Von Gräfe's operation, I find material differences, although I have no doubt from what I have since learned, that we arrived at the same conclusions as to the necessity for a modification of Schufte's and the flap operation by a similar process of reasoning, and apparently at about the same time. In my first operations, as the most convenient instrument accessible in a provincial town, I made the incision with a ground-down Zehender's cataract knife, reduced to about the size of the ordinary secondary cataract knives sold in most cases of ophthalmic instruments. I have since used Von Gräfe's knife; but it is quite possible to make the incision with Bader's smallest sized iridectomy knife, cutting gently round from left to right within the prescribed limits. As the risk, even if vomiting occurs, is very slight with this form of incision, I usually prefer to have the patient narcotized. Keeping the lids apart with the stop-speculum, I then fix the eye by seizing the upper border of the external rectus tendon with a pair of forceps, and enter the long narrow-bladed knife in the corneo-sclerotic junction, about the commencement of the upper third, or junction of the upper with the middle third of the cornea, say 2½ or 3 lines lower down than in Von Gräfe's method, and rather more forward in the corneo-sclerotic junction. The site of the entrance puncture must vary somewhat, according to the size of the globe, as a very shallow flap will suffice if the cornea is of average or more than average dimensions, and a deeper one will be required if it is below the standard size. Care must be taken, in entering the knife, that it is made to penetrate the chamber at once. Any gliding between the layers of the cornea is much to be deprecated, as the consequent want of correspondence between the internal and external

wound will be sure to cause disappointment in the subsequent steps of the operation, and the lens will be impeded in its exit by little bands of partially incised corneal tissue occupying the angles of the wound. Having entered the chamber, the knife must be pushed gently across, and made to emerge at a counter puncture similarly placed. The necessity for using a knife very small in bulk, narrow, and sharp-pointed, will here be appreciated, as the resistance offered by the dense tissue to the passage of the knife in this situation is often sufficient to cause slight bending of the blade. It is important that the counter puncture should not be made too far back—an accident very apt to occur, and by which one is betrayed into making an unduly large wound, a considerable portion of which will be situated in the sclerotic, thus giving rise to risk from escape of vitreous humour. A little practice, the avoidance of haste, and occasional apposition of the operator's forefinger, will usually suffice to prevent any trouble on this score. Having completed the counter puncture, I push the knife well through, and holding it parallel to the iris, cut directly upwards about two lines and a half, or until the edge of the blade is on a level with the central summit of the cornea. It is then turned directly forwards, and the section completed by a gentle sawing movement, thus forming a small, shallow, elongated flap, situated well back in dense tissue, with sloping sides and transverse centre, which last occupies the summit of the true cornea. The aqueous escapes when the counter puncture is made, and unless a preliminary iridectomy has been performed, the iris frequently folds over the edge of the knife. Gentle pressure on the cornea, and careful management of the blade, I have always found sufficient to free it, and enable the operator to complete the section without wounding the iris.

Having completed the incision, I usually remove the speculum so as to allow the eye to recover somewhat, and a little aqueous humour to collect, and then proceed to the second step of the operation. It is safer now and subsequently to insert only one blade of the speculum under the upper lid, the other blade being supported on the patient's cheek by the finger of an assistant; scarcely any pressure is thus made on the eyeball, and risk of rupture of the hyaloid membrane is considerably diminished.

As the conjunctiva is only incised, and to a very slight extent at the extremities of the wound, there is usually little trouble from hemorrhage up to this stage; should the incision, however, by accident be placed too far back, and bleeding occur, it is well to apply slight pressure to the eyeball with a pad of cotton-wool, before proceeding to the second step of the operation, which, if an iridectomy has not been premised, will be the excision of a small piece of iris. This is readily accomplished by inserting the forceps into the central portion of the incision, which occupies the apex of the cornea, while an assistant, if necessary, gently depresses the globe. This situation of the central portion of the incision enables the operator to enter the chamber at once, and the difficulties sometimes met with when the forceps and pricker have to be thrust under loose conjunctiva, with its sub-mucous tissue infiltrated with blood and aqueous humour (a source of embarrassment in Von Gräfe's method) are thus avoided. Having introduced the forceps, the blades must be opened and the iris seized near to the pupillary border, gently withdrawn, and divided close to the edge of the section. For this purpose I use very fine-pointed bent scissors, and insert the points just within the angle of the incision: if this is not attended to, the iris is very apt to prolapse in this situation, and to some extent delay the progress of the case as well as occasion a little deformity, which will be noticed afterwards. Having divided the iris at the right angle of the incision, it must be separated gently, so far as may be, from its ciliary border to the opposite edge of the wound, and cut off with the same precautions to secure the small piece so apt to prolapse as before. Should the iris bleed, so as to threaten to fill the anterior chamber, and so obscure the future steps of the operation, it is well to repeat slight pressure with a cotton-wool pad for a minute or two; this will arrest the hemorrhage, and any blood may then be removed by suction, or coaxed out by *manipulation of the eyeball*, while the posterior lip of the incision is

gently pressed back with the curette. In one case (Mrs. N., Case 44), where it was very desirable to enlarge the pupil, I excised a portion of the lower central segment of the iris through the same wound, and at the same time as the upper, with a very good result. The iridectomy is more readily performed, and the excision of a smaller portion of iris will suffice, if the pupil is well dilated prior to the operation. This is best effected by a strong neutral solution of atropine, applied the night before, and repeated in the morning. The pupil contracts on the escape of the aqueous humour, but not so readily as it would, if not previously dilated; and the subsequent dilatation on which the success of the operation may in some degree depend, is afterwards much more easily maintained. I have, in several cases, where great age, marasmus, arterial degeneration, or the existence of other causes was likely to impede the progress of the case, or otherwise vitiate the result, adopted Mooren's precaution, and performed the iridectomy six weeks before extraction; and in very unfavourable cases, as in the eye remaining to be operated on in Smith's case, where suppuration threatened for twenty-four hours after the extraction, I have excised a large piece of iris both from the upper and lower segment of the pupil as a preliminary precaution. This division of the operation of extraction for cataract into two portions, with a prolonged interval, is, however, manifestly inconvenient both for the poor, who can ill spare the time required for two operations, and also for the wealthy, who often have a considerable distance to travel. Sometimes, also, from a want of correspondence between the wound made in extraction and the smaller one used in the iridectomy, I have found the iris more apt to prolapse at the corners than if excised in proportion to the length of the incision at the time of extraction. In some cases, however, a preliminary iridectomy has many advantages; by it the operator becomes thoroughly acquainted with his patient, and he is enabled to form a correct opinion as to his behavior under extraction—a matter of no slight importance, as all accustomed to deal with such cases must allow. The important question as to the use or omission of chloroform may also then be decided; and, if administered, its effect upon the patient at the time, and subsequently, so far as vomiting is concerned, may be noted and utilized to the evident advantage of both patient and operator. In many cases also, where cataract is complicated with other manifest or incipient diseases of the eyeball, tension is reduced, and the tendency to cyclitis diminished by a preliminary iridectomy; subsequent extraction is easier to perform, and risk from bruising the iris abolished. The operation is thus reduced to its simplest elements, and chances of failure materially lessened by eliminating causes of accident. Having completed the iridectomy, the operator should seize the globe with forceps close to the cornea, above the insertion of the inferior rectus tendon, depress the globe with very slight pressure, and incise the capsule freely, taking care to pass the pricker beneath the lower segment of iris, and open the capsule from bottom to top, in two or three places. In withdrawing the pricker, it should be gently inserted under one of the angles of iris left by the excision, and drawn to the extremity of the other, so as to lay the capsule freely open, on a level with the equator of the lens. It is important to open the capsule in this situation, as I have more than once seen the exit of the lens retarded by little undivided shreds of membrane occupying the angles of the wound. Not unfrequently the lens comes forward, and is extruded immediately the capsule is incised; but, if from any cause it should be retarded, it will be necessary to make pressure on the globe, while the posterior lip of the wound is slightly pressed back with a small spoon or curette. Pressure is best made with the ordinary strabismus hook, placed in contact with the globe, below the insertion of the inferior rectus tendon, about four or five lines from the margin of the cornea. It must be moderated according to circumstances, should cease when the lens is partially extruded, and evacuation completed either by the insertion of a sharp hook into the lens or by *gentle* pressure on the corneal surface, so as to lift forward its lower border *as it is in the act of escaping.* Cortical fragments, if any remain, may usually *be extruded* by slight pressure; but if this does not suffice, I usually place

temporary pad of cotton-wool on the eye, and allow the patient to recover om the chloroform narcosis, when the aqueous will be re-secreted, and any agments concealed by the iris may be washed into the pupillary area by ight pressure on the globe external to the lids, and evacuated by gently pening the wound with the curette. If any remain, I remove them ith Mr. Teale's suction curette (the sucker must always be used with great aution, otherwise hyperæmia ex vacuo will be induced), and then tear across r withdraw any little fragments of capsule with iris or canula forceps. By ius carefully removing the capsule subsequent to the extraction of the lens, believe all the advantages obtainable by the evacuation of the lens, with its apsule entire, as in Pagenlecher's operation, are obtained; but if the operator hould desire to remove the lens and capsule in its entirety, as in that operaon, I believe the form of incision which I have recommended is the one best dapted to secure a safe and perfect result. Should the lens, from adhesions r other causes, resist such pressure as may be safely employed without the isk of rupturing the hyaloid membrane, as has happened in some of my ases, I extract it with a small skeleton spoon. From the large size of the ound this becomes very easy, and as the flap yields readily, the lens is easir tilted forwards, and there is no necessity to incur the risks inseparable om the insertion of the spoon too far back into the vitreous cavity.

If it is desirable to extract by the lower section, the same form of incision ay be employed. The operator seizes the conjunctiva above the cornea in s vertical meridian with toothed forceps, while an assistant draws down the wer lid, and assists in fixing the globe. The lower section is preferable in ases where the lens is superficially softened, as any soft cortical matr left behind in the eye will gravitate to the bottom of the anterior hamber, pass harmlessly through the coloboma iridis, and either undergo bsorption, or be washed out in the gushes of aqueous humour. The risk of s lodging behind the lower segment of iris and setting up subsequent irriation is thus avoided, but the section is less sheltered, and the coloboma glier and more disturbing to vision.

The operation completed, I apply a drop of solution of atropine in the rner of the eye, and cover the lids with a pad of fine cotton-wool dipped in, ut not saturated with, cold water. If sodden, the pad is heavy, irregular, nd unmanageable; but just moistened on the surface, it fits accurately into ie orbital cavity, and speedily dries into a thin smooth film closely bedded the surface of the lids, absorbing moisture, insuring equable support, a gular temperature, and the exclusion of light. It must be secured *in situ*, ith gentle pressure, by thin strips of soap plaster, spread on unglazed calico which adheres without warming). I find this by far the best dressing. ressure can be accurately applied by strips fixed to the forehead and cheek, nd, if necessary, by an additional broad strap passed from temple to temple. apply atropine and compress to both eyes, with a view of preventing the olling of the eyeball and sympathetic action of the pupil, which takes place concert with its fellow if one eye is uncovered. Over the compress I sually fit a strong pasteboard shade on to the forehead, with a little wool ntervening, and extending to the tip of the nose. This will protect the eye om any accidental blow or involuntary motion occurring during the sleep f the patient. Any kind of compress bandage, if required, may be readily pplied over the pads and plaster. If there should be much pain complained f after the operation, and it should not cease in a few hours, I give a small ose of morphia, and repeat it if necessary. Should this not suffice to relieve he patient, I apply four leeches to the temple, and as fast as one drops off eplace it with another, until either the symptoms are relieved, or it is evident hat no benefit may be expected from further depletion. Occasionally, the njection under the skin of the lid of $\frac{1}{8}$ to $\frac{1}{4}$ of a grain of morphia will afford mmediate relief; and if there is much heat complained of, or perceptible increase of temperature, a linen compress must be applied, wet with iced water, and kept cold by a small piece of ice placed in the corner of the orbit, nd re-applied as fast as it dissolves. A brisk purge is sometimes of service nd blood, in cases favourable for depletion, may be taken from the arm

Other symptoms must be treated as they arise on general principles. If swelling of the lids come on, with profuse semi-purulent secretion, the cold pressure bandage, as advised by Von Gräfe, must be applied, and changed every four hours, a hot aromatic poultice being applied for twenty minutes at each change.

The swelling of the lids may also be treated with a solution of nitrate of silver, in equal parts of spirit of nitric ether and distilled water (10 gr. to ℥j.) or by the application of tincture of iodine. Chronic irritation may be relieved by similar means—mercurial inunction with belladonna over the brow, and the evacuation of the aqueous humour, prolonged rest in the recumbent posture, or a second iridectomy downwards. If any capsule is left, it is better not to attempt its removal for two or three months. As to results, since devising the above described method, I have operated on 52 cases of cataract in succession, with only one complete failure.

The ages of the patients range from 5 months to 86 years. The majority, however, were persons of great age, several verging on 80, six being upwards of that age, and four upwards of 85 years. Some were suffering from marasmus and constitutional cachexiæ, others from various diseases of the eyeball, tending to vitiate the results of an operation. These persons were operated upon in succession as they applied, no selection was made, and I have never refused a case. Indeed, two or three, including the one complete failure (occurring in a feeble woman, aged 86, blind for twelve years with degenerated cataract and firm synechiæ), had been declined by other surgeons. In two cases (aged 65, No. 23, and aged 68, No. 17) healing was protracted, and the patients remained a month under treatment. One (aged 83. Case 18) died from apoplexy, the result of epilepsy, thirty hours after the operation, when the wound was found healed, media clear, and result of operation excellent. This proves that the wound, even in persons of great age, will heal in twenty-four hours. In one (aged 65, Case 25) suppuration threatened for twenty-four hours, the patient ultimately recovering with a closed pupil, and fair chance of restored vision by a second iridectomy downwards. The remainder recovered without a bad symptom, and were discharged on the seventh day; such as were free from remnants of capsule or amaurotic complications being restored to excellent vision. Some have been too recently operated upon to permit a fair record of the optical results at present. In two cases the lens was removed by the scoop, and in seven extracted by Mr. Teale's suction method. Brief reports of the whole of those cases operated upon by myself since November 1865 will be given in the concluding part of this paper. I have preferred to give *all* the cases, including the suction and scoop extractions; and, where there can be no objection to this being done, have followed Dr. Mooren's example, and appended the names and addresses of the patients, so that there may be no room for doubt or cavil as to the results.—*Edinburgh Medical Journal, Feb.* 1868, *p.* 700.

68.—A NEW OPERATION FOR CATARACT.

By Thomas Windsor, Esq., Manchester.

[The changes in the treatment of cataract have been frequent in recent years: depression, discision, and extraction having in turn succeeded each other.]

What are the dangers of extraction and what the means by which they may be avoided? Even during the operation itself we are exposed to risk from unsteadiness of the patient, from violent contractions of the ocular muscles, and from an inaccurate manual performance on the part of the surgeon or his assistant. All these, so far as they are due to the behaviour of the patient, can be overcome by fixing the eye with forceps, and by the administration of chloroform. Immediately following the operation we have primary suppurative destruction (necrosis) of the cornea and primary iritis; the former is essentially due to the corneal incision, and the frequency of its occurrence rapidly increases as the latter includes a greater extent of the

neal circumference, though no doubt a diminished power of restoration
a considerable share in its production in the extremely old and in maras-
patients. The latter (iritis) is caused by contusion of the iris, by the irri-
ion of cortical remnants or of the retracting capsule. At a later period
ne secondary suppuration in the cornea and secondary iritis or irido-cho-
ditis. Secondary corneal suppuration is generally a consequence of a low
m of iritis. Secondary iritis is caused by the extension of inflammation
m the cornea or from the capsular epithelium. We may infer, if these
ws are correct, that to prevent primary suppuration, we should make the
und as small as is compatible with the passage of the lens, and of a form
t will interfere as little as possible with the nutrition of the cornea, and
t will keep the lips of the wound in apposition. To prevent primary iritis,
should remove both the lens and the unopened capsule.

. have endeavoured to fulfil these indications in the following manner:—
e patient is kept throughout the operation fully under the influence of
oroform, so fully indeed, that he shows no sign of reaction when the con-
ictiva of the eye which is not to be operated on is seized with forceps. A
all* incision is then made at the corneo-sclerotic junction, the adjacent
tion of iris is excised, the zonula is opened with a small hook close to
wound, the scoop is gently passed into the vitreous cavity just behind the
s, and the latter is slowly removed. The after-treatment may be much
after extractions; the eye may, however, be safely examined at a much
lier period, say 24 to 36 hours after the operation.

The scoop that I have used, and that seems to answer its purpose well, was
de for me by Mr. Wood, instrument maker, Manchester.

My cases are yet too few, and most of them have been performed too re-
ntly, to allow me to attempt any complete account of the results: I can
y say that I have been pleased and surprised with the rapidity of healing
I the absence of any threatening symptoms.

A comparison of this operation with the others, to which I have already
erred, will show that it promises all the advantages claimed for them,
hout their disadvantages; the only objections that can be raised are in
pect to opening the hyaloid membrane and a possible loss of vitreous. As
the former, I believe the risk is extremely trifling; as to the latter,
t no loss of any moment can occur, provided the patient is fully under the
luence of chloroform. Its advantages appear to be, that the risk of the
eration is very much lessened; that the cataract is completely removed
a single operation and no secondary operation is required; that the eye
be opened at an early period and be carefully examined without danger;
t the convalescence is more rapid; that the patient may be sooner dis-
arged with safety, and that the average vision is (probably) superior to
t afforded by other methods.

It scarcely requires mention that the operator must expect some failures,
ecially in his earlier cases; the capsule is sometimes extremely delicate
d will give way, but such accidents are likely to become less frequent as
niliarity is gained with the method; on the other hand, I see no reason
y such an operation should not be employed in some cases, at least, of
t cataract.

have purposely omitted intra-ocular hemorrhage from my list of mishaps,
ause of its very rare occurrence: it would probably be still more uncom-
n after the proposed method than after flap-extraction, for the great and
lden relaxation of tension from loss of vitreous during the operation, and
lo-choroidal congestion at a later period, which must so much promote it,
uld scarcely ever happen.

n conclusion, I may again repeat that *the object of this operation is to
vent primary corneal suppuration and iritis by the form of the wound and
removal of the lens in its unopened capsule.*—*Ophthalmic Review, April*,
57, *p.* 251.

I am inclined to think that it matters little whether the wound is straight, as in von Graefe's modi-
linear extraction, or forms a small flap, *i. e.*, a flap of sufficient length, but little height. The
und must, however, be always of sufficient length, for example, always longer than Schuft's
sion (8‴).

69.—ON LINEAR EXTRACTION OF THE LENS.

By C. Macnamara, Esq., Surgeon to the Calcutta Ophthalmic Hospital.

[The writer commences by stating that he has performed the operation of linear extraction on upwards of 350 patients in hospital since 1864, and that in his private practice he very seldom resorts to any other proceeding in cases of hard cataract.]

In a former paper I inculcated the practice of excising a portion of the iris before attempting to extract a hard cataract from the eye. This proceeding is generally followed: in fact, Mr. Bowman says (p. 368, No. 4, 1865. of the Royal Ophthalmic Reports), "The iridectomy which is a necessary part of it, is no disfigurement, nor does it in any important degree affect the optical result." Further experience has led me to modify the rules I before laid down on this subject, as will be seen from the above table; for of the whole number of hard cataracts operated on (forty-two), I have only excised a portion of the iris in five cases. The plan I now adopt is, after the section has been made, and the capsule lacerated with the point of the knife, to pass the scoop so far into the anterior chamber as to enable me to reach the outer margin of the pupil with its blunt extremity, and, gently retracting the instrument, I draw open the pupil with it to such an extent as to allow of my pressing the edge of the scoop against the outer margin of the lens, which immediately tilts over on its axis, and the scoop being thrust onwards, the cataract comes to lie in the concavity of the instrument, and may then be withdrawn from the eye. By following out this plan, the scoop is never inserted deeper into the eye than on a plane slightly posterior to that corresponding to the natural position of the iris; and thus the vitreous is left intact. Upon this manipulation the success of the operation often depends. The matter is more fully described in my published lectures on the diseases of the eye. I would insist, however, on the fact that the scoop must not be thrust behind the lens as it lies imbedded in the vitreous; if this practice be pursued, there is considerable danger of an escape of the fluid, and its consequences. In two instances only out of the above one hundred cases, was there any loss of vitreous, and both of these went to the bad. I have little doubt that a fluid state of the vitreous existed prior to the operation, and no precaution on my part, as far as the operation was concerned could have prevented its escape. Von Graefe (Ophthalmic Review, No. 9, p. 11) states that of 118 cases operated on by him, "there were nineteen cases in which anomalies occurred in the course of the operation; viz., for the most part from the prolapse of the vitreous." I am inclined to attribute the difference in the conclusions which Von Graefe and I have arrived at on this point, to three causes: first because I am not in the habit of thrusting an instrument behind the lens till its attachments to the posterior caps have been broken down, but I make the lens turn over on its own axis till it comes to rest in the hollow of the scoop; secondly von Graefe does not administer chloroform so freely as I am in the habit of doing; and, lastly, I make my incision through the cornea considerably anterior to its line of junction with the sclerotic.

To the first of these causes I have already adverted, and the subject requires no further comment; with regard to the second, I may observe, that I never, under any circumstances, operate by linear extraction, unless my patient is *fully* under the influence of chloroform, and certainly the statistics above given bear me out in the propriety of this proceeding, for out of the whole number only seven patients vomited after the chloroform, and every one of these made excellent recoveries; consequently the fear of my patients vomiting after the administration of chloroform is not a complication which I dread much; and if the patient is prepared beforehand, it seldom occurs at all. My practice is to administer a purgative two days previous to the operation; on the day preceding the extraction I allow my patients to take very little food after 1 p.m., and then operate early the next morning, before they have swallowed either liquid or solid food. The chloroform

hould be administered with a free hand, so as to get the patient under its nfluence speedily, and thus save him from inhaling any large quantity of he anæsthetic; and should there be any disposition to vomiting, a bag conaining ice should be laid on the pit of the stomach; but, as I before emarked, I have not that innate dread of the effects of vomiting after linear xtraction which appears to cling to some surgeons, especially if care is taken mmediately after the operation to apply a compress and bandage over the losed eyelids. I do not, therefore, consider that the possible accidents which nay occur after the administration of chloroform should for an instant allow is to refuse to employ it, knowing as we do the certain benefits which the ast majority of our patients must derive from its use. Chloroform not only enders extraction of the lens a comparatively easy proceeding, and there-ore within the reach of any practitioner who is accustomed to operate; but t also prevents that injurious straining of the eye during and immediately ollowing the extraction of the cataract, which is so much to be dreaded in ases of this kind. With regard to the position and size of the opening which is to be made in the cornea for the operation of linear extraction, this is doubtless a matter of considerable importance; for unless the incision be sufficiently large to allow of the easy exit of the lens, plus the thickness of the scoop, it will be necessary to use considerable force in removing a hard cataract, and consequently damaging the epithelium of the elastic lamina of the cornea and iris. The incision should extend over a section corresponding o one-third of the circumference of the plane of the cornea, through which he opening is to be made; and unless we can succeed in doing this before he knife is withdrawn from the anterior chamber, it will be necessary to nlarge the opening by cutting through the cornea at either extremity of he incision with a pair of curved scissors, before attempting to remove the ataract from the eye. The secret of making this opening in the cornea ufficiently large in the first instance, consists in having a short but broad-laded triangular knife; and secondly, in thrusting this instrument, when nce its point is through the cornea, steadily forwards into the anterior hamber, so that the incision as it is made is filled up with the blade of the nstrument, and the aqueous is prevented from escaping. Should this fluid ush out of the eye before the section is completed, the cornea at once ecomes flaccid, and it is impossible to make the necessary opening through with the knife; but it must then be enlarged, as I have above described, ith a pair of scissors.

With regard to the position of the incision in the cornea, as a general ule I never make it within an eighth of an inch from the circumference of ie cornea. I believe it is a decided advantage to have the incision well rwards in the cornea; if it is made too near the sclerotic, there is much reater chance of prolapse of the iris and escape of vitreous; nor can I see ny objection to having the section in the position mentioned, so long as it well away from the central axis of the eye, and in a plane sufficiently rge to admit of the passage of the lens through it. I cannot say it gives ie much concern as to the direction of the edges of the wound; unless some ccident should occur, they unite within forty-eight hours, whether made at n obtuse, acute, or any other angle to the surface of the cornea.

The scoop which I usually employ is precisely of the same shape as that riginally used by Waldau; but in addition, its curved extremity is serrated. his scoop is also considerably larger, and a hole exists through its central ortion, so that, in fact, the rim of the instrument and its curved extremity lone remain. The advantage of this is the lens fits more firmly into this pen space than it does into the spoon-shaped scoop; and in withdrawing he cataract from the eye, we have merely its thickness from before back-wards to deal with, and not, as in the ordinary scoop, with the antero-posterior axis of the lens, plus the thickness of the instrument, to draw through the section in the cornea. A scoop of this kind is supplied by Weiss in all the cases of linear-extraction instruments which he sends out to this country.

It appears to me a very important point to remove any lenticular matter from the *anterior chamber* that may be left there after the extraction of the

lens. We may have to introduce the scoop half a dozen times into the anterior chamber before we can withdraw all the lenticular matter from the eye; and if small particles of it get lodged between the upper or lower portion of the cornea and the iris, a curved scoop must be employed, in order that we may dislodge them. I consider it absolutely necessary, however, that these particles of lenticular matter should be removed. Pulpy cortical substance does less harm in the anterior chamber, but the smaller the quantity allowed to remain there the quicker and more certain the patient's recovery will be.

One of the great advantages of linear extraction over the flap operation is the comparative immunity attending the former from cyclitis and suppuration of the cornea. Both of these accidents were of comparatively common occurrence among my flap-operation cases. There appears to be a very general belief among certain persons that the natives of Bengal are peculiarly advantageous subjects to operate on,—that, in fact, they rather like having their limbs or lenses removed; that wounds heal up in a marvellous way; and that we have none of the difficulties to contend against in our after-treatment which are so frequently experienced in hospital practice at home. A greater mistake than this could not possibly exist. One has only to turn to Fayrer's account of osteo-myelitis to perceive that the class of patients met with among the poor of Calcutta are about as unfavourable a set as we could conceive to operate on; and, consequently, those who attempt the flap operation for the removal of the lens, will find suppuration of the cornea and cyclitis, running rapidly on to abscess of the eyeball, by no means an uncommon sequence of their best efforts; but out of the 100 cases I have operated on by linear extraction during the year, four eyes only have been lost, either by sloughing of the cornea or abscess of the globe; and in my private practice I have not had a single instance of this kind occurring after linear extraction. This fact alone is sufficient to stamp the operation as one having peculiar claims upon our consideration. I am in the habit of dropping an eight-grain solution of atropine into the eye immediately after the operation, and then applying a compress and bandage over it. On the following morning this is removed, and extract of belladonna, containing four grains of atropine to the half-ounce, is smeared over the closed lids. The compress may be discontinued, and a simple bandage employed. On the following day the lids may be gently opened, and the solution of atropine dropped into the eye. If we then find the pupil is dilated, there will be no necessity for the further use of the atropine; but should its effects on the iris not have been fully established, we must continue to use it every six hours until the pupil is well dilated. When once this has taken place, there will be very little chance of any subsequent mischief going on in the eye; but unless the pupil is dilated, it is impossible to say how the case may terminate. Atropine, and such other remedies as may appear necessary, must be employed; but, as a general rule, the after-treatment of a case of linear extraction is, comparatively speaking, a very simple matter, as compared with one of flap extraction.—*Ophthalmic Review, April*, 1867, *p.* 239.

70.—MODES OF EXAMINING THE OPTIC DISC WITH THE OPHTHALMOSCOPE.

By C. Bader, Esq., Ophthalmic Surgeon to Guy's Hospital.

I. *Examination of "the inverted image," or indirect ophthalmoscopic examination of the optic disc.*—Suppose we wish to examine the optic disc of a healthy right eye with blue iris, the pupil having been dilated by atropia, and the observer, the patient, and the light, being placed as is usual in ophthalmoscopic examinations:—

We take the ophthalmoscope in the right hand, grasping its margin with the thumb and forefinger, and turn the reflecting surface towards the patient. We "throw" the light upon the patient's right eye, and approach the eye with the ophthalmoscope until the circular surface of light on the patient's face has nearly the size of the ophthalmoscope itself. We must take care to

keep the circular surface of light continually on the patient's eye while raising the ophthalmoscope to our own right eye. When looking through the sight hole, we perceive the patient's pupil to appear brilliant red instead of black. If the pupil appears black, we either are not throwing the light upon the pupil, or are not looking through the sight hole of the ophthalmoscope.

When we have obtained the red reflection from the pupil, we next look for the optic disc, which is most quickly found (the ophthalmoscope being held by the thumb and forefinger) by holding up the little finger of the same hand, and directing the patient to look at the tip of it. The centre of the yellow spot of the patient's right eye is thus directed to the tip of that finger, and the optic disc comes to stand opposite the pupil.

A change in the colour of the reflection from the pupil, from red to whitish-red or brilliant white, indicates that the optic disc is in view. The observer should persist in his endeavours until he has obtained the brilliant whitish reflection.

In persons with dark irides, the difference in the reflection from the pupil is still more striking, on account of the contrast of colour between the optic disc and the tunics.

Having thus obtained the reflection peculiar to the optic disc, we place the convex lens (holding it between the forefinger and thumb of the left hand) before the eye under examination. The diffused image of the flame, which is "thrown into the eye" by the ophthalmoscope, serves to illuminate those parts of the interior of the eye which lie within the area of that image. The rays of light which are reflected, *e. g.* from the optic disc thus illuminated, have to pass through the lens held in front of the patient's eye, and an inverted defined image of the optic disc, &c., is formed in front of the lens, *i. e.* between it and the observer, and is viewed by him.

In this case we do not directly examine the optic disc, but only its inverted aërial image; hence we term this method "the examination of the inverted image" or "the indirect ophthalmoscopic examination."

The little finger of the hand which holds the lens is placed on the patient's cheek to steady the lens, while the middle or ring finger is kept disengaged, to be placed if necessary, upon the margin of the upper lid so as to raise it, or gently to press upon the eyeball, to produce and watch the pulsation of the retinal arteries in the optic disc. The lens itself is held at from one to two inches from, and somewhat obliquely in front of, the patient's eye. If the lens be held vertically the images of the ophthalmoscope, which appear upon the two surfaces of the lens at its centre, interfere with the view of the part beyond. Hence, the lens should be held obliquely so as to cause the two images to recede from each other. By approaching the lens to, or removing it from, the eye under examination, we soon succeed in obtaining a distinct view of the well-defined (aërial) image of the optic disc, &c.

This image can be enlarged by our placing behind the sight hole of the ophthalmoscope a convex lens of greater focal distance (one of from ten to twenty inches focal distance).

II. *Direct ophthalmoscopic examination of the optic disc.*—Another mode of examining the optic disc with the ophthalmoscope is the direct method, or the examination of the erect image of the optic disc.

Though by the indirect method we can at one glance overlook the entire optic disc, we do not obtain such a correct idea of its colour, outline, &c., nor of its relation to adjoining parts; neither do we see it so highly magnified as by the direct method. This method, should, therefore, always be adopted when we wish to see the finer details of the interior of the eye. It is accompanied by the inconvenience of having to approach the eye to within one or two inches, but a few seconds suffice to obtain a view of the optic disc, &c., which no other method of ophthalmoscopic examination gives with equal facility.

The optic disc of the emmetropic and of the hypermetropic eye may be examined with the ophthalmoscope alone: to see that of the highly myopic eye with equal distinctness a suitable concave lens has to be placed behind the sight hole of the ophthalmoscope.

Though the direct method may be adopted w...
eye under examination by atropia, it is better to employ ...
Hospital Reports, 1868, *p.* 510.

71.—SUBCUTANEOUS INJECTION OF STYRCHNINE IN CASES OF AMAUROSIS.

By A. Samelson, Esq., Manchester.

[We take the following short extract from a letter to the Editor of the British Medical Journal.]

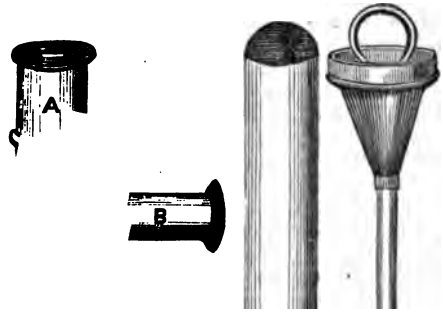
Within the last few years, there have been recorded three or four cases sufficiently authenticated, it appears (by Frémineau, Sämann, and others), of complete amaurosis, with entirely negative ophthalmoscopic evidences in which the subcutaneous injection of strychnine has proved of sovereign advantage in restoring the sight of the patients, after varying intervals of total extinction, and after unavailing trials of other methods.—*British Medical Journal*, *Nov.* 23, 1867, *p.* 486.

72.—A NEW INSTRUMENT FOR DEMONSTRATING THE MEMBRANA TYMPANI, OR OTHER DEEPLY SITUATED ORGANS.

By James Hinton, Esq., Aural Surgeon to Guy's Hospital.

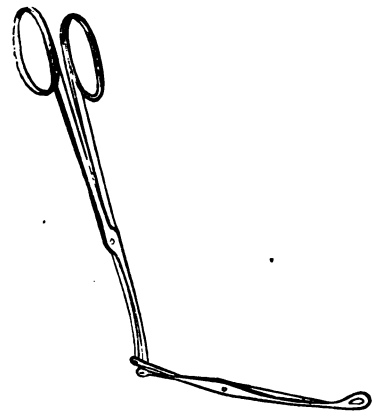
I wish to lay before your readers a short description of two small instru-

Fig. 1.



ments. The first was designed for the purpose of demonstrating the membrana tympani. I had long felt the difficulty of exhibiting that structure satisfactorily to a third person, inasmuch as, in any of the ordinary methods, it was necessary to remove my own eye from it before it could be brought within another person's vision, and without the aid of sight it is very difficult to maintain the speculum in its exact position. The instrument depicted above (Fig. 1), by the introduction of a prism at C, enables two persons to see it perfectly at once, the eye being placed at A and at B. In other respects, it is essentially the same instrument that has been long in use in the hands of my friend Dr. Anderson, of Glasgow. The light is admitted at D. The refracted rays, of course, give a lateral inversion of the object, making the right hand appear the left; but this is of no practical importance. The instrument has been found equally available for the demonstration of other internal organs—the rectum, urethra, &c.—and constitutes, I believe, an exceedingly efficient and convenient "endoscope." The rectal and urethral tubes are also represented in the cut.

Fig. 2.



The second figure represents a forceps of a new construction by which great lightness and tenuity are combined with considerable strength and firmness of grasp. It is especially suitable for cases in which, as in the auditory meatus, it is desirable to impede vision as little as possible. Both instruments are made at a very moderate expense by Messrs. Mayer and Meltzer, of Great Portland-street.—*Medical Times and Gazette, Jan.* 28, 1868, *p.* 104.

73.—REMOVAL OF FOREIGN BODIES FROM THE EAR.

From a report on Hospital Practice by the Editor of the MEDICAL TIMES AND GAZETTE.

The method we are about to describe is so simple that probably it may have occurred to others, and it has indeed been recommended in print by Mr. Hutchinson some time ago. It has, however, not yet found its way into our best manual of Aural Surgery (Toynbee, by Hinton), which advises the disappointing plan of syringing, whilst others still recommend the dangerous use of forceps or scoop. Instead of trying either of these, let the surgeon take six inches of fine wire and double into a loop; then, having the patient placed on his side, pass the loop into the ear as far as it will go, and turn it a little gently. At the first or second withdrawal the foreign body will come out in the loop. The wire being flexible gives no pain, and cannot possibly do damage. It is almost certain to find its way round the foreign body, however deeply the latter may be placed, or however closely it may, fit the cavity. Mr. Hutchinson asserts in its advocacy that it is very much

easier to use, very much safer, and lastly that he has several times succeeded with it in cases where other means had utterly failed. The scoop he regards as especially likely to do mischief, since it involves pressure against the wall of the auditory canal. There is in the London Hospital Museum part of the temporal bone of a child who died in consequence of a small bean having been forced by the scoop through the membrana tympani into the inner ear.—*Medical Times and Gazette, March 7, 1868, p. 261.*

DISEASES OF THE SKIN.

74.—THE PATHOLOGY OF THE SKIN.

By Erasmus Wilson, Esq., F. R. S.

We cannot do better than study the pathological lesions of the integument side by side with the commonest and most universal of the diseases of the skin, namely, eczema. Eczema, as a word, simply means *eruption*, and it may well be said, that it is *the eruption* of the skin; as the most common of the eruptions it is that which comes the most frequently under our observation, and is therefore the one to which we can most readily refer, the most easily study, and with which we can with the greatest facility compare other forms of cutaneous disease. The word eczema was given to this very common eruption by the Fathers of Medicine, namely, the Greeks; but they also had another name for it, which expressed one of the most prominent of the characters or qualities of the disease,—namely, *psora*, and we must endeavour to pronounce the *p* as well as the *s* in this word to give it its proper effect. Now psora means *the itch*, because of its itchiness, because, in fact, of the necessity, which is induced by its itchiness, to rub and to scratch; the word *psoein* means to rub, and *psoriaein*, to have the psora or itch. This is the language of Hippocrates, who dates back five hundred years before the Christian era; and these words are as applicable to the disease in question at the present hour as they were in the time of our great predecessor. But let us pursue the matter a little further. "Familiar to our ear as household words" are the terms eczema and psora, and so also is another word psoriasis; but until now it may not have been known to you that eczema and psora have the same meaning, and are in fact synonymous; that eczema announces the *eruption* only, whereas psora represents the most marked and troublesome of its *symptoms*, its psoric or itchy tendency. By-and-by, we shall have to point out to you that eczema or psora presents two very marked and distinctive forms represented by the words *humidum* and *siccum;* namely, eczema humidum, eczema siccum, or psora humida and psora sicca. The moist form is distinguished by moisture or discharge, and the dry form by dryness and a consequent exfoliation of the cuticle in small scales; eczema siccum or psora sicca, is therefore squamous as well as being itchy, and in these two qualities combined we find the explanation of the word *psoriasis;* psoriasis, in fact, being the squamous form or stage of psora, of which it is an obvious derivative. It is important to remember this, because the word psoriasis has been generally misapplied since the time of Willan, and the evil has been increased of late years by the assignment of the term to the alphos of the Greeks, the lepra vulgaris of Willan. We also beg to remind you that the *a* in psoriasis should be pronounced long, namely, *psoriāsis.*

But we must say a few words more with regard to eczema before we commence to observe it more carefully. It may seem to you that we attach too much importance to the meaning of a word; but, gentlemen, we must ask you to accept in faith that which we now declare to you; we have not laboured at the study of cutaneous medicine for thirty years without having come to certain positive conclusions, and conclusions which we believe to be founded in truth. One of the first articles of cutaneous faith that we call upon you to subscribe to, if you would in future be ranked as our disciples,

is, that eczema and psora are identical, and that psoriasis is a stage of psora. When the time comes, we shall be equally able to prove to you that that other disease which is now so commonly termed psoriasis by the foreign schools, is not psoriasis, but is really the lepra alphos of the Greeks, the lepra vulgaris of Willan. But, gentlemen, we have other masters besides the Greeks, and masters whom it is our delight to honour,—namely, those worthy successors of the Greeks, the Romans. Greatest among the Roman physicians was Celsus; and Celsus designates psora and eczema by the terms *scabies* and *impetigo;* scabies derived from *scabĕre*, to scratch, itself a derivative of *scaphein*, to dig as with the nails in scratching, is the analogue of psora; while impetigo, *ab impetu agens*, a breaking out with impetus, corresponds with eczema; and all the four terms apply to the same disease. At the present time, however, we dispose of these four terms differently; eczema we retain as the type of the psoric affection; impetigo, we apply to a pustular eczema; scabies, we restrict to the eczematous eruption, excited by the acarus scabiei; psoriasis we give to the dry and squamous eczemata, especially when associated with thickening and cracking of the skin and much pruritis; and psora we discard, as having no special occasion for its use.

Gentlemen, we offer you no excuse for this digression from our subject; we have led you over classic ground, and through a territory in which disputed questions prevail, but we have endeavoured to guide you with the lantern of knowledge to the temple of the veiled goddess whom all true worshippers of science most venerate; and we conclude our episode by informing you that although in cutaneous medicine we are ungracious enough to have rejected the ancient term *psora*, yet that we retain it in our popular language in the simple and forcible expression *sore*.

Well, gentlemen, if we have a subject before us suffering from eczema, we shall inevitably find *redness* or hyperæmia more or less extensively distributed; we shall have *swelling* or thickening from infiltration of the affected skin; we shall possibly discover an elevation of temperature; and we shall pretty certainly be made aware of *pain*, commonly in the character of burning or scalding, itching, and probably tingling and pricking. But besides these morbid states, or lesions, the common signs of inflammation of the skin, we shall meet with minute elevations, which are termed *papulæ*, or pimples; or small elevations filled with a transparent albuminous fluid, namely, *vesiculæ*, or vesicles; maybe with small vesicles filled with pus, vesico-pustules, or simply *pustules;* then we shall find *excoriations* exuding a viscous albuminous fluid, sometimes transparent and sometimes purulent; or the thickened skin may be fissured with *chaps;* or there may exist coverings of different kinds on the inflamed corium, sometimes assuming the character of *crusts* or *scabs*, and sometimes of small *scales* undergoing constant desquamation. Again, we may observe, in conjunction with these lesions, *scratches* denoting the operation of the nails for the relief of itching; and lastly, *maculæ* or stains left on the affected part after the healing of the skin.

Now all these *lesions* may be present at successive periods on the same person and in the same disease,—for example, eczema; and if we look at them more closely, we may see reason to divide them into two groups,—namely, such as are of earliest appearance, or *primary;* and such as are subsequent or *secondary*. For instance, the redness, the papule, the vesicle, and the pustule are primary; while the swelling, the excoriation, the fissure or chap, the crusts, the scabs, the scales, the scratches, and the stains are secondary. But besides these lesions, which are all present in eczema, there are a few more which belong to other forms of disease, and therefore we shall assemble all that are usually recognized, under the two heads, primary and secondary, and consider and describe them seriatim.

The *primary lesions* of the skin, or signs of a morbid state, are as follows:—

Rubor.	Pustula.	Tuberculum.
Papula.	Bulla.	Tuber.
Vesicula.	Squama.	Macula.

And the *secondary lesions*:—

Tumefaction.	Chap.	Desquamation.
Excoriation.	Ulcer.	Discoloration.
Scratch.	Crust.	Cicatrix.

RUBOR or REDNESS, is the state of colour of the skin produced by the abnormal distension of its vessels with blood. It may be simply *functional* and uncomplicated with any change in the vessels themselves, or it may be *structural* and depend upon a permanent state of enlargement, or hypertrophy of the vessels, or alteration in their coats. Functional redness may be present as a mere transient blush, or physiological hyperæmia, or it may represent many degrees of intensity of pathological hyperæmia and constitute an exanthema or an erythema. Structural redness, on the other hand, may be due to a permanent enlargement of the vessels of the skin, termed angeiectasia and vascular nævus; or it may result from lesion of the coats of the vessels and the escape of the blood into the parenchymatous tissue, as in purpura, and also in contusion of the skin.

Pathological redness also presents the phenomena, that it may subside without alteration of nutrition of the skin, as in some of the slighter forms of erythema, or it may induce a suspension of nutrition of the epidermis, and in this way give rise to exfoliation and desquamation of the cuticle; or it may be associated with exudation and hypernutrition of the cell-structure, causing thickening; or, finally, it may become chronic and accompany the secondary series of morbid processes, and be in itself a secondary lesion. You remember, gentlemen, that it was upon redness that Willan founded his definition of *exanthema*, or *rash*,—namely, that it consists "of red patches on the skin, variously figured, in general confluent, and diffused irregularly over the body, leaving interstices of a natural colour." In this definition, however, it is clear that Willan had in his mind the rash accompanying the exanthematous fevers,—namely, rubeola, scarlatina, and variola.

If we incline to seek for examples of the several forms of pathological redness which we have enumerated, we shall find as illustrations of hyperæmia subsiding without secondary changes, erythema, urticaria, and roseola; suspended nutrition of epidermis resulting in exfoliation is seen in the exanthematic fevers; and suspended nutrition of the epidermis with hypernutrition of the cell-tissue or exudation, in eczema; eczema may also be taken as indicating a hyperæmia of a chronic character, and belonging to the group of secondary rather than of primary lesions.

Redness, therefore, is an important sign of cutaneous disease, and betokens either an excess of blood in the skin or an escape of blood from the vessels. The excess of blood may be physiological or pathological, and in the latter case it may be temporary, in consequence of resulting from undue action, or it may be permanent, from an alteration of structure in the part. Again, it will excite in our minds a varying degree of interest according as it may be general or partial, of larger or smaller extent, of irregular or circumscribed figure, or as standing alone, or associated with other pathological signs or lesions, whether primary in their nature or secondary.

THE PAPULA or pimple is an elevation of the cutis, for the most part conical, but sometimes semiglobular or flattened, minute in size, of a variable tint of redness, generally accompanied with itching, and commonly succeeded by a thin scale. We will ask you to bear in mind the definition of papula given by Willan,—"a very small acuminated elevation, with an inflamed base, very seldom containing a fluid or suppurating, and commonly terminating in scurf." If we seek further into the nature of a papula, we shall find that it has its seat at the aperture of one of the follicles, of the so-called pores, and that it is produced by hyperæmia of the vascular plexus of the follicle and exudation into the intervascular parenchyma. The degree of redness of the papula will bear relation to the extent of hyperæmia, and its bulk to the amount of exudation. You must satisfy yourselves, at the first *opportunity*, of the exact seat of the papula, and the knowledge you will *thereby* acquire will help you very materially in comprehending the pathology

of the skin. With careful scrutiny you will discover the aperture of the follicle at the extreme summit of the papula, and you will find the summit to be transparent, from the presence of a conical plug of epidermis which normally occupies the dermal entrance of the follicle. Rayer and others pricked the summit of the papula under the expectation of finding an explanation of this transparency in the presence of fluid; but they were naturally disappointed, having mistaken the cause of the appearance.

Besides its conical or semiglobular or flattened figure, we have to note, in connexion with papula, the disturbance of innervation to which it gives rise and the consequent pruritus. The papula is remarkable for its pruritic tendencies, but we are unable to say with any certainty whether the itching is due to pressure on the filaments of the terminal nervous plexus by the hypertrophied cells of the parenchyma, or to absorption, by the nerves themselves, of the exuded fluid of the vessels, or of the excreted fluid of the cells. In a few instances the papula subsides to the normal level of the skin without desquamation; more frequently the morbid action of the tissues determines an arrest of nutrition and the consequent exfoliation of the cuticle covering the summit of the cone, while in not a few instances, the itching provokes scratching, by which the summit of the papula is torn off, and a scab is produced, either through the discharge resulting from excretion by the over-distended cells, or from the escape of a minute drop of blood, the exudation in both instances desiccating by evaporation on the summit of the papule. The duration of an individual papule ranges from a few hours to one or two or more days.

As redness is the type of the exanthemata and the erythemata, so prominence is the type of the *papulæ* or papular affections, and especially of lichen. We need not stop here to inquire by what perversion of signification the term lichen, meaning literally a tree moss, has become symbolized by a papula of the cortex of man; but we shall do well to accept it as a fact. By lichen we understand a papular eruption, and we have other examples of a papular eruption in strophulus and prurigo. If we go back to the illustration of cutaneous disease which we selected as a standard of comparison—namely eczema—we shall find papulæ to be a constant element of that eruption, and sometimes a most conspicuous symptom, as in the variety which we term eczema papulosum and also eczema lichenodes; and the papule of eczema is true to the characteristics of its family stock, the papules are minute, they are conical, hard to the touch, and extremely pruritic. Strophulus is a papulous eruption occurring in infants, and the pimples are larger than the typical papula; they are frequently rounded at the summit instead of being conical, and they have an amount of redness and exudation around their base greater than is met with in the typical lichen. The papulæ of lichen planus are remarkable for their flattened and depressed summit, while those of prurigo are remarkable for the extreme degree of pruritus by which they are accompanied; they are often not perceptible until the pruritus has excited scratching, and the scratching has, as it were, called them into being, and they are especially distinguished by the small black scab resulting from exuded blood that crowns their summit after they have been violently torn with the nails. There is another papule which is remarkable for a tingling itching, which is larger than the papule of simple lichen, which has the pale hue of the tubercles of urticaria after it has been scratched, and which, like the papule of prurigo, is followed by a small black scab: this is an eruption of children, and is termed lichen urticatus. Then we find in our catalogue of papulæ, the minute papules of rubeola and scarlatina; the papules of variola, hard and gritty at first, and afterwards undergoing development into vesicles and pustules; the papules of syphilosis, distinguished by their dull-red colour and the absence of pruritus; the papules of gutta rosacea, and the large follicular papules of acne and sycosis, together with other forms of papulæ dependent on accumulations in the follicles of cellular exuviæ or sebaceous substance; on hypertrophy of connective tissue, as in the instance of achrochordon, or on hypertrophy of papillæ, as in verruca.

If, therefore, we proceed to sum up the special characters of a papule, we

shall find them comprehended in a consideration of its size, figure, colour, seat, symptoms, and decline. Its *size* ranges between a line and two lines, or, may be, three—that is, a quarter of an inch: the latter would be esteemed a very large papule, verging on a tubercle, and our measurements of the papulæ of cutis anserina give a diameter of half to three-quarters of a line; while in height it averages about a line. Its *figure* is, for the most part, conical, sometimes globose, and sometimes flattened or depressed in the centre, as in lichen planus. Its *colour* varies from white to red, and from red to its deeper tints, reaching even to the confines of purple and livid; the white colour is seen in lichen urticatus, the bright-red in lichen simplex, and its purple and livid hues in lichen lividus. The *seat* of the papule is very obviously the aperture of the follicle, and from the follicular plexus it derives its colour and its prominence. Its most characteristic symptom is pruritus, and its decline is accompanied with the separation of the circle of cuticle which originally formed the covering of its summit.

The papule, which we have just been describing, might be called the papula of Willan, or the hyperæmic papule; for we need not remind you that the term pimple is commonly employed in a more universal sense. According to Hebra, a papula is "a solid projection above the surface," consequent on "any morbid change in the skin," and presenting a size varying between that of "a millet-seed and a lentil, and containing no fluid." In reference to *size*, he names the papulæ *miliares*, *miliformes*, and *lenticulares*: and in point of *figure*, he treats of them as being acutæ, planæ, conicæ, and globosæ. These words of Hebra remind us of the use which we sometimes make of seeds and coins as a comparison of size. Thus, there are the millet-seed; the mustard-seed, black and white; the hemp-seed; the lentil: the pea; the bean: and several kinds of nuts, of eggs, and of fruit; besides the coins in common use, the smaller and the larger silver pieces, and the copper pieces. But who amongst us can form anything more than an approximate guess of the size of an object from the use of such terms! who has determined by experiment that the size of the black mustard-seed is about a half a line in diameter; the millet-seed, one line; the white mustard-seed a line and a half; the hemp-seed, two lines; the split pea about three lines, that is, a quarter of an inch; the lentil, more than a quarter of an inch; the fourpenny piece, more than half an inch: the sixpenny piece, three-quarters of an inch; the shilling piece, one inch; and so on! We know how easily we can be deceived in the size of objects; and, therefore, we advise you, very seriously, in your essay to distinguish size, to have recourse to fractions of an inch, be they lines or eighths, in preference to the before-mentioned objects. When Hebra speaks of papulæ miliares, we may understand papules having the diameter of a line; but when he mentions papulæ lenticulares, we hesitate to accept the comparison, because the lentil measures more than a quarter of an inch in breadth, and prominences having such a bulk we should at once designate as tubercles.

Hebra also reminds us that the range of signification of the word papule or pimple, is much more extensive than our definition is calculated to admit. For example, besides the common papula produced by exudation, whether in the follicles or in the papillæ, there are the prominences resulting from muscular spasm, constituting cutis anserina; accumulations of cellular exudation in the follicles; accumulations of sebaceous matter in the follicles; abnormal structure of the sebaceous gland, by Hebra termed "degeneration," while we should call it arrest of development; hypertrophy of papillæ; and hyperplasia, or new formation of papillæ; to which he further adds, hemorrhage into the rete mucosum, and illustrates this state by reference to Willan's lichen lividus,—an evident mistake, since the lichen lividus of Willan is nothing more than purple and livid papulæ. It is well, however, that we should remember that the word pimple has a general meaning in addition to its specific and technical signification.

THE VESICULA is a prominence of the epidermis containing an aqueous fluid, of minute size and variable shape, being sometimes conical in figure, more frequently semiglobular, and sometimes flattened or depressed. Willan

defines it as "a small orbicular elevation of the cuticle, containing lymph, which is sometimes clear and colourless, but often opaque, and whitish or pearl-coloured; and succeeded either by scurf or by a laminated scab." The average range of size of a vesicle is half a line to two lines, and its height somewhat less than the breadth of its base. A vesicle three lines in diameter, that is, of the bulk of a small pea, must be regarded as of large size; and if it be larger, we should term it *phlyctæna* and *phlyctis*, or, following Latin phraseology, *bullula* and *bulla*. The vesicles of the smallest kind are those of eczema; namely, about half a line in diameter; those of miliaria are as large, and generally larger than a millet-seed, the diameter of the latter being one line; then above these we have the large vesicles, the almost phlyctænæ of herpes, as large as moderately-sized peas, measuring two and three lines in diameter, and leading upwards to the bullæ of pemphigus.

The pathological seat of the vesicle is the same as that of the punctum of hyperæmia or of the prominence of papula,—namely, the aperture of a follicle; and when a vesicle increases in bulk it takes in one or more neighbouring pores. Its shape is governed by the degree of resistance afforded by the cuticle, and in some measure by the force of the exudation; in the neighbourhood of the hair-follicles, the vesicle is semiglobular, sometimes irregular in outline, or angular, in accordance with the figure of the areæ of the lines of motion; sometimes solitary, and sometimes clustered around the opening of the follicle like a row of beads. On the finer skin between the fingers, where there are no hairs and only perspiratory pores, and where the cuticle is thin and moist, the vesicles are conical in figure; and under the same conditions, on the trunk of the body, and where the hyperæmic base is greater than between the fingers, the vesicles are semiglobular, as in miliaria. These same conditions, and especially the thinness of the cuticle, are the occasion of the brilliant transparency of the conical vesicles developed between the fingers in scabies, and, *par excellence*, of the hemispherical vesicles of miliaria. On the other hand, wherever the cuticle is thick and dense, as on the palm of the hands and the palmar surface of the fingers, the minute effusions of lymph fail in the power of lifting the epidermis into vesicles, but may be seen through the cuticle in the form of globular cavities, and where they are very numerous, they are apt to lift up the entire epidermis of the part; for example, the whole of the palmar surface of an internodial portion of a finger, or a considerable extent of the palm of the hand. At other times, in consequence of the development of vesicles in great numbers, and often in clusters, they communicate with each other, and form beneath the cuticle multilocular spaces, sometimes of considerable extent; vesicles, therefore, may be dispersed or scattered over the skin, when they are termed *discrete*; or, they may be congregated in great numbers, so as to touch each other by their bases, when they are said to be *coherent*: or, they may be still more closely packed and communicate by their cavities, when they are termed *confluent*. An eruption of eczema vesiculosum generally presents all these forms at the same time: in miliaria the vesicles are nearly always discrete; while in herpes, when the clusters are full, there is always some confluence of the matured vesicles.

A vesicle always makes its first appearance in the immediate circumference of a pore; it may be on one or other side of the pore, or it may form part of a row which surrounds the pore: or if, on the one hand, the pore be small—for example, that of a sudoriparous duct,—or the vesicle itself be moderately large, the vesicle may cover the entire circumference of the pore. In the latter case the aperture of the pore may be seen on the summit of the vesicle; or it may be tilted to one or the other side; and sometimes, by its connection with the sheath of the follicle, it is held down in such a manner as to flatten the summit of the vesicle, or to give the summit an indented, or, as it is commonly termed, an umbilicated appearance.

It must be mentioned, also, that vesicles present some differences in their contents, having reference to their age: at their first appearance the fluid which they contain is limpid and transparent; in the course of a day it is lactescent and opaque, and at a later period it becomes yellowish. Patho-

logically it is an albuminous lymph, more or less tenacious and viscous, and its opalescence is due to the occurrence of changes in its composition which lead on to the production of pus, and convert the vesicle into a pustule. We have examples of these changes in eczema, which convert an eczema vesiculosum into an eczema pustulosum; we see it also in herpes, and still more strikingly in vaccinia and variola. In both the latter affections the first developed of the primary cutaneous lesions is redness; then follows a papula hard and gritty to the touch; then a vesicula; and finally a pustule. In miliaria, on the other hand, the change is limited to opalescence, and scarcely ever runs on to the purulent stage. It must also not be forgotten that a vesicular eruption may be subjected to pressure or friction, and that such violence may result in disruption of the vessels of the derma, and the effusion of blood into the vesicles; in such a case the contained fluid may be pinkish or reddish, or even purplish or black in colour; but the nature of the vesicle remains the same; and this occurrence is only to be regarded in the light of an accident.

Older pathologists were wont to look upon the production of a vesicle as the consequence of a passive transudation of the fluid part of the blood from its vessels; but the distinguished Virchow and his school treat of it as a physiological operation of the cells. According to the former, the serous fluid was poured out upon the surface of the corium, and so lifted the epidermis in totality from off its basementary bed; but, at the present day, and in conformity with the cell-theory, we look upon the transuded fluid as a product of the rete mucosum, absorbed from the tissues of the corium by the cells of the rete, and exuded from the latter at its surface, so as to break up the connexion of the mucous and horny layer of the epidermis, and lift up the latter in the form of a dome. It is also to the vital operations of the rete mucosum that we must look for the explanation of the opalescence of the transparent lymph that first makes its appearance, and for the development of pus-globules and the conversion of the lymph into pus, as in some instances occurs.

In speaking of redness as a lesion of the skin, we had occasion to remark that hyperæmia was sometimes followed by exfoliation of the cuticle; the termination of papula is, in general, a thin scale corresponding in size with its inflamed base; but vesicula, as it combines with the hyperæmia of papula the production, besides, of a morbid secretion, gives rise to a more decided and thicker scale, and, in general, a *crust*. When a sudden outbreak of eczema vesiculosum, such as the eczema solare of Willan, quickly subsides, the contents of the vesicles are dispersed, partly by absorption and partly by evaporation, and the separated cuticle dries up into a thin scale, and is cast off in due season, of a size and thickness scarcely greater than that of papula, excepting in the instance of a coherent or confluent eruption, when the desquamation would be more extensive. Miliaria terminates in this way by a thin and almost inappreciable scale. But when the vesicle lasts longer, and runs through its opalescent to its pustular stage, it is apt, by the desiccation of itself and of its contents, to form a covering of considerable thickness, which is no longer a scale, but a scab or a crust. Such are the thick and hard scabs of herpes, firmly and deeply embedded in the skin, at first amber-coloured, then brown, and sometimes black; remaining apparent for a considerable time, and leaving cicatrized pits when they fall off. Of another kind is the crust formed by eczema, when it assumes a chronic character, and pours out a morbid secretion. In this instance it is not the vesicle which forms the crust, but the secretion exuded from the diseased surface. This secretion is partly serous and partly purulent, and, not unfrequently, it is discoloured with sanguineous effusions: hence the crust varies in consistence, in thickness, and in colour; it may be dense or friable; it may be thick and porous, or laminated; and it may be greyish, or brownish, or greenish, or yellow, reddish, or almost black. Crusts of this kind are met with on the face and scalp, in cases of ichorous and pustular eczema, and especially in eczema infantile, in that frightful-looking affection termed *crusta lactea*. But *crustæ* will form a theme for future consideration, as being one of the

secondary lesions, and we allude to them here, partly in consequence of being a mode of termination of vesiculæ, and partly because we desire thus early to call your attention to the three words, scale, scab, and crust. *Scale* is the thinnest of the three, and may be the separated cuticle alone, or the cuticle in conjunction with a thin varnish of desiccated albuminous secretion; *scab* is the desiccated vesicle, together with its contents, and sometimes includes a portion of the deeper tissues involved in destruction, as in herpes; while *crust*, however thick and hard, is superficial, and the consequence of the desiccation of morbid secretions. If you look to your dictionary, you will find that a *scale* is a thin lamina, anything that is exfoliated or desquamated; a *scab* is an incrustation formed over a sore by dried matter; and a *crust* is a collection of matter into a hard body.—*Journal of Cutaneous Medicine, July, 1867, p.* 124.

75.—ON THE TREATMENT OF LUPUS.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Diseases of the Skin.

The internal remedies for lupus may, I think, be very safely divided into three classes,—those which possess a certain amount of value, those of very doubtful value, and those of no value at all.

The only remedies which I have observed to possess an undoubted control over lupus, and which will, I believe, in every case effect a certain amount of good, and in a certain proportion of cases bring about a cure, are arsenic and calomel in lupus of the face, and iodide of potassium in lupus of the limbs. Of course, there may be others, some known, some to be yet found out; I am limiting myself here strictly to facts which I have been able to verify.

Among the remedies of doubtful value I would put all the salts of barium, antimony, soda, mercury, iodine, and potass, with the exception of those in the first class; all combinations of salts of these, such as iodide of mercury, Donovan's solution, &c.; all the oils, such as cod-liver oil; and all vegetable preparations whatsoever. In ranking these remedies as doubtful, I do not in any way seek to invalidate the conclusion formed by some writers as to their power over lupus; our knowledge of them may be as yet in its infancy; for instance, with respect to the chloride of barium, which Dr. Fraser, a most careful observer, has seen cure lupus of the limbs. What I mean is this, were any given number of cases of lupus put before a surgeon, he would not be able to say that *with these remedies only* he could rely upon curing a certain proportion of them, or perhaps a single case. The disease is often benefited for a certain time and to a certain extent by some of them,—for instance, iodine and iodide of potassium in lupus of the face; but in all the cases I have seen the amendment was very slight, and soon came to a standstill. M. Devergie treated twenty-six cases in this way for three months, and not one of them was cured. Cod-liver oil too produces often a degree of improvement, but it is equally uncertain. I would always give it as an auxiliary, but would never rely upon it to the exclusion of curative means.

Among remedies of no value, I think, we may safely rank steel, quinine, mineral acids, bitters, sedatives, alteratives, and sudorifics, change of air, sea-bathing, baths of every kind, and possibly many of the preparations naturally falling into the second class. This may seem going too far; I can only say that I see no other conclusion to be drawn, as I never noticed any beneficial change in the lupus produced by any one of these remedies. For instance, I have notes of several cases in which iron was prescribed sometimes by myself, sometimes by others. Among the preparations used were the tincture of the hydrochlorate, the magnetic oxide, the iodide, the sulphate, and the freshly prepared carbonate, made by mixing solution of the sulphate with solution of carbonate of potass, Griffith's mixture, &c. In no one instance did any of those preparations exert the slightest appreciable influence over the disease. Sometimes the health underwent a certain degree of improvement, but generally there is not much to improve. In the same way when mineral

acids, such as the nitric or nitro-muriatic, were given, the appetite became more keen, and possibly, were the whole community treated in this way, a certain number out of every thousand would exhibit the same results; indeed it would be folly to dispute their power in promoting *the health*, their value as *auxiliaries;* but as to the influence of these remedies *over the disease*, they are, for anything seen to the contrary, as capable of producing as of alleviating it.

Change of air and sea-bathing are often recommended in this complaint, as they would be recommended for the results of over-work, anxiety, and confined air; for beginning mischief in the brain, or the lingering mischief of bronchitis. Yet it has nothing in common with any of them. It pursues its relentless course with equal severity on open plains and crowded alleys, among the hills of Cumberland and in the bracing air of the east coast, in the dry, bright climate of France, and the perpetual drizzle of Ireland or West Scotland, and often rages with greater severity in the place the patient is sent to than in that which he has left. Under such circumstances only a very sanguine mind could look for benefit. Benefit may, it is true, follow change of air, but so may a very decided relapse; and the circumstances which determine either have not yet been so far elucidated as to enable us to say more than that it is a hazardous step.

With very few exceptions the external applications recommended in lupus are useless and impracticable. For instance, soothing or cooling, antiphlogistic or alterative dressings, ointments and lotions, leeches, &c., are, I believe, perfectly useless. Were they continued for a lifetime, it is very doubtful if they would ever check the march of the ulceration. Again, all remedies strong enough to give severe pain, such as chloride of zinc, Vienna paste, Canquoin paste, are and ever must be inapplicable, for the simple reason that, however valuable they may be, a very large number of patients will never suffer them to be used; others having once submitted will never allow a second trial to be made, and a third class will only consent when it is too late to prevent irremediable deformity. As to whether they effect a cure or not when they have fair play, that is beside the question; the difficulty is getting them fair play; and whatever may be said in their favour, I apprehend their use will always be restricted to a small proportion of cases, principally among patients not very susceptible of pain, or unusually resolute in bearing it, and not easily deterred by failure or relapse,—for both will occur; or where one small solitary part is invaded by the disease.

For the purposes of treatment, all cases of lupus may, I think, be advantageously divided into two great groups; one embracing lupus of the limbs only; the other lupus of the face and head, complicated or not by affection of the limbs. The arrangement is, I admit, extremely unscientific; but there is reason to think it will prove effectual in examining the action of medicines, the varying power of which over different structures, and even the same structure in different parts, has not yet attracted the attention which, I think, it might fairly claim. Some years ago I endeavoured to call attention to this subject, and attempted, by a series of proofs, to show that medicines which are invaluable in a simple inflammation of one structure are powerless against the same affection in another; thus, for instance, opium acts most beneficially in peritonitis, and inflammation of the subcutaneous cellular tissue is often promptly checked by the free use of large doses of tartar emetic, yet this salt seems to exert no influence over peritonitis, while inflammation of the cellular tissue is only controlled by it in so far as the pain is relieved. Again, the very same inflammation in different mucous membranes, as is seen in ophthalmia and urethritis, may run a widely different course, and be very differently affected by the very same remedial measures. It would lead the reader too far out of his way, were I to detail all the statements upon which this view is based. It must suffice to point out the general principle and to draw attention as far as possible to it, because it offers a nucleus for future observations, around which other facts may accrete, as crystals form round a nucleus.

So far as I have been able to observe, the most efficient remedy against

upus of the limbs is iodide of potassium. I am not speaking here of tertiary ulceration, but of that form which is often confounded with it—obscure lupoid ulceration, unconnected with syphilis or lupus of the face. For the most part, it only requires to be given in very moderate doses, and the necessity which exists in syphilitic cases for prescribing large quantities and steadily raising the amount given, does not obtain here. In general, a drachm weekly is quite enough at first, and it is seldom necessary to go beyond twice this amount. When it cures the disease, I believe it always acts soon, and the action goes on till the part is healed. When the improvement comes to a standstill, I am disposed to think that augmenting the dose will have no effect.

As to the mode of giving it, that may be safely left to the discretion of the surgeon. Perhaps one of the bitter infusions will answer as well as anything. One precaution should never be omitted,—that of getting the salt from a source where we can rely upon having it pure. It is not, perhaps, so much adulterated now as it used to be; indeed, I have been told, upon very good authority, that the adulteration rarely exceeds ten per cent., though, judging from the great difference in the action of prescriptions made up at different places, I should have considered this statement below the mark. Still even an admixture of ten per cent. may make a very material difference.

Should the bowels be confined, a mild pill may be given; in fact, in this as in any other complaint, all complications should, as far as possible, be swept away, care being taken at the same time not to interfere with the action of the iodide; for instance, in giving aperients, the alterative action of mercury should be guarded against. But I should think it was quite unnecessary to enter into details upon such a point; to keep on repeating that when gout is present colchicum combined with salines may be prescribed, and that iron is called for in anæmia; that impaired digestion and painful menstruation must be relieved; that we must attend to the general health and improve the secretions;—every surgeon knows, or ought to know, all this.

When we have to deal with lupus of the head or face, I believe the first remedy to be given is arsenic. Unless some disorder of the health, such as loss of appetite, great weakness, or anæmia is present, all preparatory treatment is, I think, wasted; but when the tongue is coated and marked by the teeth, the breath foul, the bowels confined, and the appetite bad, a saline may be given for a few days, followed by nitric acid in bitters. The only question worth considering is the mode of giving the mineral and the dose suited to the case in hand. As to the mode of giving it, I believe that most of those who have tried Dr. Valangin's solution (liquor acidi arseniosi hydrochloridi), consider it one of the best preparations ever invented, possibly superior even to the old Fowler's solution. Twelve minims may be given three times a day, with the food or directly after. It should be taken regularly, and the dose should always be measured out in a glass measure. Should this dose produce no disturbance of the stomach or bowels, no nausea or headache, no languor, or irritation of the skin, it may be brought gradually up to about sixty, or in a very few cases seventy minims daily. The former quantity is generally quite enough, as the medicine has to be continued for a long time. Perhaps simple water is as good a vehicle as could be devised; but so long as the efficacy of the arsenic is not interfered with by the addition of alkalies, mercury, or iodine,—so long as what is given with it can be given for a long time without any injury to the health—it may be prescribed in any form; but the simpler the better.

But though it will perhaps always effect a certain degree of improvement, it can scarcely be relied upon in a certain percentage of cases for producing a radical cure of lupus. It has been vaunted as a specific for the eating variety. I can only say that I cannot confirm the view. So long as the improvement seems to progress, however slowly, it would not be wise to interfere with it; it will often cure single-handed, and we thus sweep away one more complication from treatment; but so soon as the amendment comes to a standstill, so soon as ever a sufficient time has elapsed to form a valid reason for believing that it is really doing no good, and still more, as soon as ever

there is a threatening of relapse, the use of mercury should I think, at once be resorted to. I know of no remedy which so quickly checks the spread of lupus as calomel. With it I have repeatedly arrested a sudden outbreak in a patient actually under the influence of arsenic; and what is more, the same result has occurred in the hands of others, who have thus been led to the same conclusion as myself. The fact has been so often noticed by the patients themselves, that so far from dreading the continued use of mercury, they are often more anxious to take it than I am to prescribe it, although I have never yet seen any harm ensue from the proper employment of this remedy.

It may be given in the form of a pill, mixed with any substance likely to lessen the griping and pinching pain which it may cause in a few cases. A grain or two will, as a rule, be enough to begin upon; but as in the majority of cases this dose speedily loses its effect, it will in such cases be necessary to raise it at the end of a week or two. On the average this quantity does not require to be often increased, and then not largely. From two to three or four grains may in general be considered the outside limit, but no certain rule can be laid down. The only sure test is its purgative action: two, three, or four loose stools should follow each dose, and when this does not ensue, a saline aperient, such as the effervescing citrate of magnesia, a vegetable aperient like senna, or what is perhaps the best, a compound black draught, may be given the morning after; and a compound can be used, because the object here is not to investigate the action of either senna or magnesia, but to assist the action of the calomel, which is being investigated. With proper care there should be no action on the gums; but if any set in, mercury should be given up, and only the draught prescribed till this symptom has passed off. To prevent all needless repetition, it may be said here that these directions hold strictly good with respect to giving calomel for lupus of the limbs, except that it is generally less often called for and for a shorter time.

When there is a large open surface, the liquor plumbi may be used. The fluid should first of all be warmed, by placing it over hot water, and so soon as it begins to smoke it should be painted several times over the surface. What is left had better be thrown away. It ought to be applied at least once daily, all crusts and exudations being previously removed, and if the patient thinks any relief is obtained from its more frequent use, then let it be used more frequently. I am indebted to some of my correspondents—Dr. Hinds, of Birmingham, and others,—for information about its value, and I am disposed from several trials I have made, to think very favourably of the remedy. Mr. Erasmus Wilson recommends it in lupus erythematosus and in scrofuloderma.

The acid nitrate of mercury is a very valuable remedy. When its use is restricted to small surfaces, to tubercles and to patches in which the morbid action has been a good deal subdued, or which are healing, but so slowly as to justify a resort to any means which will hasten the process, it is often of great service. With proper care, the pain attending its employment is so trifling as to constitute no valid objection. It should at first be brushed very lightly over the part, and a basin of water should be at hand to bathe the surface with immediately; but familiarity soon breeds contempt here, and the patients generally cease to avail themselves of the chance of washing the acid away. The application should be repeated daily, both because the crust which is thus formed constitutes an almost impregnable barrier against the impact of the air, but also because the acid acts more painlessly than when only occasionally laid on. How it acts I do not profess to understand, —possibly by coagulating the albumen in the epidermis or the blood. Subsequent dressing (except in the eating form, when water dressing may be used) is, I think superfluous; the principal thing is to protect the surface from the air, especially cold raw east winds, under the malignant agency of which lupus will sometimes relapse in a few hours as much as has been gained in a week; indeed as far as possible, I should say the patient ought to be confined to the house. Dr. Gilchrist, of Torquay, gave me the par-

culars of a case in which a lupoid ulceration of the nose was cured apparently solely by excluding the air.

For cases intermediate in severity between those which are suited for the quor plumbi and those which require the acid nitrate of mercury, I should y the nitrate of silver is one of the best. At the beginning I would suggest a very sparing use of it in solution, not more than ten grains to an ounce. his quantity, however, may be *rapidly increased*, and after a few applications, the solid nitrate may be resorted to. Dr. Purdon, in a valuable communication which he has favoured me with, states that he gives the patient loroform and then bores the nitrate deep down below the surface of the lcer. His treatment seems to have been very successful. It is also a vourite remedy with Hebra, Dr. Alexander Anderson, and many others.

To my thinking, a lupoid surface should only be washed with very hot ater, and when soap is used it should be of the most unirritating kind that an be procured. The diet ought to be good, and fat ought to enter largely ito it. Where fresh fat meat cannot be borne, I would suggest the daily se of mild bacon or ham for breakfast. Too much meat is as injurious as o little; about half a pound a day for an adult has always seemed to me nough. Too much beef tea, jelly, or strong soup, like over-feeding, seems me to overtask the digestion without aiding the nutrition, and this mistake more than any other to be particularly guarded against when the patient taking tonics. From any ale—except pale ale—I have never seen any ood come; but a free allowance of beer, or, where it can be procured, good ort wine, is invaluable. By far the best stimulant, however, is, I think, ure old rum given in milk, which possibly supplies something deficient in he nutrition.

The results of this treatment have been even more satisfactory than when brought forward the subject last year. So far as all the inquiries I could ake have yielded any response, it is to the effect that only two cases have elapsed, and in both cases the patients did their very best, not only to ring about a relapse, but to aggravate it to the very utmost; whereas in ree of the cases, then to some extent doubtful, a perfect cure has, accordg, at least, to the reports of the patients themselves, taken place; and one hich seemed hopeless had, when last seen, greatly improved. Other cases ave gone on equally satisfactorily, which want of space will not allow me dwell further upon.—*Journal of Cutaneous Medicine, July*, 1867, *p.* 149.

76.—ON ACNE AND ITS TREATMENT

By Dr. Marris Wilson.

The Medical Times of 1859 contains a notice of Dr. Rodet, who recommends the following ointment as serviceable in all the forms of acne. It is composed of — Washed lard, 50 parts; sublimed sulphur, tannin, of each 4 parts; laurel-water, 5 parts. The proportions of sulphur and tannin are to e increased, according to circumstances, to 6 or 8 parts. The author however, throws a doubt over the special fitness of this treatment, when in a succeeding paragraph, the same ointment is advised for sycosis, an affection f the hair system, so different in cause and situation from the disease of the ebiparous system, and so widely separated by modern classification. For ycosis the ointment is prescribed after the inflammation has been attacked and he crusts detached.

In the same journal is presented the treatment advised by M. Ferrat, xtracted from the Bulletin de Thérapeutique, 1859, which very closely esembles that reported in my former paper, as pursued by M. Hardy, at a ater date, in the wards of the Hospital Saint-Louis. When the affection is n its earlier stages, and the attack not severe, and after all the causes which ppeared to maintain it have been removed, the treatment is to be commenced y the application of spirituous lotions, either tepid or hot; a teaspoonful of

the following solution being added to a glass of tepid water, and applied night and morning:—

℞. Hydrarg. bichloridi, 1 part; alcohol q. s. ad solvend.; aquæ distillatæ, 100 parts. Misce.

M. Ferrat declares that certain forms of acne, especially the punctated, and one obscurely designated sebaceous, may be certainly cured by the application of astringents locally, and advises the use of alum and peroxide of iron, in the following formula, increasing their strength when necessary:—

℞. Aluminis, 30 parts; aquæ, 300 parts. Misce. To be used as a lotion night and morning.

℞. Ferri peroxidi, 1 part; axungiæ, 60 parts. Misce. To be applied at bedtime.

When the case is more severe, protiodide of mercury is to be substituted for the iron, increasing the dose according to circumstances, to double the strength. Should the cure be long delayed, or imperfect, the biniodide is to be employed instead; and if the attack be very intense, the biniodide is to be used in the first instance.

I am not sure if my investigations have led, even in a small degree, to the fulfilment of the expectation entertained in the commencement of my undertaking, namely, that of promoting speed and facility in the cure of acne: but it is quite certain that many proposals for treatment have presented themselves to me, clearly of an opposite character. To the plan, which I cannot resist detailing below, on account of its amazing redundancy, I can hardly apply a stronger opinion than that it is simply a project of treatment, for it must appear very doubtful if any unhappy patient could be found willing to submit to its practical application. However, it is put forward as a mode of cure, and it shall speak for itself. At the same time I do not feel inclined to make myself responsible for the definition of the forms of disease named, more particularly as they seem to apply rather to gutta rosacea and syphiloderma.

In the Gazette Médicale de Paris for 1862, Dr. Hedenus of Dresden, giving clinical instruction "*sur l'acne rosea*," meaning thereby *red nose*, says that all the world knows that acne is one of the diseases most rebellious to treatment. His mode of practice, for simple redness of the nose, commences by the following medicine, administered internally:—

℞. Carbonate of soda, ʒiiss.; to be dissolved in eau de melisse, ℥vj.; add extract of orange-peel, ℈iv.; and take three spoonfuls every day.

As an external application, to smear over the surface of the nose, the following is to be used:—Laudanum, 47 drops, mixed with 4 grains of extract of belladonna. The nose is to be well washed in the morning, and small pieces of linen dipped in cold water are to be applied several times a day. When this has been pursued for some time, the following pills are to be taken, eight or ten of them, twice a day; the formula is:—

℞. Sodæ bicarbon, ℈v.; pulvis rhei, ℈iiss.; ipecacuan., gr. iij.; extracti coloc. co., gr. xv.; extracti chelidon, maj. q. s. M. Fiant pilul. xciv.

If the patient be not relieved at the end of six months, then a mineral alkaline water should be tried, such as the baths of Marienbad or of Homburg; and this treatment, if necessary, is to be continued during three years.

If the redness depend in any degree upon a scrofulous diathesis, whey mixed with vegetable juices, muriatic baths, &c., will prove serviceable, while, as an external remedy, the following ointment is to be applied:—

℞. Axungiæ, ʒiss; zinci sulphatis, gr. ij.; extr. thebaicæ, aquos., gr. iv; extract conii, gr. viij. M. To be spread upon linen and applied alternately with cold fomentations.

During the winter a spoonful of a mixture is to be taken three times a day, compounded of muriate of baryta, and essence of hemlock, dissolved in cherry-laurel water. At the same time one of the following lotions is to be used for external application:—Solution of acetate of lead, laudanum, tincture of benzoin, alcohol, and elder-flower water; or, solution of lead, acetate of ammonia, and water.

During the summer time the waters of Ems in particular are to be used

If the cause of the *red nose* depends on menstrual or hemorrhoidal disorder, foot-baths containing nitric acid, and frequent applications of derivatives to the inferior extremities, are useful as external means, while the pulp of potato or scraped carrot may be applied directly to the nose.

At the same time there are to be taken in the morning two spoonfuls of an electuary, composed of agrostis, taraxacum, chelidonium, and millefolium, with a glass of Seltzer water.

The reporter, Dr. Goeschen, exclaims:—"This complicated medication, in which are seen to figure so many remedies, and the mineral or thermal waters of Marienbad, Carlsbad, Homburg, Ems, &c., is appropriate for an affection designated by the author as *simple red nose*, acne rosea.

Now let us examine the mode of treatment advised for acne rosacea, *la couperose*. I must not, however, enter upon so much detail, for the sake of space, as in the previous case, and it will be enough to sketch the plan proposed loosely.

In the first place a liniment, composed of extract of belladonna, extract of opium, ointment of nitrate of mercury, and olive-oil, is to be spread upon the affected parts, by the aid of a pencil or small piece of linen, and allowed to remain for half an hour. If this prove unsuccessful, a strong solution of corrosive sublimate is to be applied for a quarter of an hour two or three times a day, after the plan of Hebra. While this is being used, a very efficacious remedy — muriate of zinc, muriatic acid, and water—is to be taken internally as a mixture; or the muriate of zinc may be taken with extract of myrrh and extract of guaiacum, in the form of pills, according to circumstances. In case the liver should become very irritable under this treatment, cod-liver oil is to be given in large doses, up to nine spoonfuls a day, while the same oil is to be applied externally.

Dr. Hedenus also recommends iodide of potass, combined with extract of hemlock, in progressive doses, commencing with 2 grains twice a day, increased every day by 1 grain, until a dose of 16 grains twice a day is reached, and then the quantity is to be diminished in the same progression until the original dose is attained. As a local application, ointment of corrosive sublimate is to be applied on linen during the night.

Should all these means fail, it is advised to commence afresh with the baths, a few new localities being added to those above; such as Töplitz and Aix-la-Chapelle, taking at the same time pills composed of crude antimony, extract of walnut, and powder of bitter-sweet, and spreading upon the skin, with a camel-hair pencil, a liniment of oxydulated acetate of mercury, glycerine with starch, recent butter of cacao, and oil of almonds well mixed together.

Such is the plan advocated by Dr. Hedenus; but he appears still to have considerable doubt as to its sufficiency; and thus, after all these medicines, mineral baths, and years of medication may have proved unsuccessful in curing the disease, I think I cannot be rash in adopting the *dictum* that acne, regarded from this point of view, is really "one of the diseases most rebellious to treatment."

The treatment of the most recent time is of a very systematic character, and designed to correct constitutional disarrangement. Its principle is most concisely set out in Mr. Erasmus Wilson's *Student's Book of Cutaneous Medicine*, and consists in regulating the functions of digestion and secretion, by bitters, mineral acids, and chalybeates, and by employing the ferro-arsenical solution in special conditions of nutritive debility. The torpid and weakened tone of the skin is to be remedied by hypochloride of sulphur ointment, rubbed upon the eruption at bedtime, followed next morning by thorough washing with soap and water. Bichloride of mercury dissolved in spirits of wine, or bitter almond emulsion, preceded by juniper-tar soap and water, is another valuable remedy recommended by the same author.—*Journal of Cutaneous Medicine, July*, 1867, *p.* 158.

77.—CARBOLIC ACID IN SMALL-POX.

By Dr. Horatio Yates, Kingston, Canada.

[The following case is that of "a beautiful young lady," who was seized with small-pox in a severe form whilst passing through Kingston.]

The first object, after saving the patient's life was to prevent disfigurement. The ward, of which she and the nurse were sole occupants, is large, lofty, and thoroughly ventilated. The temperature throughout was maintained at 64° degrees during the day, and 58° at night. The room was darkened, and a candle only used for light. The usual general treatment was adopted—viz., animal broths, bread, &c., for food, four ounces of port wine daily, and a quarter of a grain of morphia in half an ounce of brandy at night. On and after the ninth day of the eruption, the quantity of wine, morphia, and brandy was doubled; Gregory's mixture was also given when necessary.

But I am induced to transcribe as briefly as possible the local treatment, its success having been so gratifying. We have for years expected to prevent pitting of the face by excluding light and air from its surface. For many years I have habitually used nitrate of silver, tincture of iodine, mercurial ointment, &c., to the face for this object, with variable success. In this case I employed an ointment—of carbolic acid, two drachms; mutton suet, two ounces, and coloured with lampblack. This ointment was spread thickly upon black cotton wadding, which was applied over the face and forehead, holes being cut for eyes, nostrils, and mouth. This mask was changed every second day, and the face gently washed with soap and warm water, and then including the whole body, with warm water impregnated with carbolic acid.

From the commencement of the eruption no unpleasant symptoms occurred. There was none of that intolerable itching, and no secondary fever—the suppurative fever—ever made its appearance, and on the separation of the last scab with the removal of the last mask, I was delighted to find that not a single pit had been produced.

I am persuaded that the carbolic acid diminished the amount of the suppuration and its duration, and that it prevented the secondary fever. Mr. O'Sullivan, the intelligent house-surgeon, smeared daily one arm of the patient with the ointment. The contrast in the character of the pocks on the two arms was remarkable. The pustules on the arm operated upon were not nearly so full, and the scabs fell off earlier than those on the other arm.

Whatever part the carbolic acid played in this case in preventing pitting, or how much the exclusion of light and air had to do with it, I cannot say; but this we do know, that carbolic acid is a powerful disinfectant, and that it destroys microscopic vegetations. It certainly prevented the peculiar odour of small-pox in the room, and, as I have already said, I attribute the absence of secondary fever to this agent.—*Lancet, Jan.* 25, 1868, *p.* 151

78.—ON THE USE OF CARBOLIC ACID IN BURNS.

By William Pirrie, Esq., Professor of Surgery in the University of Aberdeen.

[Having read Professor Lister's paper on the use of carbolic acid in wounds, it occurred to Mr. Pirrie that it would prove of great service in cases of burn, and he determined to try it on the first opportunity. This soon occurred, the patient being a girl eleven years of age, who had been scalded severely on the face, neck, and one shoulder. She was in great pain.]

Two folds of surgeon's lint, dipped in a liniment of one part of carbolic *acid to* six parts of olive oil, were closely applied to the whole of the scalded *surface,* a double layer of tinfoil was placed above the lint, and the whole *secured by* means of a bandage. The air was thus completely excluded;

and in ten minutes, the patient, much to my surprise, stated that she was free from pain. On the second day after the accident the skin was greatly improved, and the bullæ seemed withering away. On the twelfth day the skin was everywhere perfectly healed, the cuticle having been thrown off; and, although the case was watched with the utmost care, not a single drop of pus was discovered.

The sudden and perfect subsidence of the pain, the withering of the bullæ, the complete healing of such an extent of scalded surface without the slightest suppuration, in a patient of feeble constitution and greatly depressed by shock, appear to me to show that carbolic acid is well deserving of trial in burns of the first and second degree. I have seen some scalds of less extent prove fatal, and I have seen many not so unpromising at first end in suppuration and ulceration of skin, and require months to heal.

For making clear the state of parts in the first and second degrees of burns, I have been in the habit of stating to our students that they are precisely the same as those which usually result from the application of a blister; the inflamed part, without separation of the cuticle, resembling the first, and the part covered by vesicles and bullæ resembling the second, degree of burn. This resemblance of the states of parts in blistered surfaces to those in the first and second degrees of burns suggested to me the desirableness of trying the effect of dressing a blistered surface with the above-mentioned liniment of carbolic acid and olive oil. This was done in the following case, which was carefully watched and recorded by Mr. Nicoll, one of the most talented of our students, who at the time was doing duty for our house-surgeon.

Helen W., aged 27, was admitted under my care on the 27th of August, in consequence of an injury to her right hip-joint, caused by a heavy sack of meal having fallen upon her from a great height. Some subacute inflammatory action took place in the joint; and, after other treatment, a blister of the size of the hand was applied, on the 4th of September, to the outside of the hip, and allowed to remain for seven hours till vesication to a considerable extent was produced, when it was removed; the sensations of the patient and the condition of parts being precisely the same as in burns of the first and second degree. The blistered part was dressed in every respect as in the preceding case of burn, and in fifteen minutes the pain entirely ceased. On the morning of the 5th of September the blistered surface and bullæ presented the same appearance as on the removal of the blister; on the evening of the same day the smaller bullæ had begun to disappear. On the evening of the 6th the smaller bullæ were gone, and the larger greatly diminished; and on the evening of the 8th of September the parts were perfectly healed, no suppuration having taken place.

In both cases carbolic acid seemed to accelerate the subsidence of local symptoms, to procure rapid and perfect relief from pain, and to promote healing without suppuration. If in other cases of the same degrees of burn, carbolic acid should be equally useful, it would be a great blessing in the treatment of these dangerous and painful injuries.

Although water may answer for diluting carbolic acid, when used for some purposes, oil must be preferable in cases of burns.—*Lancet, Nov.* 9, 1867, *p.* 575.

79.—CARBOLIC ACID IN THE TREATMENT OF SCALDS OR BURNS.

By Dr. Allan Wilson, Castle Eden.

As carbolic acid in surgery is justly occupying the attention of the profession at the present time, it may be interesting to some of your readers to know that I have recently used it with marked benefit in cases of severe scalding. The way in which I apply it is as follows:—

1st. To thirty pints of the ordinary oil and lime-water, add one of the acid and mix.

2nd. Saturate linen rags in the carbolic emulsion, and spread them on the scalded parts.

3rd. Keep the rags moist by smearing them over frequently with a feather dipped in the emulsion.

4th. Cover the application with oiled-silk or gutta-percha tissue.

The advantages of this mode of treatment are twofold, as was well exemplified in two cases I have just brought to a favourable termination, one of which was of a most serious nature, involving the face, neck, and chest of a child two years of age.

1st. It excludes or destroys the noxious elements of air, and in this way will diminish shock by removing the cause of local irritation.

2nd. The sores are allowed to heal rapidly by a natural restorative action, without the formation of pus, consequently preventing a most frequent secondary cause of death in burns—namely, purulent infiltration of the lungs or other internal vital organs, and also in a great measure preserving the personal appearance of the patient—a matter of no small importance to some people.

Case 1.—Mrs. M., aged 40, on the 20th November last got her forearm severely scalded with hot tea. The ordinary oil and lime-water was applied at the time by herself, and continued for two or three days, when she got alarmed by the arm becoming much swollen and very painful, and sent for me to see it. I gave her full directions how to apply the carbolic acid treatment, which she attended to strictly, and to her great astonishment the arm was well in less than a week.

Case 2.—C. E., aged 2 years, the daughter of a farmer, on the 23rd of December last slipped from a stool on which she was standing, and, in attempting to prevent a fall, pulled a pailful of boiling hot water off the table on to her face and bosom. The epidermis of the breast and shoulders came off for the most part with the clothes, and that of the face and neck was raised in blebs. On first seeing the case, about half an hour after the accident occurred, it seemed from the extensive injury and extreme state of shock, amounting almost to coma, that the case was a hopeless one. The antiseptic treatment was at once applied, and in four hours afterwards the child was quite conscious and cheerful. From this time she steadily improved up to the 27th, when she suddenly got very weak, and was said to have had a slight rigor, doubtless caused by the temperature of the room being allowed to get low, and also by being put on low diet. Ordered one teaspoonful of brandy in water every four hours, and raw eggs in milk. On this regimen she rapidly improved, and at the present date she is running about almost as well and pretty as she was previous to the accident. All the medicine administered in this case was four grains of mercury with chalk, one of rhubarb powder on the third day.—*Lancet, Feb.* 1, 1868, *p.* 181.

80.—A NEW METHOD OF USING SILVER WIRE IN THE TREATMENT OF WOUNDS AND IN OPERATIONS.

By Dr. S. Wordsworth Poole, Aberdeen, Surgeon to the Cray's Paper-Mills.

Being unaware that silver wire has been employed in the following manner in any of the hospitals, I venture to think that the plan is not much known, though it seems so simple and obvious that it must have occurred to, and probably been practised by, many.

A very common accident in paper-mills is the vertical amputation of one or more fingers, through about the middle of the last phalanx, by the paper-cutting machine. In such a case sutures are not available, an operation is hardly called for, and by the ordinary treatment with water dressings or stimulating lotions, a month elapses before a perfect skin is formed; but by the following means a perfect cure is obtained in, at the farthest, fourteen days:—**A slip of adhesive plaster being taken, about as wide as a finger-nail, four loops of wire are attached to it. They are to be so placed that**

when the plaster, heated, is applied firmly at a little distance from the truncated extremity, four free ends shall project beyond the finger on the dorsal aspect, and four on the palmar. A larger piece of plaster having been applied so as to give additional fixation, the opposite wires are twisted over the wound, two and two, by a dressing forceps, as much as seems advisable. Thus the exposed surface is considerably diminished in size, and a quantity of blood and lymph is forced out, which forms a firm coagulum that does not decompose, and remains until it is found that a new and perfect skin is formed underneath.

The wire may be similarly used in flap operations on the fingers and the great toe, and I have found it much superior to plaster in fastening a lacerated finger to a splint. The opportunity has not occurred to employ it thus in a larger operation; but it seems to me that a modification of it would be useful wherever the circumference of the limb could be embraced by the strip of plaster. The first application is undoubtedly troublesome, but, as a rule, no further interference would be required, and it would not prevent the use of those antiseptic dressings lately come into vogue.—*Lancet, May* 9, 1868, *p.* 588.

81.—ON THE TREATMENT OF PRURIGO.

By J. L. Milton, Esq., Surgeon to St. John's Hospital for Diseases of the Skin.

In all forms of this justly-dreaded affection, the remedies which seem have succeeded best resolve themselves pretty well into six classes.

1. *An alkali.*—Soda, for instance, seems to have always been a favourite ingredient in the prescriptions of the most successful practitioners. I do not attribute much importance to the form in which it is given; perhaps the carbonate, or a mixture of carbonate and sulphite, answers best. If there be much indigestion or acidity, ten grains may be given two or three times a-day, just after meals, either in an ounce of bitter infusion, with which a little compound tincture of cinnamon or spirit of nutmeg is combined, or it may be administered in conjunction with aromatic confection and dilute hydrocyanic acid. These remedies should be continued till indigestion is removed; should no indigestion be present they may be given up at the end of ten days or a fortnight.

2. A remedy is required which will act on the skin. Sulphur in some form or other is perhaps the mildest and most certain. Antimony, however, in such a preparation or dose as will not nauseate or depress, may also be prescribed with benefit, and I see no objection to combining these remedies with a little nitrate of potass. When the skin is very dry and harsh, as it almost always is, a powder containing these ingredients may be given on going to bed, in a little gruel or warm whey. Like the previously mentioned remedies, these need not be continued more than about a fortnight.

3. The use of either of these two sets of remedies should be accompanied by a course of medicine which will act gently on the liver and bowels, as, for instance, the occasional use of small doses of iodide of potassium, with rhubarb, either in the form of infusion or pill. A little mercury may be given in the shape of blue pill, combined with soap, henbane, and jalap or colocynth; or as grey powder, with nitrate of potass. Mr. Startin places mercury under interdict, though I have never seen it do the least harm.

4. But the great internal remedy in prurigo is arsenic, and in all very severe or long standing cases, and in those which do not seem to be benefited by the preceding treatment, I would advise that it should be immediately begun with; and the other remedies used only as auxiliaries, or in the place of the arsenic when it is necessary to interdict it. It may be prescribed in full doses, and when this is done, I am disposed to share Mr. Hunt's opinion that prurigo is not such an intractable disease if properly treated, although I am aware that the statement may be looked upon as a modern heresy, and that Mr. Wilson considers it a stubborn and even a grave complaint in

erly persons. Under the old plan of treatment it seems to have resisted ry attempt to subdue its malignity, and, perhaps, there were more sui- es from prurigo than from all other diseases of the skin put together. Mr. nt says—"Of the entire recovery of a patient thus affected, or even of nsiderable alleviation of suffering, not one single gleam of hope can be thered from any author who has written on the subject. And yet there is truth in the whole circle of medical science more vividly impressed on my vn mind, than that, under proper management, arsenic is an effectual rem dy for this disease." Mr. Hunt bleeds to faintness in refractory cases befor- iving the arsenic; in one case he took about seventy ounces of blood fro he arm, and probably fifteen additional ounces by leeches, and certainl according to his statement, the success seems to have quite justified t means. Lisfranc used also to bleed in cases where the itching was connect with difficult menstruation, and the practice is said to have been very ben- ficial.

5. Certain remedies which seem to stimulate nutrition in some as yet u- known way. Of these two are well worth a trial—strychnia and cod-liver, oil. The strychnia may be given in doses of a sixtieth or a sixty-fourth part o a grain every three or four hours till a decided effect is produced upon the disease or till nervous symptoms show themselves, when it may be left off. It sometimes acts like a specific upon the pruritus, and when this symptom is once thoroughly quelled, we shall seldom, if ever, have much difficulty in dealing with the remaining symptoms. Nux vomica was a favourite remedy with Neligan, but I believe we are indebted to Dr. Burgess for this mode of exhibiting its active principle. It should not be taken along with any other remedy, whereas the cod-liver oil may be given almost as an article of *diet* for a long time, and in moderate doses, quite irrespective of any other medicine the patient may be taking. I confess my entire ignorance as to how cod-liver oil and strychnia may act, and it may, perhaps, save some trouble if I say, once for all, that I am not prepared to offer any explanation of the action of medicines, for the simple reason that in respect to most of them nothing whatever is known for certain, and as to cloudy conjectures and cabalistic forms of speech, I leave them to those who like them. I am content to admire at a distance the person capable of solving such questions as causes and modes of action, as I quite despair of my own ability to do so.

6. A free use of hot baths, especially the Turkish bath. I prefer the latter when it can be used, but when the patient is very nervous, suffering, or supposed to suffer, from disease of the heart, or subject to fainting; where he resides at a long distance from any establishment or baths of this kind, or where expense is an object, the hot bath is our only alternative, and fortunately it is a very good substitute. The water should be at quite ninety-eight or a hundred degrees of Fahrenheit. The patient, so soon as he enters the bath, should scrub himself all over with the flesh brush, lather the affected parts with either the soft soap of the London Pharmacopœia or Pears's transparent soap, and then let himself down into the hot water, in which he ought not to remain more than three or four minutes. Having dried himself thoroughly, he should apply any ointment he may be using and dress directly.

But the Turkish bath is the thing; it scarcely ever fails to do good, and is, perhaps, more peculiarly suited for prurigo than for any other disease of the skin. There are many persons in the habit of sponging all over daily, and who, therefore, have quite made up their minds that they require no other kind of purification. They cannot too soon be undeceived as to the effic of water used in this way: it will no more free the skin from soot, dust, scarf skin, and secretion, than rubbing a horse gently down with a soft t will make his coat glossy. To rid the surface thoroughly of these impurities things are necessary: free perspiration must be induced, and the skin be well rubbed and kneaded. Trainers are well aware of this, and preparing a man for a fight, make him perspire freely, and then rub him *with a hard towel.* The vigorous system of cleansing adopted in a T *bath is more* like that used in training than any other, and will soon op

eyes of those who put their faith in cold sponging only, by bringing away an unexpected quantity of dirty skin. As there is no danger to be apprehended from the use of the bath, so long as the simple precaution is adopted of not staying in too long at first, and always taking the bath on an empty stomach, it can scarcely be overdone. I have known one taken three or four days together without any harm arising from it and should not anticipate any.

Some most extraordinary objections have been made to it. Mr. Hunt, for instance, expatiates with grim sarcasm on the portentous aspect of things on entering a Turkish bath—the air so hot as to make one fancy that it cannot be breathed without setting the lungs on fire, kneading and trampling on the patient, and the drowning with a deluge of cold water, but he admits that it cures fanciful people of their whims in superlative style. Then one of the whims it will cure them of is fancying there can any harm come from using the bath.

Whichever form of bath be adopted, I would strongly insist upon the necessity for taking it regularly and often enough, and not yielding to any nonsense about baths being lowering, weakening, &c. I dwell upon the subject because the advantage of hot bathing in prurigo generally so soon becomes manifest. If nothing else be gained, free perspiration is promoted, and though this is no panacea, yet it is attended with relief. Prurigo often breaks out in persons who have nothing to reproach themselves with in regard to cleanliness. Mr. Startin, indeed, says, "that the neuralgic itching which some writers consider as a form or variety of prurigo is, perhaps, more frequently met with in the respectable walks of life than any other cutaneous affection." Such persons often seem surprised at being told to make free use of hot baths, but they forget that they do not take sufficient exercise to keep the skin in a healthy state, and that means which would do very well with men riding twenty miles a-day, or working hard at training, are quite inefficient when exercise is reduced to a gentle stroll. Elderly persons in good circumstances, and people who have retired from business, often seem to think it is hardly respectable to go beyond a steady walk, but prurigo will not yield to such gentle means, and till regular active exercise has become a settled habit, the action of the skin must be encouraged. In all cases I think no woollen ought to be worn next the skin.

Mr. Wilson gives generous diet and tonics in this disease, and considers arsenic, properly given and watched, as a specific. Frictions, baths, carbolic acid soap, and juniper-tar soap, are his chief local remedies, but he looks upon the prognosis as doubtful, on account of the exhaustion and suffering which accompany the complaint.

Mr. Startin's treatment of prurigo is that of lichen; he relies chiefly on mineral acids, chalybeates, opium, and ammonia, and has never found benefit from mercury or arsenic; on the contrary, they rather do harm. In a very severe case of prurigo formicans, recorded by this gentleman, twenty drops of dilute sulphuric acid and ten of Batley's sedative three times a day, followed by tincture of muriate of iron, in infusion of quassia, and an opiate at bed-time, proved perfectly successful. These means were, however, seconded by the use of an ointment of white precipitate of mercury and creosote, ten grains of the salt, and a few drops of the fluid to an ounce, and after this a weak solution of bichloride of mercury in creosote water, used warm; later on, the ointment was entirely discontinued, and bisulphuret of mercury was added to the lotion. A strict diet, consisting of milk, bread, and boiled meat was observed. Mr. Startin finds hot air baths and cinnabar fumigations of great service. Dr. Neligan used to prescribe iron in infusion of hops, with the juice of conium in pretty large doses. He found in the prurigo of old people great benefit from the use of this remedy along with magnesia. Some years ago, Dr. John Waterfield communicated to the Lancet and Medical Gazette a paper on the value of tar and charcoal pills, and he now tells me that he has treated several cases of prurigo very successfully with this remedy. Dr. Hillier says that in some chronic cases diuretics, "such as the sweet spirits of nitre, decoction of broom, with the

ound tincture of juniper and saltpetre," are of benefit. Dr. Purdon communicated to the Journal of Cutaneous Medicine three cases in which the romide of ammonium in doses of from ten to twenty grains effected a very apid cure.

Hebra, who only admits prurigo mitis and formicans, views the malady in its gloomiest light; the picture is filled in with the most sombre tints that utter despair of being able to do any good can lend. The patient is doomed *tencro de ungue*. The relentless malady begins with childhood; even in his schoolboy days the martyr to it is an outcast; playfellows and masters alike shun and worry the victim of unappeasable itching. As he grows up, he is expelled from society or becomes a recluse; if he be a working man, he must not sleep in the same room with a fellow workman. He dare not try to establish a home, nor, should he already possess one, can he bring a wife to it. With advancing life the disease acquires more hold on the system, and only leaves him in the grave. Till that hour comes he knows no hope and no peace. The complaint is not difficult to cure, for the simple reason, that it is incurable, and all that the physician can do, is to make the patient's condition a little more tolerable. Hebra seems never to have given arsenic a fair trial, and it may be safely predicted that so long as he does not, he will find the complaint as refractory as he describes it to be. He utterly abjures bleeding, purgatives, and starvation, possibly with reason, but in point of fact I know nothing of the disease as spoken of by him, and never read of it in the pages of any other writer. Possibly in this severe, hopeless form it is as peculiar to Austria as pellagra to Italy.

In some cases, particularly when prurigo attacks the pudendum, scrotum, or anus, the itching is so intolerable that something must be done locally for it; indeed the itching is the symbol of the malady, the one essential and tangible symptom, and the cure of it is the cure of the disease. Mr. Wilson recommends, in a general way, that the skin should be frequently rubbed with a damp sponge, dipped in fine oatmeal; after this, the tincture of croton, made by steeping an ounce of bruised croton seeds for a week in four ounces of spirit, is applied, and after this has been done a few times, a lotion of bichloride of mercury in almond emulsion, fifteen or twenty grains to a pint, will often prove very efficacious. Painting the surface with iodine is useful, as is also glycerine applied with a sponge. For the affection of the pudendum, Bateman recommends a lotion made of two grains of bichloride (oxymuriate) of mercury in an ounce of lime-water. Mr. Wilson says the juniper-tar ointment is peculiarly valuable in this variety. In pruritus of these parts injections of very hot water, juniper-tar ointment, blisters to the thighs, and small bleedings, seem, from all accounts, to be the most reliable means of cure. Mr. Wilson has found an opium injection relieve the irritation after all other means had failed. Strong nitric oxide of mercury ointment, and podophyllin in doses of one-sixth of a grain, are said to have proved of great service in prurigo of the anus. Mr. Startin, in a case related in his lectures, where the scrotum was also affected, directed mucilaginous hip-baths, daily ablutions with yolk of egg and tepid water, the application twice a day of very dilute mercurial ointment, with a few minims of creosote, strict diet, and one sixth of a grain of bichloride of mercury in cold infusion of hops three times a day. The case was very severe, and occurred in an old man; but a cure was effected in little more than two months. In the prurigo of old people generally, Mr. Startin's prescription for external use is a liniment of glycerine and trisnitrate of bismuth or powdered talc rubbed in with a flesh brush. Dr. Frazer recommends for trial finely powdered camphor mixed with six or eight parts of rice or potato starch and a small quantity of acetate or carbonate of lead. This is dusted on the skin three or four times a day, its action being aided by calomel ointment. Latterly, Dr. Neligan confined himself almost entirely to chloroform ointment, which seems one of the best, if not the best ever introduced. It is made by mixing half a drachm of chloroform with an ounce of cold cream. *I believe this* and the following lotion are two of our most valuable rem

℞. *Hydr.* bichlor. gr. iv.; bismuth oxyd. ʒss.; acidi hydrocyan. di

Lond. 3ss.; aq. calcis ad. ℥viij. M. To be applied warm two or three times daily.

Mr. Balmanno Squire considers that prurigo senilis is always due to pediculi, and we may naturally expect to find that his principal reliance is upon external means. That in some persons pediculi will induce symptoms which might very easily be mistaken for prurigo is possible enough, but that they ever bring on genuine prurigo, a disease in its severer form so generally associated with some deep-seated constitutional disorder, is a very different matter. Mr. Naylor, who thinks the insect cannot produce any form of this complaint, says he has known a very weak nitric acid lotion—half a drachm to eight ounces of water or one of bismuth, a scruple to six ounces of water, —prove exceedingly serviceable. He also says that when prurigo is an idiopathic affection the use of chloroform is often of great service, applied either in the form of vapour or of an ointment consisting of equal parts of chloroform and camphor liniment.—*Medical Press and Circular, March* 11, 1868, *p.* 219.

82.—ON PRURIGO.

By Dr. Henry Purdon, Physician to the Belfast Dispensary for Diseases of the Skin.

The following three cases of prurigo have been selected for the purpose of showing the value of the bromide of ammonium in this distressing complaint.

1.—Mrs. M., aged 60, admitted at the Belfast Dispensary for Skin Diseases, in February, suffering from prurigo, and which has existed for upwards of six years, is always worse at night. Formerly was in better circumstances, and attributes the disease to mental anxiety and bad food. At present (February) her arms and chest are excoriated, presenting all the typical appearances of prurigo; the cuticle is also discoloured.

Various remedies were tried in this case, both locally and constitutionally, such as lotions containing hydrocyanic acid, borax, and morphia, the latter being administered by hypodermic injection, and from the use of which most benefit was derived, but no permanent results were obtained. At the end of February I determined to try the effect of the bromide of ammonium, commenced in doses of ten grains, three times a day, gradually increased to twenty; and discontinued all local treatment. After taking the bromide for about a fortnight the feeling of formication disappeared, the patient obtaining sleep at night. The diet consisted principally of farinaceous food and milk. At the present time (July) she is perfectly well.

2.—J. M., aged 64, formerly a butler, admitted at the Dispensary for Skin Diseases, May 1st, suffering from prurigo, which has existed for about two years; was formerly much given to drinking whisky, to which he attributes the present disease; appetite fair; occasionally sleeps well at night; bowels usually costive. In this case the treatment was commenced by administering a pill containing podophyllin, pil. colocynth co. and ext. of Indian hemp, every second night for about a week, so as to get the liver and bowels to act naturally, after which the bromide was commenced, continued steadily till June 29th, when he was discharged cured.

3.—H. S., aged 59, admitted into the infirmary of the Belfast Charitable Society, under my care, on February 4th, suffering under a severe attack of prurigo; has always been temperate, but is greatly debilitated; no appetite, and his mind wanders occasionally. An extensive eczematous eruption exists on his chest, arms, and back, arising from the patient tearing and scratching himself to relieve the excessive itching. In this case quinine was prescribed, and a lotion of sulphate of zinc, glycerine, and water, as an external application, with twenty drops of liquor morphiæ hydrochloratis at night to procure sleep. In the month of March, as no benefit was derived from the above treatment, and from the success attending the administration of the bromide of ammonium in case 1, I determined to commence its use, its effects being

apparent in a few days, the patient expressing himself much better. The only local treatment adopted was the application of calomel ointment (a drachm to the ounce of lard), under the use of which the eczema disappeared, and, as the itching was greatly moderated, he did not feel the same inclination to tear himself. By the end of April the patient was quite well of the prurigo, but several carbuncles made their appearance on his neck, some of which quickly suppurated. The treatment now adopted was, on the first appearance of the carbuncles, pressure, and when it was evident that suppuration must take place, a poultice for a few days, after which they were opened, a yeast poultice being then applied: at the same time a generous diet and wine was ordered.

The patient's constitution being evidently quickly breaking up, and as he made very little urine, and that high-coloured, I prescribed a stimulating diuretic, which treatment I believe is recommended by Dr. Day for prurigo in his work on "Diseases of Advanced Life," but from which no benefit was derived. Considering his present complaint to be due to a blood poison, I ordered the bisulphite of soda in infusion of cinchona; this certainly prevented the appearance of any new carbuncles; but a large anthrax having already formed on his back, from this he rapidly sank.

The above case is, I think, interesting, but I shall leave it to those whose experience is more extensive than mine to show the connection between the two diseases. I shall only mention that I have frequently seen an eruption of furunculi make their appearance after prurigo has been relieved. Could prurigo depend on a blood poison? for this disease is often associated with jaundice, gout, rheumatism, and Bright's disease.

Since the above cases presented themselves I have treated several others with bromide of ammonium with marked success. The only local application that I use is borax and glycerine, and that merely as a *placebo*. The bromide, when first administered, quickens the pulse, and when given in large doses is a powerful anodyne, occasioning sleep and increasing the secretion of urine.

According to Dr. A. T. Thompson, prurigo frequently arises from the abuse of fermented liquors, as also indigestible kinds of food, and is, I believe, now looked on as a neuralgia of the papillæ of the skin. Many dermatologists consider it to be evoked by heat, and when constitutionally recurrent is a form of eczema, to which the name eczema papulatum has been given. Dr. McCall Anderson considers prurigo to be a form of lichen, and "consequently an eczema, the papules being identical with those of lichen, the black crusts being produced by scratching." Pruritus of the anus frequently arises from engorgement of the liver and ascarides in the rectum.

Cazenave considered prurigo to be a disease of sensibility, depending on the retention of some of the constituents of the urine in the blood. Pediculi often occasion the disease in old people, from the irritation of which the patients tear and scratch themselves, a papular eruption being produced.—*Journal of Cutaneous Medicine*, *Oct.* 1867, *p.* 326.

83.—LARGE CYSTIC BRONCHOCELE CURED BY IODINE INJECTIONS.

By T. B. Curling, Esq., F. R. S., Surgeon to the London Hospital.

Emily G., aged 54, a widow, was admitted into the hospital on February 20th, her home being at Long Milford, in Suffolk. About fifteen years previously she noticed a swelling towards the left side of the thyroid cartilage. It grew very slowly; but about five years before admission its rate of growth increased very much; and when first seen on February 20th, the tumour, which was smooth, and fluctuating towards its anterior and lower part, occupied the lower two-thirds of the anterior triangle of nearly all the posterior, causing the circumference of the neck opposite the most prominent part of the tumour to measure 19 inches. Towards the outer and posterior part an

artery, apparently equal in size to the external carotid, being quite superficial, could be seen and felt running upwards towards the angle of the jaw; numerous small veins were also visible. The swelling ascended when the patient swallowed, showing its connexion with the larynx.

On Feb. 25th, the tumour was tapped. and seventeen ounces of reddish serous fluid were drawn off, reducing the size to 16 inches. No constitutional disturbance followed, but by March 16th the size had increased to 18 inches. It was then again tapped (about 14 ounces of dark bloody serum being removed), and injected with about one drachm and a half of compound tincture of iodine (Ph.) and water, mixed in equal proportions, which merely caused a smarting for about an hour.

The following day the tumour had returned nearly to the size it had before the tapping and injection.

On Friday, March 26th, the tumour, having again increased to 18 inches, was tapped, but only a small quantity of fluid drawn off. It was injected with the compound tincture of iodine undiluted, about a drachm and a half being thrown into the sac, and left there. No pain or uneasiness was felt in it until the evening of the following Monday, when she had pain in the head, giddiness, and much fever; and on the following day the tumour became hot and painful, and measured 19 inches.

In a few days the constitutional disturbance subsided; the tumour being first treated with a poultice, and subsequently ice was applied. Although the general symptoms became less, there was considerable dysphagia, and the tumour increased in size, and on April 12th measured 20 inches, and was very red at its anterior and lower part. She could then swallow only fluids.

April 14th. Tumour (now measuring 20½ in.) was more painful, very hot, and the skin œdematous at the posterior part, which now fluctuates distinctly; no appetite nor sleep; pulse 120. It was now determined to let out the fluid. The sac was first punctured with a trocar, and then opened freely with a scalpel, letting out about twenty ounces of fluid, which afforded immediate relief. The fluid consisted of an opaque liquor, containing altered blood-discs and compound granule-cells. A drainage-tube was then introduced, and in a few days the discharge became purulent, thick, of a greenish colour and offensive odour.

30th. The tube was still kept in, and the discharge was copious. Protruding through the wound there was seen an apparently ordinary slough, but upon withdrawal it was found to be a firm membrane folded on itself—being, in fact, the wall of the cyst. After this the discharge continued, though somewhat lessened in quantity and more healthy in character. The general health was very good. The circumference of the neck was reduced to 15½ in.

June 3rd. Still discharging from the original opening, a little to the right of which there was a prominent fluctuating swelling, which was opened and several ounces of pus let out. During the next four weeks two other small collections of pus became prominent, and were opened, discharging pus.

July 22nd. The general health was now quite restored; there was no dysphagia, and the appetite was good. The circumference of the neck was now only 14½ in. There were three or four small openings in the anterior part of the remainder of the tumour, which was firm and painless. These openings discharged a little thin fluid, and were occasionally touched with nitrate of silver.

She left the hospital on the 30th, the tumour decreasing in size, and being so much diminished as to occasion very little deformity. Two of the sinuses were closed, but one continued to discharge a little. There was scarcely any swelling in the neck, and the patient was discharged cured.

The morbid changes in the thyroid gland producing the swelling called bronchocele are of two kinds; hypertrophy of the natural structure, the common and well-known form of the disease; and the development of a cyst, the case related being a marked example of the latter. Cystic bronchocele is a much rarer form of the disease than hypertrophy of the gland. It occurs at all ages after puberty. The cystic formation is generally single, and it differs further from hypertrophy in being limited almost invariably to

one lobe of the gland. When the cyst acquires a large size, the glandular structure becomes obliterated by pressure. The seat of the disease, however large the cyst may be, is rendered obvious by its connexion with the larynx. When the patient swallows, the tumour moves up with the windpipe; and in this way cystic bronchocele is easily distinguished from the cysts formed in the areolar tissue of the neck, called "hydrocele of the neck." A large cystic bronchocele is a serious disease, not only on account of its producing an ugly tumour and great deformity in a conspicuous part of the body, but also from its liability to press on the trachea and œsophagus and to obstruct these passages. Gooch, an old surgical writer, whose works are now seldom read, but who has recorded many interesting and remarkable cases, mentions one case of cystic bronchocele in which the trachea was compressed and contracted by the cyst. Some similar cases in which deglutition and respiration were impeded by a cyst in the thyroid gland, will be found in a paper in the *Archives Générales* for 1837. A cystic tumour in the thyroid gland, however, does not always grow to a large size. Whilst still small, it may remain stationary and cease to increase. Some years ago I removed from the dead body a small cyst in the right lobe of the thyroid evidently of very old standing, for it is encased in bony deposit, forming a complete osseous sac. The specimen is in the London Hospital museum. When the cyst remains small, no surgical interference may be called for; but in the case of Emily G. the tumour had attained a great size and was steadily enlarging, so that the patient became very anxious for something to be done in order to get rid of it.

The cure of cystic bronchocele may be effected in three ways—by excision, seton, and injection. Dr. Beck, of Fribourg, who published some cases of cystic goitre in 1836, treated three of these by excision. Two were cured, one after severe inflammatory symptoms; the third was doing well, when low fever ensued, and the patient died of pyæmia. The circumstance that an artery of great size passed over the outer part of the cyst precluded my thinking of excision in G——'s case. Besides, the disfiguring long cicatrix remaining after excision constitutes an objection to this operation. The seton is an effectual mode of treating this disease. Maunoir, of Geneva, in a memoir published in 1825, gave an account of four cases of large thyroid cysts which were cured by seton; and Mr. Spencer Watson lately showed to the Pathological Society a woman with a large cystic bronchocele, which interfered with respiration and deglutition, and which he was treating successfully in this way. Injections of iodine act so well in the cure of serous cysts that I was induced to try them in G——'s case, especially as the sac was by no means thick. The second injection caused active inflammation, followed by suppuration and separation of the cyst. The result, however, was satisfactory in effecting a cure of the disease. The rapid increase of size after inflammation had set in interfered seriously with deglutition, so that the patient was unable to swallow any solid food. This was quickly relieved by a free opening into the cyst, made by the house-surgeon, Mr. James Adams. Suppuration in a cyst of so large a size, and near so many important structures, is not altogether free from danger. There is a preparation in the museum of St. Bartholomew's Hospital of a large cyst of the thyroid gland, which inflamed and enlarged rapidly after tapping, and the patient died rather suddenly, suffocated. The cyst had burst, and discharged its contents into the pharynx and larynx. The same thing happened in Gooch's case. The patient expired suddenly in consequence of the cyst inflaming after being incised, and opening by ulceration into the trachea. Injections of iodine are certainly not so well adapted as the seton to the cure of cases where the cyst is much thickened. There is another advantage in the seton over injections—that it is free from the risk attending the rapid enlargement of the cyst and the confinement of matter, which proved fatal in the cases just alluded to, as there is always an outlet for the escape of fluids. So that, upon the whole, I regard the seton as the more valuable plan of treatment, and the one most generally applicable to these cases.—*Lancet*, Dec. 14, 1867, p. 729.

84.—EXTENSIVE ERUPTION OF WARTS: RELATIONSHIP BETWEEN WARTS AND CANCER.

A very extraordinary example of the free production of common warts has been under treatment lately, at the Hospital for skin diseases. The subject is a young clerk of 19, who was sent to Mr. Hutchinson by Mr. Garman, of Bow. His face was covered with warts, and very greatly disfigured. They grew in all parts, but especially on the forehead. They were of the ordinary foliated form, some flat, others slightly pediculated. On both hands are also very numerous warts. On the right hand they completely surround the nails and lift their edges, though without pain or inflammation, and without disturbing the nutrition of the nails themselves. Mr. Hutchinson remarked on the rarity of any large production of warts in the face, and stated that he had never seen so many as in this instance. He also directed attention to an interesting fact in the patient's history; that his mother had died of "cancer of the womb." He stated that several facts which had come under his observation confirmed the opinion of those (Mr. Nunn and others) who believe that there is some connexion between the liability to warts and the liability to epithelial cancer. The youngest patient upon whom he had ever operated for epithelial cancer, was a man, aged 22, with cancer of the penis, and whose hands were covered with large warts of most unusual exuberance.

In this case, the warts have been treated by touching them with the acid nitrate of mercury, and most of those in the face are now cured. A portrait of the lad, showing his original condition, has been kept.—*British Medical Journal, Dec.* 14, 1867, *p.* 549.

SYPHILITIC AFFECTIONS.

85.—A CONTRIBUTION TO THE HISTORY OF VISCERAL SYPHILIS.

By Dr. W. Moxon, Assistant Physician to Guy's Hospital.

The value of the study of morbid anatomy has never had a stronger proof than it is receiving from the service it has rendered, and is rendering, in showing that syphilis causes grave and fatal visceral diseases, which are easily recognised.

When John Hunter wrote—"The brain, heart, stomach, liver, and kidneys, and several other viscera, have never been known to be affected by syphilis," he expressed the truth, as far as it is seen in common experience, of what I may call the *surgical stage* of syphilis; but now-a-days a *medical stage* of syphilis has to be recognised, which had not then drawn special attention. The labours of Dittrich, who showed the morbid anatomy of syphilitic lesions, deserve to rank with those of Bright, and this not less for the bold, comprehensive, and correct conclusion which either observer drew from his observations than for the frequency and gravity of the disease concerned in either case; while, in one respect, the demonstration of these syphilitic lesions surpasses in value even Bright's great discovery, inasmuch as a more definite and hopeful plan of treatment is indicated by the discovery of syphilis than by the discovery of Bright's disease of the kidney.

Though the further labours of Wilks, Bristowe, and other observers have done for Dittrich's discovery more than Rayer and Christison did for Bright's, even now I am persuaded that the frequency of syphilitic causation of medical disease is not usually so familiar and ready to the mind as it should be in order to the early detection of it under the very various forms in which it comes clinically before the physician.

Until the doctrine which ascribes to syphilis the lesions in question is universally and fully allowed and put into practice I conceive that those who have considerable means of testing the truth of the view lie under obli-

gation to state publicly their experience. For, as is usual, and as is right, in the rise of new doctrines, there are those who oppose a sceptical face to the new opinions and deny the syphilitic nature of the affection. Such natural and useful doubts expressed by observers of distinction are very powerful to hinder the spread of the recognition of visceral syphilis among those practitioners who have little or no opportunity of personally examining whether anything characteristic of syphilis is in the viscera of cases which have been treated unsuccessfully,—perhaps, because the suspicion of syphilis as the cause of the trouble was never aroused.

The full and universal acknowledgment of the syphilitic nature of these morbid products will not be reached untill observers who have large opportunities give their experience equitably, stating all their observations which bear on the question, whether those observations favour the doctrine or any other. One-sided statements will be received with suspicion, for a disease so frequent as syphilis is sure to be very often by chance found associated with other diseases. That a diseased state may be fully recognised as directly syphilitic it must be shown that it occurs in syphilis, but it must also be shown that it does not occur except in syphilis. There must be doubt of the syphilitic nature of lesions which are believed often to occur apart from a history, or from the ordinary manifestations, of syphilis; and, to put the question into a state of certainty, it is necessary that *all* the cases in which that lesion occurs should be fairly examined. Each lesion, too, must be separately so examined, or well-founded doubts concerning one may weaken otherwise good evidence in favour of another which is grouped with it.

For example, the conditions of liver which are set down as syphilitic are:—1. Simple perihepatitis; 2, simple interstitial hepatitis; 3, gummous hepatitis; and 4, the amyloid or lardaceous change. The description of all these as alike syphilitic produces a very inequitable judgment of their relative significancy; any one will doubt whether cases of thickened hepatic capsule, or of simple induration of liver by fibrous formation, can be regarded as syphilitic, since we all meet these conditions when there is no suspicion of syphilis. But it is quite otherwise with gummous hepatitis. This is practically never found without the presence of other evidences of syphilis. So that it is necessary to discriminate between these four lesions in the gathering of evidence, and to take each on its own merits. We shall, I believe, conclude with more and more certainty that the gummous form is essentially syphilitic; but as for the capsulitis and fibrous hepatitis, the occurrence of these conditions from spirit drinking, &c., must impede our arrival at proved conclusions respecting their essential connexion with syphilis. A careful inspection of the forms under which capsulitis and fibrous hepatitis appear in syphilitic bodies and an examination of any apparently distinguishing characters by a fair comparison with these lesions as they appear in unsyphilitic bodies are very desirable, and need to be done with petition, ...refully. I believe that it will be found that whenever capsulitis with quickly ...atitis are strictly localised to parts of the liver there will be found Mr. James Ada... of the presence of syphilis.

many important s... allowable that I should contribute such experience on preparation in the museum hand in Guy's post-mortem theatre during the last the thyroid gland, which in though not all unequivocal, will, I believe, tend the patient died rather sudden... syphilis is among the most important of the charged its contents into the cognise.

pened in Gooch's case. The ... anything new respecting syphilis, but to the cyst inflaming after being ... of the conditions found in bodies proved to trachea. Injections of iodine have allowedly syphilitic changes present in to the cure of cases where they ... it will be probable that a certain form of advantage in the seton over ... tain forms of disease of the lung, and of the ing the rapid enlargement of ... ly syphilitic. But even if no definite conclu- proved fatal in the cases just ... these cases, I would narrate them, that they escape of fluids. So that, ... ups of syphilitic cases, for I believe that useful valuable plan of treatment, ... rally after many collections of cases are on cases.—*Lancet, Dec.* 14, 186... sions may be quite unsupported—perhaps

rcely indicated—by any one set of cases. By enlarging the field of ervation we may fairly hope to learn other effects of syphilis beyond se we already know.

Every lesion which so becomes recognised as syphilitic forms a new and uable means of recognising other syphilitic changes. These will be ily discovered in proportion to the constancy of their association with wn effects of the disease, and when their taint is recognised they will re- t on their associates and strengthen the whole evidence in doubtful cases. For it must be allowed that there are sure to be cases truly syphilitic ere no account of the syphilis can be got from the history; circumstances en render it imprudent or improper to ask directly after such a disease, and truth of replies cannot be relied on when the question is put. And, ther, even if we could always learn all that the patient knows about his n case, we should find much difficulty from the confusion of the non-in- ting sore with the truly syphilitic chancre, so that after the primary sease has long passed by we may be quite unable to learn whether a scarce membered disease of the genitals was really syphilis.

This dubious quality of the direct history makes it very highly desirable at all the effects of medical syphilis should be well known, so that any one them that presents itself may be identified by the aid of others which are ouped with it in the particular case. For whatever theoretic views any of may hold as to the constitutional nature of syphilis, no one would be con- nt to discover only local lesions in a case where tertiary syphilis forms art of the patient's condition; every one will see that syphilitic cachexia is thing which it is of the first importance to recognise.

Cases of a disorder so diversified in its numerous forms—there is no disease ry unlike some form of syphilis—are difficult of arrangement. The fore- ost aspect of pathological anatomy is that which bears on clinical practice, d hence I think it will be preferable that the cases should be grouped in spect to the organ for whose disorder they came under treatment.

[These remarks of Dr. Moxon are followed by the post-mortem examination 27 cases in support of his views, which we advise the reader to study at- tively. We have only room to give one case from each of the classes ntioned by Dr. Moxon.]

I. Case in which the Nervous System was chiefly diseased.

Case 1.—*Syphiloma of brain with softening; syphilitic disease of mid. ebral artery; syphiloma of testes.*—Joseph D., aged 29, admitted Septem- 29th, 1865, into the clinical ward under Dr. Moxon's care. He was fering from hemiplegia, with imperfection of mind.

He gave some account of venereal disease, but the state of his mind was imperfect that little could be made of it.

He had been ill about three months. The attack was gradual; hemiplegia the left arm commenced after a convulsion, and though varying in amount, progressed. Meanwhile he had had two other fits; in none of these convulsive fits had he quite lost consciousness; his mind had grown gradu- weak, he did simple things, and laughed or cried on slight occasions.

He was a man of middle height, with square features and red-brown hair; complained bitterly of headache, now frontal, now occipital; his face re an expression of suffering; his left arm was quite useless; he was re- dless, and he passed his evacuations under him.

While in the hospital he had attacks of vomiting, and suffered much from adache; his bowels were natural; the temperature in the axilla was never er 98 degrees; he was noisy and dirty. He fed himself with a spoon with right hand to within a few days of his death, but then that hand also be- me very feeble. He died rather suddenly as if choked.

On inspection the body was considerably wasted; the left foot dropped at e ankle; there was no bedsore; the pupils of the eyes were equal.

Head.—On raising the dura mater the hinder part of the right brain hung way down like a bag of liquid, and the removal was difficult from fear of reaking this. In the diamond-shaped space the membranes were opaque,

and the basilar artery had lymph around it; also the membranes in the Sylvian fissure of the right side were rather thick, and the middle cerebal artery had a thick yellow patch on its wall, whilst the corresponding artery on the opposite side had none. On cutting off the vertex of the right brain in the usual way, a quantity of white liquid like milk came away, carrying with it some, and leaving more, of fine ragged shreds of white brain tissue, all soon becoming pink from admixture with a little blood. Curiously different was the state of the grey matter over the soft part; this was almost cheesy in consistency, and considerably yellow and brownish in patches; the softening extended nearly across the hinder half of the corpus callosum. On opening the ventricles the liquid therein was somewhat turbid, the lining not granular. There was no discovering the boundary of the ventricle at its hinder part, but there must have been one from the difference in the liquid in the soft brain from that in the ventricles.

In the left hemisphere were small soft patches, exactly similar in quality to the affection of the right; also in the corpus striatum were two deep hollows with yellow on their walls. The lining continued over them perfect, but sunk in a little; thin threads (vessels) crossed the cavities; the cerebellum appeared to be normal, also the optic thalami, the pons, and the medulla spinalis.

The microscope showed in the soft brain-substance granular cells in large numbers, and crowded numbers of small fatty cells in the yellowish firm cortical grey matter. I could not see any blood pigment in this. Many glistening bodies like in form to amyloid corpuscles were seen, but iodine made no change in them (colloid corpuscles).

Thorax.—The left *pleura* was slightly adherent behind. In the principal bronchus of the lower lobe of the right lung was a piece of apple impacted at the bifurcation. There was no tubercle of the *lung*. The lungs were flaccid, yet full of blood. The lower lobes contained little air, especially the lower right lobes, whose flotation was less considerable than that of other parts of the lung. The œsophageal epithelium was stripping.

The *heart* was very flaccid; the endocardium and valves much stained; no trace of vegetation nor ante-mortem coagula. The right heart contained a moderate amount of black clot. The left heart held a little liquid blood.

Abdomen.—The *alimentary canal* was normal.

The *liver* weighed 47 oz.; its substance was very flaccid from decomposition; no marks on the surface, and no syphilitic patches.

The *spleen* was flaccid; it weighed 4 oz.

The *kidneys* weighed 8½ oz.; and appeared perfectly healthy.

The right *testicle* had a hard, almost gristly, opaque yellow mass embedded in its substance, and surrounded by fibrous formation, structurally continuous therewith; also a smaller hard mass in the epididymis. The left testicle was free from any nodules of the kind.

Had the new formation in the brain been the only one present, the characters of it were sufficiently pronounced to have led to the conclusion that it was of syphilitic origin. But a most significant feature of syphilitic formation is its appearance in different viscera of the same body. The same yellow cheesy matter is produced in the brain and testis—two organs as remote from each other in position and function as any two organs of the same body can be. Some common cause exists for the two identical formations. That this cause is not tubercular is shown in the absence of tubercle from its usual seats, especially from the lungs. On the other hand, the characters and position of the new matter correspond with those of ordinary syphilitic matter, and the history of venereal disease, though it could not be traced in detail, gives some support to the conclusion drawn from the characters of the disease. In passing I may notice the absence of any increase of the temperature of the body, when so extensive a softening of the brain was present. This helps to show that white softening of the brain is not inflammatory. The thick yellowish node on the artery of the syphilitic part of the brain is of interest, and compares with that found in the case recorded by Dr. Hughlings Jackson ("Medical Times and Gazette," 1866). This state of vessel,

ther with the extensive syphilitic induration of the pia mater and grey ter, through which the white matter receives its vessels, will sufficiently ount for the white softening of the brain.

. CASE IN WHICH THE CHIEF DISEASE WAS OF THE RESPIRATORY ORGANS.

ase 8.—*Syphilitic disease of the throat (larynx), spleen, and liver; acute ma of glottis.*—Amelia F., aged 20, admitted under Dr. Gull, Clinical rd, 24th March, 1866; died 24th March, 1866.

. F., had been attending for some weeks, as an out-patient, for syphilitic ngitis, under Dr. Pavy, who, seeing her danger, sent her into Clinical rd. She died on the same evening, in a sudden exacerbation of her pnœa, before assistance could reach her.

n inspection the body was well nourished. There was a scar just above clavicle, in the lower triangle of the neck. No discolouration of sur-

lead.—Venous blood in great quantities in *brain* and membranes, but the tomical state of the organ natural.

here was extreme œdema of the arytæna of the larynx; lower down the ous and submucous tissues were swollen and hard, and on the left side, ve the vocal cord, was the opening of an abscess, whose contents were i-consistent, and were graduated into the thickened tissue around. This cess rested on the arytænoid and cricoid cartilages, and the yellow semi-sistent matter extended under the perichondrium for a certain distance. e condition was that of gummous perichondritis.

ome of the cervical glands under the sternomastoid were enlarged, and ost pellucid.

horax.—In the upper lobe of the left *lung* a sub-pleural effusion of ery blood, about the size of a walnut, extended into the tissue, being tinuous with lobular congestions in the neighbourhood. The lung show-extreme congestion of the centres of the lobules.

he *heart* was small and firmly contracted.

bdomen.—The *intestines* were healthy.

he *liver* weighed 48 oz. It was healthy as to substance generally. There s inright lobe a yellow nodule of size of a horse bean, and also a depressed ch one inch long, which was fibrous on its outer part, with an opaque low patch in it. This patch could be seen to contain wasting tissue rged with some fibre-growth.

he *spleen* showed a condition pronounced by me at first to be "phthisis spleen," but with some misgiving. The sulphur-yellow masses, of the of peas, softening at their centres, were utterly unlike anything I have r seen in the spleen in cases of phthisis or tuberculosis; these were pretty form in size, and scattered plentifully deep in the organ. On seeing the hilitic patch on the liver I had no doubt of their true character as syphi-deposit in the spleen.

he *kidneys* weighed 7½ oz., and were quite healthy looking.

yphilitic gumma of the spleen is not common. The formations present he spleen in this case were markedly different from any other products t we are familiar with. They were almost all deep-seated, and differed s from the "embolic" masses which we find in endocarditis. They were s white and less firm than the nodules present in "Hodgkin's disease," l, further, were not associated with enlargement of the lymphatic glands in that disease. The lungs were entirely free from tubercles, and from s alone I should doubt exceedingly that these masses were tubercular. I e never seen anything approaching such an appearance in any of the erculous cases that I have examined. I have no doubt that this is an ex-ple of syphilitic gumma of the spleen.

III. CASE IN WHICH DIARRHŒA WAS THE PRINCIPAL SYMPTOM.

Case 16.—*Constitutional syphilis; waxy stomach and intestines, &c., &c.*—

J. B., aged 24, formerly a soldier, and lately a labourer, was admitted into Stephen Ward, on July 7th, under Dr. Habershon.

He had syphilitic buboes six years ago, but had no chancre or discharge. He was received into Job Ward, under Mr. Poland, for fistula in ano, and sores on scrotum. He has had diarrhœa and vomiting since admission, and has passed blood to the amount of half a pint in his stools for two or three days; blood comes also from the mucous membrane of his mouth, and when he is asleep it collects in considerable quantity and coagulates. He has had pain in the long bones since admission, and also soreness of the tongue.

Present condition.—He has a large number of copper-coloured spots on forehead and cheeks, scaly circular spots of copper and rose colour on back, and very slightly on arms; and on the legs has had several larger sores, which have just ceased to discharge. He still vomits much of what he takes, though less than he did; there is no bile in the vomit. The tongue is furred and sore. Heart and lung-sounds normal. Pulse 84. Urine pale straw colour, albuminous. Sp. gr. 1023.

During his stay in the hospital he suffered from uncontrollable diarrhœa and vomiting, and grew very much emaciated before his death

The post-mortem examination was made by Dr. Fagge. The *membranes* of the brain were opaque, and the *brain* itself anæmic. The *lungs* contained no ordinary tubercle; at one apex, besides a cavity there, a few masses which, when cut across, felt almost gritty. The *heart* was healthy. The mucous membrane of the stomach was pale, and markedly lardaceous. The *intestines* lardaceous in most marked degree. The *liver* was lardaceous, and apparently also fatty. The *spleen* lardaceous. The kidneys were small, weighing only seven and a half ounces together; they were most markedly lardaceous, iodine developing the characteristic dark colour in the pyramids as well as in the cortex.—*Guy's Hospital Reports, Vol. XIII*, 1868, *p.* 329.

86.—ON BUBO.

From a report in the LANCET on the practice at Guy's Hospital.

The following is an outline of the methods of treatment adopted by Mr. Durham in the ordinary forms of bubo.

1st. In the case of the numerous, slightly enlarged, hard painless glands usually associated with true syphilitic sores, no local treatment is considered necessary. The glands participate more or less directly in the general treatment. This essentially consists, in the majority of cases, in regular mercurial inunctions about the inguinal regions.

2nd. In the case of the less numerous (or single), more enlarged, painful and tender, but still somewhat hard, glands, associated with simple or suppurating venereal sores, or with syphilitic sores which have become inflamed, or sometimes with gonorrhœa, *rest*, as thorough as possible, is enjoined. At the same time, all practical measures are taken to remove the irritation and allay the inflammation of the parts primarily affected. When it can be borne, continuous pressure is maintained over the enlarged glands by means of a large pad of cotton-wool or lint, and a spica bandage. In some cases the iodide of potassium ointment, or the iodine ointment, is freely smeared over the parts beneath the pad, and from time to time gently rubbed in. In other cases (especially in those in which pressure cannot be borne, and in which the patient is obliged, or is able, to lie up), the skin over the glands is repeatedly painted with the strong tincture of iodine, or with some blistering fluid. The former line of treatment is adopted in cases in which it is desired to act upon the glands by means of medicaments absorbed; the latter in cases in which counter-irritation seems likely to be useful, and in which it can be carried out without hurt or undue inconvenience to the patient. In many cases in which there has been much tenderness, a combination of *extract of belladonna* with iodide of potassium has been applied with apparent *benefit*. Every endeavour is made in every case to prevent suppuration al-

together, or at any rate to put it off as long as possible, and to confine it within the narrowest limits. If, however, suppuration seem inevitable, warm fomentations and poultices are recommended.

3rd. In the case in which the skin is painful, red, and thinning, and in which it is manifest that suppuration has taken place in or about the glands, the buboes are at once opened by the knife in such a manner as to give the freest and quickest exit to the pus, and poultices are ordered to be applied for a few days. The direction and extent of the incision made vary with the shape, size, and condition of the abscess. As a rule, the incision corresponds in direction with the longest diameter of the abscess; and the length of it is determined by the extent to which the thinned and damaged skin will probably be left "undermined" after the bulk of the matter has been discharged. A well-judged free incision in the first instance often prevents the necessity of subsequently slitting up troublesome sinuses, and of dividing portions of undermined skin which will not adhere to the subjacent structures. In cases in which the abscess is roundish in outline, the vertical incision is preferred. In the majority of instances, the abscesses are elongated in the direction of the inguinal fold; in such cases the incision is made about parallel with Poupart's ligament.

In cases in which there is but a small, soft, fluctuating spot surrounded by more or less defined hardened structures, and in which the skin is but slightly reddened, or not at all, and in which there are few signs of acute inflammation, a small puncture into the abscess is considered sufficient.

In some cases the abscess appears to be in the structures surrounding the glands, rather than in the glands themselves. In such cases, after the matter has been evacuated, the glands may be seen standing out, as it were more or less enlarged and hard, and overlapped to some extent by the undermined edges of the incision. The application of strips of lint smeared with the red oxide of mercury ointment to the glands is the treatment ordinarily adopted when the case has assumed a chronic aspect.

In cases in which open buboes take on a chancrous aspect they are treated on the same principles as the primary sores to which they correspond. Some are stimulated, as by nitric-acid lotion or black lotion; some are soothed by the application of preparation of opium or anodyne poultices. Occasionally, but rarely, destruction of the surface by strong nitric acid is necessary. In many instances the iron lotion (ferri potassio tartras, from ten to twenty grains in one ounce of water) is found useful, or a mixture of the iron lotion with some opiate.

In all cases the most thorough rest compatible with the patient's circumstances is insisted upon; and in cases in which general rest is impossible, local rest is insured as far as practicable by means of pad and bandage. In all cases, also, the general health of the patient is carefully attended to, and any constitutional symptoms are treated from time to time as they may arise.—*Lancet*, *Nov.* 23, 1867, *p.* 641.

37.—TREATMENT OF SYPHILIS BY LARGE DOSES OF IODIDE OF POTASSIUM.

By Sir Henry Thompson, London.

For several years past, at least eight or ten, I have been in the habit of pointing out, at University College Hospital, to students and others, the value of large doses of iodide of potassium in certain forms or stages of syphilis. Commencing with twenty grains daily, I have frequently increased it to seventy-five. This fact has been, as is well known there, a favourite clinical topic with me, the curative effects of the drug having been often extremely remarkable. At the present moment a man in my ward is taking sixty grains daily; he has had seventy-five, and has continued that quantity for many days together.

After a very large experience of the remedy, I have arrived at the following conclusions:—

1st. That iodide of potassium in large doses (i. e., from thirty to seventy-ve grains daily) is almost a specific for the cure of large spreading tertiary, r late secondary, ulcerations of the skin, such as those so common, and generally so intractable, affecting all parts of the body, and which often follow an eruption of rupia.

2nd. For those late syphilitic eruptions which affect the nose so disastrously, and often so rapidly, these same doses are equally valuable. I am in the habit of saying that "no man ought to lose his nose from syphilis if he can take these doses."

3rd. Iodism is exceedingly rare in presence of tertiary syphilis; and large doses do not, as a rule, occasion loss of flesh. If they cure the syphilis, the patient usually gains weight under their influence.

I have the notes of numerous cases of large tertiary sores rapidly healed by this agent, and have often intended to bring the subject formally before the profession. But having given it constant publicity for so long a period at the hospital, I have thought this might suffice, and I am very glad that Dr. Pollock has done so. I am compelled to add that I consider iodide of potassium as undesirable in primary syphilis and in early secondary symptoms, as the iodide of mercury in *small* doses is valuable at that period of the malady. —*Lancet, Dec.* 28, 1867, *p.* 814.

88.—IODIDE OF POTASSIUM IN LARGE DOSES.

By Dr. R. S. Sisson, Malda-Hill.

During the years 1858-59, whilst a student at the Edinburgh University I performed a series of experiments in order to test the effects of different drugs upon myself. In the Lancet of April 14th, 1860, you did me the honour of publishing the result of my observations on the iodide of potassium. To that report I beg to refer Dr. Julius Pollock for further details. Suffice it here to state that, with gradually increasing doses, I was enabled to take *one ounce per diem* of that drug with no other effects than slight catarrh and symptoms. I at that time thought that, in the treatment of syphilis, chronic rheumatism, &c., all the good effects of the drug might be obtained by small doses; but further experience has convinced me that large doses—twenty or even thirty grains—are not only well borne, but are frequently required in order to effect a cure.—*Lancet, Jan.* 18, 1868, *p.* 105.

89.—ON THE USE OF IODIDE OF POTASSIUM COMBINED WITH QUININE AND IRON IN THE TREATMENT OF SYPHILIS.

By Dr. Peter Eade, Norwich.

Like most other practitioners, I also have continually found the iodide, when given in small or moderate doses, fail to arrest the ravages of ulcerative secondary or of tertiary syphilis, but I have for some time past been aware of the singular increase to the efficacy of this drug which is produced by combining with it a salt of iron, or, what I find to be better still, a double salt of iron and quinine. In this way I find that a dose not exceeding eight or ten grains of the iodide of potassium, with the same quantity of the citrate of iron and quinine, rarely fails to stay the progress of the disease, and has repeatedly, in my hands, been at once and progressively effective, when the same dose of the iodide by itself has exerted little or no power over its progress. Indeed, so frequently has this been the case, that my rule now is to begin at once with the compound mixture, instead of with the iodide alone, or in solution in the usual bitter infusion.

Iron has long been known to be very useful in tertiary and some secondary affections; and I doubt not that its action is to counteract and rectify the blood cachexia which is so markedly present in these cases. But I believe,

so, that the quinine has its special advantage over other bitters; in all obability as more markedly opposing undue destruction of tissue. But atever the theory of their action, I can state that my case-books now show my instances of most successful results from the above method, after the lide alone, even in good doses, had been given in vain; and I have at this ry time two patients under my care in the Norfolk and Norwich Hospital, ffering from severe tertiary affections, who are rapidly receiving the most rked benefit from this mode of treatment, of whom one—a woman—has ffered for nine years from ulceration of the nose, hard palate, and pharynx, d has been treated in vain at one—if not two—public hospitals, where it ay be safely inferred she has taken the iodide of potassium in at least the ual doses.—*Lancet, Jan.* 18, 1868, *p.* 105.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

90.—ON TURNING IN MIDWIFERY.

By Dr. Robert Barnes.

If we were restricted to one operation in midwifery as our sole resource, I think the choice must fall upon turning. Probably no other operation is capable of extricating patient and practitioner from so many and so various difficulties. In almost every kind of difficult labour with a pelvis whose conjugate diameter exceeds three inches, it would be possible to deliver by turning with a reasonable prospect of safety to the mother, and in many instances with probable safety to the child. We might very greatly restrict craniotomy. We might dispense with the forceps; but neither forceps nor craniotomy will serve as a substitute for turning in special applications. It is difficult therefore to exaggerate the importance of carrying to the utmost limit the perfection of this operation. Yet the text-books exhibit a very inadequate appreciation of the subject. Turning by the feet was once said, not inaptly, to be the master-stroke of the obstetric practitioner. And still the operation was very imperfectly developed.

I propose to describe and illustrate with some fulness the conditions upon which the mobility of the fœtus *in utero* depends, the various modes by which the fœtus may be made to change its position, and the application of this knowledge to the practice of turning, embodying the teaching of Wigand, d'Outrepont, Radford, Simpson, d'Esterlé, Lazzati, Braxton Hicks, myself, and others.

Having regard to the various allied operations which it is convenient to class under a general description, I would define *Turning as including all those proceedings by which the position of the child is changed in order to produce one more favourable to delivery.*

There are three things which it is very desirable to know as much about as possible before proceeding to the study of turning as an obstetric operation—

1. What are the conditions which determine the normal position of the fœtus *in utero?*

2. What are the conditions which produce the frequent changes from the ordinary position?

3. What are the powers of nature, or rather the methods employed by nature, in dealing with unfavourable positions of the fœtus?

1.—*The Conditions that determine the Normal Position of the Fœtus in Utero.*—It would be idle to do more than glance at the fanciful ideas upon this subject that have obtained currency at various times, although most have an element of truth in them. Ambroise Paré believed that the head presented owing to the efforts made by the child to escape from the uterus. Even Harvey believed that the fœtus made its way into the world by its own independent exertions. Dubois endeavoured in a long argument to show that the fœtus has *instinctive power*, which determines it to take the head-position. Simpson rightly concluding that the maintenance of normal position depends very much upon the life of the fœtus, observes that it has no power of motion except muscular motion, and infers that the fœtus adapts itself to the uterus by *reflex muscular movements* excited by impressions—as by contact with the *uterus*—upon its surface. Thus we come down by a curious scale of theories, *in which* the philosopher may trace the influence of contemporary physiologi-

cal doctrines or knowledge. First, the fœtus is endowed with the high faculty of *volition*; then it falls to the lower faculty of *instinct*; and lastly, it is degraded to the lowest nervous function—that of *reflex* motion. I should be disposed to estimate at a still lower point the influence of the fœtus as an active agent in maintaining its position during pregnancy or labour. It is incontrovertibly true that the normal position of the fœtus and the course of labour are intimately dependent upon the life of the fœtus. But I think I am enabled to affirm from very close observation that a fœtus, if full grown and *only recently dead*—that is, for a few hours—may be nearly as well able to maintain its position and to conduce to a healthy labour as one that is alive. How is this? It depends simply upon the preservation of sufficient tone and resiliency in the spinal column and limbs to maintain the form and posture of the fœtus. Whilst alive, or only recently dead, the spine is firmly supported in a slight curve, the limbs are flexed upon the trunk, the whole fœtus is packed into the shape of an egg, which is very nearly the shape of the cavity of the uterus. It has a long axis, represented by its spine. This long axis, being endowed with sufficient solidity, resembles a rod, rigid or only slightly elastic. It is a lever. Touched at either pole, the force is propagated to the opposite pole. If the head impinge upon one side of the uterus, the breech will be driven into contact with the opposite point of the uterus; head and breech will move simultaneously in opposite directions. In labour, when the uterus is open to admit of the passage of the fœtus, the propelling power applied to the breech is propagated throughout the entire length of the spine or long axis, so that the head, the end furthest from the direct force, is pushed along in the direction of least resistance, turning at those points where it receives the guiding impact of the walls of the canal.

When the fœtus has been some time dead, the elasticity and firmness of its spine are lost; flaccidity succeeds to tonicity. Force applied to one extremity is not propagated to the other extremity—or, at least, very imperfectly so; the long axis bends, doubles up like a rod of gutta-percha softened by heat. If the fœtus in utero being in this state, pressure be applied to one side of the head, the head will simply move towards the opposite side of the uterus. And if labour be in progress, the propelling force applied to the breech will not be duly transmitted to the head, but will tend to double up the trunk, to make it settle down in a squash in the lower segment of the uterus or in the pelvis. The head—the cervical spine having lost its resiliency—will not take the rotation and extension turns. It will run into the pelvis like jelly into a mould. Or, at an earlier stage, the limbs, especially the arms, having lost their tonicity, drop or roll in any direction under the influence of gravity or of pressure; and hence may fall into the brim of the pelvis, constituting what are called transverse presentations. The influence of this law is clearly seen in the course of that process called "spontaneous expulsion," by which a dead child is expelled, a shoulder presenting.

Other factors besides the child have to be considered. Scanzoni correctly observes that the frequency of head presentation is dependent on the operation of various causes. 1. There is the force of gravitation; 2. The form of the uterine cavity; 3. The form of the fœtus (to which must be added the properties I have described due to life or death); 4. The quantity of amniotic fluid; 5. The contractions of the uterus during pregnancy and the first stage of labour. In the early stages of pregnancy the embryo is so small relatively to the cavity containing it that it floats suspended in the liquor amnii. But about the middle of pregnancy the fœtus grows rapidly; it acquires form; and, at the same time, the uterus grows more in its longitudinal than in its transverse diameter. As soon, therefore, as the fœtus—an ovoid body—attains a size that approaches that of the capacity of the uterus, the walls of the uterus will impose upon the fœtus a vertical position. The fœtus has become too long to find room for its long diameter in the transverse diameter of the uterus. Mutual adaptation requires that the long diameters of fœtus and uterus should coincide.

A condition not, to my knowledge, hitherto noticed, which has a powerful influence upon the determination of the child's position in utero, is the nor-

mal flattening of the uterus in the antero-posterior direction. In the non-pregnant uterus, the cavity of the body—the true and only gestation-cavity—is a flat triangular space, the angles of which are the orifices of the Fallopian tubes and the os internum uteri. A similar triangular superficies is marked out on each half of the uterus, anterior and posterior. The anterior superficies lies flat against the posterior superficies, touching it as if the two were squeezed together. When pregnancy supervenes these surfaces are necessarily separated to form a cavity for the growth of the ovum. But the original form is never entirely lost. The cavity is always more contracted from before backwards than from side to side. This is proved by direct observation if the fingers are introduced after abortion, or the hand after labour at term. The uterine cavity is closed by the flattening of the anterior and posterior walls together. This takes place the moment the uterus contracts. If the finger or hand be in the uterus at the time, this is plainly felt. Now, this flattened form of the uterus is the reason why the fœtus takes a position with either its back or its belly directed forwards. The fœtus is broader across the shoulders than from back to front, and therefore its transverse diameter is fitted to the transverse diameter of the uterus. There is a physiological design that dictates the downward position of the head. The fundus is the part designed for the implantation of the placenta, where it can grow undisturbed, and continue its functions during the expulsion of the child. The lower part of the cavity is therefore left free for the development of the embryo. Why the back is commonly directed forwards to the mother's belly is this—The child's back is firm and convex; its head is also firm and convex behind. The anterior aspect of the child's body is plastic and concave, and therefore fits itself better to the firm convexity of the mother's spine. It is clear that the two solid convex spines of mother and child would naturally repel each other; and the child being movable, it is the child's back that recedes, turning forwards.

2.—*The conditions which produce the frequent Changes in the Child's Position.*—Any considerable disturbance of the correlation of the factors which keep the fœtus in its due position of course favours malposition. The principal disturbing conditions may be stated as follows:—*An excess of liquor amnii* acts in two ways—first, it favours increased mobility of the fœtus; secondly, it tends to destroy the elliptical form of the uterus. The transverse diameter increasing in greater proportion than the longitudinal, the cavity becomes rounder. Hence the fœtus is no longer kept in a vertical position for want of the proper relation between its form and size and those of the uterus.

Obliquity of the uterus was considered by Deventer to be a main cause of malposition. It is now very much discredited, but I am disposed to believe that it has, not seldom, a real influence. Dubois and Pajot showed that in 100 women 76 exhibited a marked lateral obliquity to the right, four to the left, and twenty an anterior obliquity. Wigand had shown that deviations of the uterus to the right and forwards were far the most frequent. The normal direction of the non-pregnant uterus is nearly that of the axis the pelvic brim. As it grows during pregnancy, rising above the brim, the projecting sacro-vertebral angle and the curve of the lumbar column deflect its fundus to one or other side; and, if the abdominal walls be very thin and flaccid, the fundus will fall forwards. The tendency of these obliquities, if carried beyond ordinary measure, is to throw the axis of the uterus out of the axis of the pelvic brim, and to bring some other part than the vertex of the fœtus to present. The probability of this will be increased by the irregular contractions of the uterus likely to be excited by parts of the fœtus pressing unequally upon its walls. For example, in extreme lateral obliquity the breech may press strongly upon one side of the fundus; contraction taking place here, will drive the head further off the brim on to the edge, where, if it finds a *point d'appui*, it will rotate on its tranverse axis, producing forehead or face presentation, and favouring the descent of the shoulder. Wigand explains how a too loose and shifting relation of the uterus to the pelvis disposes to cross-birth. In this condition it is observed that the head is now fixed in one place, now in another, and now not felt at all.

He further says that any obliquity of the uterus exceeding an angle of 25° is unfavourable; and that even a lesser obliquity, with excess of liquor amnii or a small child, is likely to cause the presenting head to be displaced, and to bring a shoulder into the brim, especially if strong pains or bearing-down efforts be made *early* in labour.

He explained that the os uteri might be brought over the centre of the brim by internal drawing upon the os, combined with external pressure upon the fundus in the opposite direction, thus putting in practice the principle of acting simultaneously upon the two poles of the uterus.

Deformity of the pelvis or lumbar vertebræ is often a powerful factor. The comparative frequency of transverse presentations in cases of deformed pelvis is certainly greater than where the pelvis is well formed. I think, however, that *slight* deformity has more influence in causing malposition than extreme degrees. In these latter, malpositions are rarely observed.

The attachment of the placenta to the lower segment of the uterus is, as Levret has clearly shown, a cause of malposition by forming a cushion or inclined plane, which tends to throw the fœtal head out of the pelvic axis across the brim. Hence the frequency of cross-birth and of funis-presentation in cases of partial placenta prævia. But there are numerous cases in which the placenta dips into the cervical zone, growing downwards from the posterior and lateral walls of the uterus, without leading to hemorrhage, and thus not suspected to be cases of placenta prævia, which, nevertheless, form an inclined plane behind or on one side, and produce malposition.

Then there is the *influence of external forces*, as of pressure applied to the uterus through the abdominal walls. The dress of a woman at the end of pregnancy is a matter of no small moment. The pressure of a rigid busk of wood or steel upon the fundus of the uterus, modified by the various movements and postures of the body, may flatten in the fundus, thus reducing the longitudinal diameter of the uterus, or it will push the fundus to one side, causing obliquity. It will, at the same time, press directly upon the breech, and thus tend to give the fœtus an oblique position, throwing the head out of the pelvic axis. Pluriparæ should do the reverse of this. They should wear an abdominal belt, which supports the fundus of the uterus from below upwards.

Want of tone in the uterus, which implies inability to preserve its elliptical form, and a tendency to fall into rotundity, a form which obviously favours malposition.

Irregular or partial contraction of the uterus causes malposition. Naegele insisted upon this. He found that in some cases malposition was averted by allaying spasm.

The researches conducted by several German physicians, amongst whom I may cite Credé, Hecker, and Valenta, establish the fact that the fœtus changes its position with remarkable frequency. Valenta examined 363 multiparæ and 325 primiparæ in the latter months of pregnancy. He found that a change of position took place in 42 per cent. Change was more frequent in multiparæ, and in these in proportion to the number of previous pregnancies. Narrow pelves very frequently cause change of position. Circumvolutions of the cord, so often observed, are produced by changes of position, and hence bear evidence to the correctness of the proposition. It is interesting to observe that the general tendency of changes of position is towards those which are most propitious. Thus, cranial positions are least liable to change. Oblique positions are especially liable to change. These mostly pass into the long axis by spontaneous evolution. *Self-evolution is a very frequent resort of nature.* In some cases several changes of position have been observed in the same patient. The presentations are made out by external manipulations. Valenta thus describes his method of ascertaining a breech-position during pregnancy:—He lays his right hand flat on the fundus uteri, and then strikes the tips of the fingers as suddenly as possible towards the cavity of the uterus, against the part of the child lying at the fundus. By this manœuvre he has always succeeded in recognising the head, if lying at the fundus, by its peculiar hardness and evenness. He detects

the head in oblique and cross positions in the same manner. P. Müller relates a case in which within five days a complete version of the fœtus was effected six times.

Yet the fact of the "spontaneous evolution" of a living child, as described by Denman from actual observation, has been doubted!—*Medical Times and Gazette, Dec.* 21, 1867, *p.* 667.

91.—ON THE USE OF THE VECTIS IN ASSISTING PARTURITION.

By Dr. G. D. R. McCarthy, Esq., Wellington, Salop.

[Mr. McCarthy for many years used the forceps, but now for some years has greatly preferred the vectis. He says:]

I have found the vectis the best instrument. For if, after all, it cannot extract the head when it has descended into the lower region of the pelvis, the forceps can but be used, and would require but little trouble to apply them. It has nearly superseded the use of the forceps in my practice. Not entirely; for occasionally one meets with a case of impaction which calls for the forceps or even the perforator; though I am thankful to say I have not met with many such. I consider it as a sort of artificial hand. An accoucheur must often wish he could introduce his hand so as to pass it behind the head and help it in its passage. Now this desideratum is in a great measure potentially, though not actually, secured by the vectis in the manner in which I have used it.

My method is to introduce the four fingers of my left hand, well anointed with lard, with their backs to the perineum, and, bringing the handle of the instrument to the pubes, the thighs being held open, slide the convexity of the curve along the inside of my fingers and then between the sacrum and the head, which is often done in a minute or two. I then bring the handle to a line in the direction of the upper axis of the pelvis, and, first depressing it—the patient lying at the edge of the bed—and then gently elevating it, the curved part passes over the head. Then, holding the handle steadily with my left hand, and carefully maintaining a sustained traction, examining with my right the progress, I keep the head from receding in the interval of the pains, which, as a proof how glad—if I may so express it—Nature seems of seasonable assistance, become more powerful and effectual. If I do not gain anything, after a short time, I partially withdraw the instrument and introduce it on the lower side of the head, when it lifts it up like a lever, using some degree of traction at the same time. If I find I cannot slide the instrument over the head as I wish, I withdraw it for a time and then introduce it again, when it generally succeeds. Under these circumstances, I give the ergot in decoction, and thus gain all the natural and artificial combined power possible.

I am happy to say that I have greatly facilitated and accelerated the delivery of many by this simple instrument, using it, as I have said, like a hand, and never injured the mother, and seldom the child. I believe it requires a little tact, which may be gained by practice; and whoever has tried it somewhat frequently—I mean as often as the protracted nature of cases, or the failing of natural efforts, have caused alarm and anxiety to any degree—has, I dare say, experienced its usefulness.—*British Medical Journal, Dec.* 14, 1867, *p.* 542.

92.—ON THE INDUCTION OF PREMATURE LABOUR BY INJECTION TO THE FUNDUS OF THE UTERUS.

By Prof. J. Lazarewitch, Professor of Midwifery at the University of Kharkoff, Russia.

[The safest, most effectual, and readiest method of inducing premature labour is undoubtedly that known as Cohen's, viz., the injection of water by means of a catheter between the uterine walls and the fœtal membranes. After

preliminary remarks to the above effect the author recapitulates as follows:]

1. The surest methods of induction of premature labour are those in which the body or fundus of the uterus is excited.

2. In the method of Cohen, the greater the quantity of fluid used in injections the nearer it reached to the fundus of the uterus, the surer was the action.

3. When, after injection, the fluid passed upwards to the fundus of the uterus, violent contraction always ensued immediately.

4. In the experiments on rabbits the thickest part of the uterine walls proved to be the most irritable; in a pregnant woman the body and fundus of the uterus is the thickest.

5. On introducing the sound into the cavity of the uterus in its normal state, pain is caused only by contact with the fundus.

6. At the time of parturition contractions of the uterus are more quickly excited by rubbing the fundus of it than by the irritation of its inferior section.

7. The inferior section of the uterus is exposed to many different irritations, and therefore its sensitiveness is blunted.

8. The commencement of parturition is caused by the gradual separation of the membranes of the ovum from the walls of the uterus, and when this separation is complete, nearer to the fundus of it.

From the above statements we may conclude that, *for sure action of injection, it is necessary that the fluid injected should approach as near as possible to the fundus of the uterus*, this being the most sensitive to irritations.

The fluid injected by my method is directed upwards to the fundus of the uterus, there spreading, separates from it the membranes of the ovum, and flows back only after some minutes, or even after some hours, because it is easily retained in the superior section of the uterus. In this manner considerable and rather protracted irritation is produced at the fundus of the uterus.

In those cases of induction of premature labour in which the fluid was not retained in the cavity of the uterus, but flowed back immediately after injection, there was little or no effect, because the fluid spread only in the inferior part of the uterus, little sensitive to irritations. Cohen was convinced that for a successful injection a conical tube should be employed, which, being closely applied to the orifice of the uterus, would thus prevent the fluid from flowing back. In my opinion the fluid should, in this case, be directed to the fundus of the uterus. But if injection is used, when a woman is brought to bed not for the first time, in the last month of pregnancy, when the vaginal portion is very soft, and its internal and external orifices are wide open, it is impossible to press the tube to the orifice of the uterus, so as to prevent the fluid from running back; still less possible is it when the uterine orifice is considerably distended, and it is necessary to excite lingering labour; in these cases, as in all others, it is very convenient to use also a conical tube, not with the end closed and side apertures, as Cohen advises, but with a single aperture at the end, in such a case all the fluid injected must pass in an upward direction, to the fundus of the uterus. In this modification of the tube the difference between my method and that of Cohen consists—by his method the injected fluid must pass into the cavity of the uterus, by mine it must be directed *to its fundus*.

The tube employed by me in the first three cases, which I am about to describe, is elastic, from six to eight inches in length, one end being one and a half line in diameter, and the other six lines; its small end is somewhat rounded, and has one aperture. For the more convenient introduction of the tube, and to give it the necessary curvature, a metallic wire is introduced into it, bent to a right angle in two opposite directions. Thus a joint is formed, which retained by the edge of the larger aperture of the tube, does not admit of its passing through the narrow aperture of the tube. The tube is fixed to the end of a syringe, and is adjusted to it by means of gutta percha.

In 1861, during my stay in Vienna, the instrument maker Leiter adjusted

the above-described tube to an india-rubber bag, which worked like a syringe. With such an apparatus Professor C. Braun in one of the following cases employed my method of induction of premature labour.

In the same year, at Berlin, Professor Martin made to me the following remark, respecting my method of injection: he considered the possibility of air passing into the cavity of the uterus to be dangerous.

It becomes quite impossible with a well constructed apparatus for injection and the proper employment of it. After *filling the syringe, and holding it with the handle downwards, the piston should be moved till the air rising to the end of the tube, be expelled by the water, which has to pass the aperture of the tube.* The piston must move easily, and fit the cylinder exactly.

For the purpose of injecting fluid into the cavity of the uterus, M. Edelberg in Kharkoff has, after my direction, constructed a special apparatus. This apparatus consists of a metallic cylinder seven and a half inches in diameter. A piston furnished with an indented stem is worked by a toothed wheel, with a spring inclosed in a drum attached to the upper part of the cylinder. The spring is wound up by a small crank, which may be taken off during the operation. The action of the spring, and consequently of injection, may be retained with a stop. By slightly pressing a knob it is easy to connect the stop with the toothed wheel, and in this manner arrest the action of the spring. On the smooth side of the stem are divisions marking the quantity of fluid contained below the piston.

To the cylinder a tube is attached six inches long, and three lines thick, with one aperture at the end of it. This tube is made of pliant metal, and therefore it may be bent at pleasure; on its concave side it is marked to show how much of it is introduced into the cavity of the uterus.

Another tube, similar to that described above, but with a closed end, and with two apertures, one in front and the other behind. The fluid injected through this tube must spread over the uterus, and cannot take a side direction to the orifice of the viaduct.—*Obstetrical Transactions, Vol. IX., p.* 167.

93.—ON THE INDUCTION OF PREMATURE LABOUR BY THE DOUCHE.

By Dr. Thomas Telford, Ex-Assistant Physician, Rotundo Hospital, Dublin.

The induction of premature labour in cases of undersized pelvis, is an operation of great importance to the practitioners of midwifery, and has, for a long time, engaged their attention; by its means we are sometimes enabled to save the life of the child, and thus add greatly to the happiness of the parents. Various modes of proceeding have from time to time been adopted, and with a varying amount of success—such as rupturing the membranes, separating the membranes round the os, passing a catheter between the membranes and the wall of the uterus, by the hydrostatic dilator of Dr. Barnes, the administration of ergot, and by the douche; the latter is the plan which I adopted in the three following cases, in preference to any other.

Rupturing the membranes is very uncertain in its action. I have frequently seen women come into hospitals hardly in labour, though the membranes had been ruptured some days before admission. Early rupture of the membranes predisposes to a rigid condition of the soft parts, adding a new difficulty to the case; the separating the membranes, or passing a catheter between the membranes and uterus, unless done with great care, is likely to rupture the bag of waters, and is, therefore, open to the same objection. Of Dr. Barnes's dilator I have no experience. The administration of ergot, I am confident, is not efficacious in producing labour, as, from a series of experiments I made under the direction of Dr. Denham, master of the hospital, for the purpose of testing the toxic effects of ergot on the fœtus, we found that ergot *ministered* to a woman seven or eight months pregnant, neither induced pain, or exerted any deleterious effect on the child.

The application of the douche is extremely simple, it is best done by placing the patient in the usual obstetric position, with the hips drawn well over the edge of the bed; by passing a full-sized Ferguson's speculum, the os is brought into view, the nozzle of an ordinary syphon syringe is then inserted into the os, and a continuous stream of water injected into the cavity of the uterus. On the withdrawal of the syringe, the water pours out of the uterus in a full stream; this may again be repeated a couple of times; one such application will generally induce labour within twenty-four hours. Before using the syringe you must be careful to fill it with water, so as to exclude the admission of any air into the uterine sinuses. Some physicians advise the alternate use of hot and cold water; this, I think, a matter of unimportance; tepid water is most agreeable to the patient, and should therefore be used; otherwise, as the action of the water is altogether mechanical, it is immaterial whether we use hot or cold. The action of the water closely resembles the action of the uterus, by gradually separating the membranes around the os and cervix uteri. The three following cases illustrate the rapidity with which labour is induced.

Mary Brien, aged 23, pregnant of her second child, was admitted to hospital November 28th, 1866. She had been delivered in the Rotundo Hospital, at Christmas, 1865, by the crotchet, owing to narrowing in the antero posterior diameter of the brim. She was then advised, should she again prove pregnant, to come into hospital between the 7th and 8th month, in order that premature labour might be induced, as we feared, owing to the narrowing which existed, a child at the full term could not be born alive. She neglected to come into hospital till just eight months pregnant, when the probability of saving the child was much less than had she been seen earlier. Having cleared out the bowels, I douched her in the manner already described at 12 noon, November 30th; labour commenced at 2 p.m., the pains being short and frequent. At 12.30 a.m., of December 1st, the membranes ruptured, no presentation being then discernible, it being altogether out of reach of the finger.

At 8.30 a.m., I was called to see her, and, on examination, found the right hand in the vagina; the child was alive, as proved by the hand grasping the finger, when introduced. Version was at once determined on, and chloroform having been administered, I passed my right hand, and turned with the greatest care; the breech and body of the child were then delivered; the arms offered considerable resistance, and were with much difficulty brought down; the delivery of the head was then attempted in the usual manner, and a considerable amount of extractive force used by myself and Dr. Denham, but without avail. Fearing that any further force might tear the vagina from the neck of the uterus, I delivered the child by perforating behind the ear and using the crotchet; the child was a male, and much larger than most eight months' children. Her convalescence was rather slow; she suffered from some tenderness of the abdomen for a few days, and afterwards from an attack of sciatica, but went out quite well in a couple of weeks.

The second case is that of Eliza Scott, aged 26, pregnant of her fourth child. Her first two children were delivered by the crotchet. She was then advised to come into hospital when seven months pregnant, should she again prove so; this she did, premature labour was induced by the douche, and she was delivered of a live child in Sept. 1862; the child lived till eleven months old, when it was carried off by measles. Finding herself again pregnant, she applied for admission on the 28th of February of the present year, being at the time nearly eight months gone. The bowels having been opened, the douche was applied on March the 1st, at 11 a.m., and again at 4 p.m.; labour came on that evening, the membranes ruptured at 11 a.m., March 2nd, the os being then fully dilated. The pains were quick and strong, but as the head had made no advance at the end of two hours, and the fœtal heart increased in rapidity, delivery by the forceps was decided on. With the assistance of Dr. Cronyn she was delivered, after considerable difficulty, of a healthy male child. Her convalescence was most satisfactory, not a single bad *symptom having arisen.*

The third case is that of Mary Doyle, aged 21, pregnant of her second child. She had been delivered by the crotchet June 25th, 1866, and was then advised to come in when seven months pregnant, this she neglected doing till past the eighth month. The douche was applied at 11 a.m., June 21st, 1867; labour came on almost immediately; the membranes ruptured at 5.30 p.m., when the right arm was found in vagina. She was put under chloroform and version performed: the arms were brought down with ease, but no amount of traction would deliver the head; the head was then perforated through the mouth and extracted; her convalescence was most satisfactory, she going out on the eighth day.

The foregoing cases are interesting as showing the certainty with which premature labour can be induced by means of the douche, when properly employed. In the Medical Times and Gazette for November 2nd of the present year, a paper was read by Professor Lazarewitch, "On the Induction of Premature Labour by Injection to the Fundus of the Uterus." This operation, however, requires a special apparatus, and is not more effectual than the douche used in the ordinary way. [See preceding article, 92.]

In two of the cases related the arm was the presenting part, and turning had to be performed. This operation is recommended by Sir James Simpson in cases of undersized pelvis, where the head is the presenting part. On the supposition that the head will mould itself to the pelvis, I have tried it myself in three cases, and seen it done once or twice by others, but the result did not equal my expectations. Of three cases I had myself, the child had to be delivered by the crotchet in all, and in one the uterus was torn from the vagina, the woman dying in half an hour; yet, those were cases considered favourable for the trial.

These cases are brought forward in order to show the facility with which premature labour can be induced.—*Medical Press and Circular, Nov. 27, 1867, p. 483.*

94.—ON A CASE OF PUERPERAL FEVER OR PUERPERAL PYÆMIA, AFTER AN ABORTION.

By Dr. Snow Beck.

Paper read before the Obstetrical Society of London.

In this case, abortion was induced at the end of the fourth month, in a healthy young lady, aged twenty-six. After a short time the symptoms observed were, dirty and muddy complexion, great weakness, intense thirst, constant retching, cold perspirations, extremely rapid pulse, mind clear, though wandering sometimes, and much irritability. She died on the sixth day after the abortion. The post-mortem examination showed the uterus large and flabby, a portion of placenta imbued with purulent fluid, adherent to the uterus, and attached to it a considerable amount of coagulated blood; purulent fluid in the uterine sinuses, which were otherwise healthy, but so patent as to admit of fluid being injected through them; peritoneum not injected, though the pelvic portion was covered by a thin layer of soft lymph, and there was effusion of brownish serosity into the cavity; lobular congestion of the lungs; effusion of serum into the pleuræ, with soft lymph on the surface; small collections of similar exudation beneath the pleura. The minute structure of the internal surface of the uterus was fully detailed, and the relations of the placenta found to accord with the descriptions of the Hunters and others. The case appeared clearly to show the purulent infection of the general system through the pervious state of the uterine sinuses; also, that one of the most fatal forms of puerperal fever arose from this cause; the sinuses being pervious in consequence of a want of permanent contraction of the uterus. The statement of Dr. Barnes that he had repeatedly seen puerperal fever after perfect contraction of the uterus, was examined, and the cases related by him were considered by the author to be diseases very different from puerperal fever,—viz., Bright's disease, obstruction of the gall-duct, acute atrophy of the liver, scarlatina, and phlegmasia dolens.

Lying-in women, when exposed to the infection of zymotic diseases, frequently took those diseases, but their essential characters remained unchanged. They often existed epidemically, and were infectious, and hence arose the assumed epidemic character and infectious nature of puerperal fever. In the treatment it was considered of much importance to prevent the purulent infection, by effecting a firm and permanent contraction of the uterus; when it had occurred, to cleanse out the uterine cavity, and give the sulphites. The large administration of brandy, and the application of a light bandage were prejudicial, and the latter impossible, from the tenderness of the abdomen and uterus.

After some remarks from Dr. TYLER SMITH, the following letter from Dr. BARNES, who was unavoidably absent, was read:—"I am confident that contraction of the uterus, although a very desirable object to attain, is not a security against puerperal fever. I wish also to observe that Dr. Beck bases his criticisms upon five cases referred to in my lectures, as if these cases embodied very complete views upon puerperal fever. The lectures were broken off for want of time. I had, of course, a great deal to say about septicæmic or pyæmic puerperal fever arising in the patient's own system. I maintain that the division I propose in those lectures on puerperal fever into excretory, uræmic, cholæmic, scarlatinal, septicæmic, &c., is of great clinical value. For example, a lying-in woman is taken with fever. It is very difficult, sometimes impossible, at first, to tell whether that fever is really of epidemic origin, as scarlatinal, or whether it be strictly of puerperal origin. The puerperal state will impress its stamp upon any kind of fever, no matter whence obtained. Therefore it is a sound clinical proceeding to regard every fever in a lying-in woman as puerperal fever first; then to endeavour to analyse the symptoms with a view to discover the peculiar or special nature of that fever. To say that true puerperal fever is not contagious is to refuse to believe in one of the best-attested facts in medicine."

Dr. PLAYFAIR said that he was in the position to relate a fact connected with puerperal fever which seemed to him to be of itself sufficient to negative Dr. Beck's theory that zymotic diseases were not modified by the puerperal state. Some years ago a lying-in ward had been established at King's College Hospital. The utmost care had been taken in the construction and management of the ward, but in spite of every precaution the mortality had been for the last year or two excessively high. About a month ago numerous cases of erysipelas appeared in the surgical wards, and immediately afterwards the two most recently confined women were attacked with a very adynamic form of puerperal fever, which proved fatal in both instances. There can be no doubt that the cause of the disease was the same as that which was producing erysipelas in the surgical wards. There was, however, no trace of erysipelas as such in the puerperal cases, and the natural inference was that the action of the poison was modified by the state of the patients. He was happy to state that the authorities of the hospital had now determined to close the ward altogether. Doubtless a relaxed condition of the uterus was, as Dr. Beck maintained, a strong predisposing cause of puerperal fever; but he believed it would be a most false and dangerous conclusion if we were to generalise from this fact, and overlook the other and well-established modes by which puerperal fever was produced.

Dr. GRAILY HEWITT stated, that the views he had on a former occasion enunciated on the value of pressure by means of the binder in the early stage of puerperal fever, and as to the extreme value of large doses of alcoholic stimulants at the same period, he still maintained. In a large number of cases, an account of which he hoped shortly to bring before the Society, he had tested their value.

Dr. WYNN WILLIAMS considered the disease in question to be identical with that met with during the progress of other diseases, but objected to the name pyæmia, believing it to be an accidental occurrence during the progress of a certain disease, and not always present. He alluded to septicæmia which was caused by the absorption into the system of the putrid emanations from decomposing animal matter. Pure pus itself, when introduced into the

circulation, was no more injurious than any other foreign material, as quicksilver. When scarlatina, erysipelas, and other allied diseases are present the air is always contaminated with putrid emanations, and this, being brought in contact by vibrios generated in such an atmosphere, or other ways, readily sets up putrefaction in the discharges of the parturient woman, and these, being retained, are very apt to infect the patient. If, therefore, in the discharges there is the least offensive smell, it is right at once to have recourse to some disinfectant injection, of which a solution of iodine is the best.

Dr. BRAXTON HICKS was inclined to join issue with Dr. Beck upon the statement that this preparation tended to prove the existence of open mouths into the placental cavity. He held it almost certain that such openings did not normally exist. In fact, Hunter had overlooked the existence of the delicate membrane which represented the wall of the vessel as it passed through the innermost layer of uterine tissue to ramify on the mucous surface opposed to the placenta. The injection Hunter employed burst through this membrane, and thus it appeared to lead into the placental cavity. This membrane would readily be destroyed under the conditions in which the uterus was placed in the case related. But he thought we should make a dangerous and retrograde step if we cease to acknowledge the zymotic diseases as a cause of so-called puerperal fever. He considered that that disease may be divided into two great classes, with a few minor kinds—namely, that produced by décomposition of a clot, or the secretions in the uterus, thence called endogenetic; and that derived from zymotic poisons, of which scarlatina and erysipelas were the most common. He had in his own practice found that about three-fourths of the puerperal fever he saw was somehow mixed up with scarlatina. But in the lesser class, owing to the decomposition of clots, he thought very great benefit was to be derived from washing out the uterus in all cases where the discharges were offensive. Of disinfecting fluids he preferred the solution of permanganate of potass, because it showed when it had done its work. It was a curious fact that offensive lochia had been present in all cases but one of acute puerperal mania which he had seen.

Dr. BRUNTON said that, with regard to the theory of the production of puerperal fever by zymotic poison, his experience was decidedly negative. He had attended several confinements in which, not only was there scarlet fever in the house, but the children were actually lying ill in the room, and no bad symptom had occurred to the mother during her convalescence. He agreed with Dr. Beck to a considerable extent, especially as to the advantage of securing a firm contraction of the uterus after delivery.

Dr. SANSOM could not agree with Dr. Tyler Smith, that drugs were hopeless when once pyæmia had set in, and that "antiseptics were the feeblest of all feeble measures." He was glad to hear that the author of the paper had found the treatment by the sulphites successful. He believed Dr. Polli's introduction of the sulphites into therapeutics to be one of the most valuable additions to medical science. He did not agree, however, with Dr. Polli in his idea that the sulphites do not act upon the organised germs of disease, but upon the pabulum in which these germs are propagated. He believed that they acted directly as septicides. Dr. Sansom stated that he had succeeded in producing a series of salts which combined the diffusibility of the sulphites with the potential efficacy of carbolic acid. These agents, the sulphocarbolates, he would take an early opportunity of introducing to the notice of the profession.

The PRESIDENT agreed with the author in his view of the pathology of the case which he had brought before the Society. It was one of puerperal septicæmia, due to the absorption into the blood of putrid matter from the uterus. But while admitting that perfect contraction of the uterus was most important, he could not consider the want of it as more than an occasional element in producing puerperal fever, which had many other *sources*. He was of opinion that cases of sporadic peritonitis, should not be *tabulated* with cases of puerperal fever, as the former arose from ordinary

causes of inflammation, while puerperal fever, whether accompanied or not by peritonitis, arose from the introduction of poison into the blood, either by the veins or the uterus, or by the lungs in inspiration. Contrary to the author, he was a strict believer in the contagion of puerperal fever immediately from patient to patient, and medially through a third person. He was also convinced that other fevers, as typhus, scarlet fever, measles, and the emanations from hospital gangrene, had been prolific causes of puerperal fever. The taint conveyed from post-mortem examinations was another source of puerperal fever. This was well illustrated by the statistics of the Vienna Lying-in Hospital, where at one time, when the students passed directly from their dissections to the lying-in women, the mortality was 1 in every 10 deliveries. When this arrangement was changed, the per-centage of deaths fell to 1 in 74. Lying-in hospitals are no doubt a boon to poor women without homes, but one greatly mitigated by the fact that the mortality in them from all causes is rarely less than 1 in 90 or 100, and generally greater. The institution of cottage hospitals would no doubt greatly lessen that mortality, and he hoped some day to see them established. Out-door institutions, giving home attendance, such as the Royal Maternity Charity, presented a much smaller mortality. In that Charity only 1 death in 350 deliveries occurred. In conclusion, he could add, from long experience, his testimony to the great value of disinfecting injections of a proper temperature, timely resorted to, in preventing and arresting at its outset septicæmia of uterine origin in cases of putrescent lochia. But they came too late when delirium and other formidable symptoms had supervened.

Dr. Snow Beck, in reply, observed that lying-in women were no doubt liable to the various zymôtic diseases; but when they did occur they presented the same essential characters as at other times. Nothing could be more erroneous than to term every fever and every disease which occurred after parturition "puerperal fever." Typhus fever, scarlet fever, &c., had no doubt again and again been introduced into lying-in hospitals, and decimated the inmates; but the destroying agent was still typhus fever, scarlet fever, &c. Suppose the transformation theory was adopted, it would lead to this: that pregnancy had the marvellous power of transforming every disease it might be associated with, not into so many other diseases, but all into one and the same disease—puerperal fever. Such conclusions could only be admitted after the strongest proofs, and not on the mere assertion of anyone. Puerperal fever had been correctly compared to surgical fever, seeing that they both arose from the same cause—purulent infection of the general system. In primiparæ the uterus often contracted very languidly; and when it did contract well, and expel both the fœtus and the placenta, it was far from uncommon for it to become gradually relaxed; and it was this relaxation which caused the sinuses to become so patent as to admit of purulent infection through them, and which required therefore to be so much guarded against. There could be little doubt that many diseases which had nothing to do with the pregnant state were included under the term "puerperal fever," also that purulent infection of the general system through the pervious sinuses give rise to one of the most dangerous affections following parturition. But whether these propositions included all the phenomena observed in puerperal fever, or not, could only be determined by the careful clinical observation of facts, and equally careful examinations after death.—*Lancet, Dec.* 28, 1867, *p.* 805.

95.—"CARBOLIZED SPONGE TENTS."—ON THE DEFECTS OF ORDINARY SPONGE TENTS: AND ON AN IMPROVED METHOD FOR THEIR MANUFACTURE.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

The use of compressed sponge for the dilatation of the cervix uteri has become so very general, that the subject of this paper may appear almost trivial in its character. But in my opinion the grave objections which exist

against the employment of those in common use fully justify an earnest attempt, either to remove defects, so generally complained of, or to produce a substitute at once as cheap and effective, but also free from the bad qualities possessed by the sponge. An eloquent writer on Uterine Surgery says, "He who gives us an efficient, pleasant, and cheap substitute for sponge tents will confer a great boon on surgery. I know," he adds, "of no competent substitute, or I would be too willing to adopt it." It was expected that the sea-tangle tents would have served this purpose as efficiently as the sponge. But the experience of many obstetrists and my own, has shown that they are far less efficient as dilators, and but little less offensive in their employ than the material for which they are proposed to be substituted. In addition to which they are not easily secured in position, and are apt to be ejected from the canal of the cervix before their work is properly accomplished.

I must fully agree with the eminent authority already quoted as to the excellent qualities of sponge as a dilating agent, and I believe that no substitute for it is as yet known to us which can compare with it in its applicability to the end in view. This material is so flexible, elastic, cheap, and handy to use, adapts itself so perfectly to the varying calibre and curvatures of the cervical canal, that it is much to be regretted, that in actual use it is attended with such disgusting and dangerous properties as to make us eager for the discovery of any efficient substitute. In addition to its other good qualities is this, that when properly made it remains *in situ*, and requires no tampon to keep it there, or at any rate none larger than a small pledget of cotton-wool.

But the sponge tents sold by the instrument makers, even by such as have, as we would suppose, the largest experience in their manufacture, are so clumsy and ill-proportioned as to be really a discredit to their producers. Some of those which I have obtained from Edinburgh, and which were manufactured by the eminent chemists instructed by Sir James Simpson, are as bad as any I have procured at places of less renown in London. They are seldom sufficiently compressed to be of as much dilating power as they really might possess; and their length is so extravagantly great that it is impossible to imbed them entirely within the cervical canal without causing a considerable portion to project far into the proper uterine cavity—a most objectionable circumstance. And if they cannot be thrust up to this extent, then the lower portion projects itself into the vagina, and owing to the form of the tent, the rest generally follows and is found some hours after, lying in that canal instead of in its proper place.

The common sponge tent is not merely liable to this defect of want of proportion to the canal we propose to dilate by its means, but also by its figure it is incapable of answering that purpose as it ought to do. It is frequently made of so extremely conical a shape as to facilitate, in fact, to invite its own rejection. It is saturated with tallow and wax, and a thick layer of this compound covers the surface. Since this material does not quickly melt at the temperature of the body it follows that the sponge tent coated with it becomes really a slippery wedge, which refuses to be retained in place unless propped up by a plug. If the material were more quickly melted, then the sponge would at once adhere to the sides of the cervical canal and keep its proper position. The addition too of a filthy and greasy double fold of tape, to be allowed to hang out externally, appears as though it were designed to invite the premature removal of the tent by its impatient wearer.

With an instrument thus constructed, and so ignorantly formed, it is not surprising that the surgeon often bungles over the simple business of inserting the tent. Often, indeed, much pain is produced by the clumsiness of the thing, and especially when to keep it *in situ* we attempt to press it high up into and through the *os internum*. But defects of the kind here named are easy to be corrected by any one who will take pains to adapt his instrument to his case. And there are probably few members of this Society who will not agree with me in the opinion that there is an almost infinite variety in the *size, capacity*, and conformation of that portion of the body for which this *instrument is* designed. A sponge tent perfectly well adapted for one vari-

ety of the cervix uteri is entirely unfit for another. In this, indeed, this material, sponge, displays its superiority over all its substitutes—that it can so readily be fashioned to meet the requirements of the individual case to be dealt with. And in sponge tents properly manufactured, of a suitable description of sponge, of an appropriate length and figure, adapted to the precise circumstances of the canal proposed to be operated upon, we possess, as it may be thought, all the qualities necessary to us.

But there is a radical defect in sponge as a dilating material, which is so serious as almost to neutralize its value. This is the horrible facility of decomposition which it seems to confer upon the discharges of these passages. No surgeon can have used sponge tents to any extent without a fear of the result to the patient in the production of blood-poisoning. The disgusting grave-like odour which is generated by them, after a few hours' residence in the cervical canal, can only be fully appreciated by those who have frequently removed them in this putrefying condition. It is horrible alike to the surgeon and to the patient, who is frequently distressingly conscious of this impurity. The cause of this decomposition seems to have escaped the notice of most observers—at least I have never seen it alluded to. There can be no doubt it is really due to the atmospheric air entangled in the interstices of the sponge, which, favoured by the high temperature, reacts upon the mucous and albuminous constituents of the discharges.

It is in consequence of the existence of this most offensive and dangerous property of sponge that surgeons have objected most strenuously to its employment. And the tents of the *Laminaria digitata* are undoubtedly less liable, on the whole, to this objection. There can, I think, be little doubt that serious if not fatal results following after dilatation with sponge tents may be attributed to this proclivity to decomposition, and though to some degree an advanced stage of putrefaction may be prevented by a more frequent change of the tents—still the retention of them only for a few hours is sufficient to commence it—and may be enough to excite mischief in a delicate and unhealthy subject.

In reply to these observations it may be said that the sponge tents of Dr. Marion Sims, prepared by soaking sponge in mucilage of gum arabic, and then drying, are properly manufactured, of convenient sizes and shapes. The objection just raised, applies, however, equally to this as to the other kind of tents—and it is the most important of all. In addition, the tents made on Dr. Sims' plan have certainly not succeeded as well in my hands as in his own. They are hard, inflexible, and almost resemble the sea-tangle tents in their density—and they possess a configuration which by no means represents that of the canal for which they are designed. The mucilage, also, of which they partly consist, is extremely apt to be acid in its quality, and thus to act as an irritant of the surface to which it is applied. They have the advantage, however, over the ordinary kind, of being easily retained in place, and they have no dependent strings attached to their base.

Having in vain sought to discover a substitute for sponge as a basis of the dilating tent, I have, nevertheless, succeeded in producing by its means a dilator, to which, I think, few if any of the objections hitherto brought against sponge can be truly applied. I submit to the notice of this meeting the specimens of carbolized sponge tents on the table, and I believe they will be found to fulfil every object for which sponge is valued as a dilator, without its general disadvantages and defects.

The carbolized sponge tents possess the following properties:—

1. They are sufficiently firm to admit of easy introduction, and yet pliant so as to adapt themselves to the varying curvatures of the cervical canal.

2. They are prepared very quickly, of cheap and simple materials, and may be made by any surgeon for his own use with great facility.

3. They take firm hold of the cervical canal, and require no tampon to keep them in place: and are easily removed with a dressing forceps, the tape being dispensed with.

4. They act with great efficiency as dilators—two or three in succession being generally sufficient to open the cervix so as to admit the finger.

They remain perfectly inodorous, and exhibit no sign of putrefaction during their retention in the uterine canal.

Their properties are due—1, to the peculiar, half-ovoid shape of the tent; 2, to the investing material, the theobromic oil; and 3, to the addition to the latter of a certain quantity of pure carbolic acid. In shape they have a rough resemblance to the cervical cavity, and are not acute pyramids or elongated cones like the ordinary tents—they are thus more readily embraced and retained. The coating employed is hard yet possesses a certain pliability, and very quickly melts at the temperature of the body. The well-known properties of carbolic acid, as a disinfectant, confer on these tents their most valuable attribute of resisting decomposition. In the quantity which I recommend to be used, this addition does not irritate the surface to which it is applied.

They are thus manufactured—I shall be happy at the close of the meeting to give any practical information on this point to the members desiring it, my object being entirely a practical one in submitting so homely a subject to this Society:—

Sponge, which must not be of too close, nor of too cellular a character, may be had in small pieces, of the chemists, at a very small cost. The larger pieces are neither so good in quality nor so adapted for our use, and they are much more expensive. It must be well washed in several waters until all dirt, sand, and fragments of stone are removed from it. It is then to be very thoroughly and carefully dried. It is to be cut into pieces of about two inches in length, and of such a shape as to produce, when rolled tightly over with tape, a shape somewhat fusiform in its character. The pieces will be cut thicker or thinner according to the degree of dilatation intended to be effected. For the largest sizes they will require to be as thick as two fingers, or even larger. For the smallest—about the thickness of the little finger.

It is important to remember that the canal we propose to dilate by this means is not generally larger than from one inch and a quarter to an inch and a half—and is often not so long. It will therefore be necessary to have some of the pieces much shorter than two inches, but very rarely, and only in abnormally elongated cases, longer than this. The sponge tents which have been supplied to me by some chemists have been in several instances nearly three inches long—and would, if I had ventured to use them, have extended from the os uteri to the fundus of that organ! I have been obliged to cut down these most clumsy contrivances to bring them to a proper size.

At any tool warehouse a slender square-sided instrument may be got called a "broach," and this forms an excellent axis on which to roll a sponge. On a round wire or other tool it constantly slips during the making. On the table are specimens of the kind I find most useful. Inserting this pointed tool in the centre of the piece of sponge so as to form its long axis, one of the cut edges is to be forcibly squeezed down and pressed under the opposite, which may be made to roll over it and hold it down.* A piece of narrow tape is then to be rolled very tightly over the apex and brought up in regular spirals, lying close to each other, to the base, where it is fastened off. The fusiform shape is to be carefully preserved in binding the tape over the dried sponge. The tent is now ready for the first coating.

The best material for this part of the process is the cocoa-butter, or oil of theobroma. It has the advantage of being very firm, and so holding the compressed sponge well together, and it so quickly melts at the temperature of the body that the sponge tent immediately takes to its work, and does not slip from the canal. In general it is useful to add a few grains of white wax—from three to five—for every drachm of this oil. And in hot weather this is the more necessary. But I object to any larger addition, since it elevates the melting point, and vitiates the action of the tent.

In order to confer on these tents their peculiar disinfectant character, I

* In the centre of the sponge I always place three or four strands of common cotton wick which have been dipped previously in pure carbolic acid.

have with great success, added to each ounce of the theobroma oil, one drachm and a half of pure carbolic acid. The effect is most remarkable; the tents are withdrawn as devoid of offensive odour as when they were introduced—and by this means I believe the great desideratum has been accomplished. It is certainly true that no longer can the sponge tent, thus prepared, be regarded as a disgusting, or worse, as a poisonous agent. The carbolic acid, in the proportion given, has no injurious effect, in the cases I have observed, upon the surfaces with which it is brought in contact—and these proportions are sufficient. If used in larger quantity it is very probable that some irritation and soreness would arise.

The tent is dipped in the butter thus prepared, and kept in a fluid state either over a small flame or in a hot-water bath. It is simply immersed in the melted oil for a few seconds, so as to saturate only the outer layer of the sponge, and this it does, penetrating readily through the tape. The tent is then placed in a very cool place, and after it has solidified the tape is cut and unrolled. All that is now necessary to complete it is to dip it once more into the same preparation—which for this latter purpose must be much cooler than before, and of about the consistency of cream. By this means a smooth and uniform covering is given to the tent, and it is ready for use. The tents thus prepared, must be kept in a wide-mouth stoppered bottle, so as to preserve their antiseptic properties.

I may observe, in conclusion, that since the carbolic acid answers every purpose for which we require, I have not attempted the use of other disinfectants. But it would be difficult to find a substitute for it, since many of the disinfecting agents act as astringents—whereas we need the very opposite quality. And the carbolic acid unites perfectly, and in any quantity, with the cocoa-butter, and does not in any way interfere with the dilating properties of the sponge.

The carbolic acid which I use is the pure, crystalline kind. It is readily melted by gentle heat, and mingles with the oil at once on being stirred into it while fluid. It may be convenient to some of the members of the Society if I state that Messrs. Bradley and Bourdas of Pont Street, Belgrave Square, undertake to supply medical men with these carbolized sponge tents at a very moderate price.

For placing the sponge tent *in situ*, I have constructed for myself, out of a common uterine sound, the simple and excellent instrument now shown to the Society. It is formed by merely soldering a small fillet of copper plate an inch from the sharpened end of the sound. The sponge tent is carried, its base resting on this plate, to its position, and by a touch of the forceps it is left behind, the sound coming away with great ease. The engraving represents this instrument, and shows also a slender steel wire running down its shank. This wire is for detaching the tent after it has been introduced into the cervix. By pushing it upwards it lifts the tent off the end of the sound, and the instrument is then withdrawn. This arrangement is handier in use than the forceps. Messrs. Mayer and Meltzer, of Great Portland Street, make this instrument.—*Obstetrical Transactions, Vol. IX.*, 1868, *p.* 121.

96.—ON A PLAN FOR DEODORIZING SPONGE TENTS.

By Dr. J. H. Aveling, Senior Medical Officer to the Sheffield Hospital for Women.

[Dr. Aveling has been working at the same object as Mr. Ellis, whose paper precedes this, but has accomplished it in a different way.]

I have been trying permanganate of potash. Messrs. Cubley and Preston, druggists, of Sheffield, have prepared deodorized sponge tents for me in the following way:—

A conical piece of solid Turkey sponge is transfixed in its long axis by a silvered knitting-needle, and tightly bound round with twine. When dry, the needle is withdrawn, leaving a tubular opening throughout the whole length of the tent. After the tent has been filed into shape, and rubbed

smooth with sand-paper, its smaller end is sealed with a drop of melted white wax, and it is then ready to be filled. This is a delicate operation. The permanganate of potash must be very finely powdered, and dropped into the tent in extremely small quantities, and shaken down carefully, or the tube becomes clogged. When full, it is closed with another drop of wax, and having passed a small thread of silk through the end of it, the tent is ready for use. I have had them made of various sizes, and numbered according to their lengths as follows:—

Number	1.	2.	3.	4.	5.	6.	7.
Length	1 in.	1¼ in.	1½ in.	1¾ in.	2 in.	2¼ in.	2½ in.

This plan of numbering sponge tents according to their dimensions may be useful both in ordering the size of tent you require, and in describing how large a tent has been used in the treatment of a case.

I will merely add that the deodorization takes place in the following manner:

Until the tent has dilated to its very centre, the charge of permanganate (about two grains) remains undisturbed. It then gradually dissolves, and in solution renders the tent so perfectly sweet that upon its removal no unpleasant odour can be detected.—*Obstetrical Transactions, Vol. IX.*, 1868, *p.* 267.

97.—NOTES OF AN INTERESTING CASE OF MIDWIFERY.

By Dr. W. H. Taylor, Anerley.

On the 22nd of April, 1865, I was called to attend a woman, aged 29, in labour with her first child. Finding the pains were very slight I left. I called again in the afternoon, and made an examination, but could not feel the os. I began to suspect there would be some difficulty in the case, but, as the pains were about the same, I went away, telling the nurse to send for me as soon as they became stronger. About 8 p. m. I was summoned, and now found the pains were all the right sort. I made an examination, but could neither detect any presentation, nor find the os. I felt about for some time, and at last detected a very small indentation (not an opening) about the size of a pea. This I considered must have formely been the os, and that I had a case of occlusion of it. I also found the pubes projecting very much inwards, making the antero-post diameter very narrow, and altogether rendering it a case in which I did not feel justified in acting without a second opinion. I therefore sent for my friend, Dr. Stilwell, of Beckenham. He came, and agreed with me as to the nature of it. We also came to the conclusion that nothing remained but to make an artificial os, and overcome the pubic projection by forceps or craniotomy. I informed the husband of the difficulties of the case; he suggested further advice from town, but I told him the confidence we had in each other by reason of our having acted together in several most difficult cases which had occurred in our respective practices, made us feel quite equal to any emergency, and that we declined the assistance of a physician-accoucheur from London.

First, to make an os, I passed one of the blades of a pair of long scissors through the indentation in the uterus, and made several notches around it, which enabled me to introduce the point of the forefinger into the uterus. We then waited to see the effect of the pains. It did not dilate much. Then, by degrees, I introduced the middle finger, and by stretching the edges, somewhat increased the opening. After three hours' work I managed to get two fingers in, and could feel the head presenting. After manipulating for another two hours the opening was sufficiently dilated to render it advisable to rupture the membranes. This being done, the head, after a time, came down as low as the pubic projection would admit; but here all further progress was stopped. We then applied the forceps, but there was not room enough, and we had to give it up. There was now no help for it but to perforate the cranium. This we did, and after great difficulty succeeded in delivering the woman. There being some placental adhesions, we broke them

down, and removed the placenta; and heartily glad we were to complete the case. It was now 8 a. m., and we were somewhat fatigued with our anxious night's work.

The most gratifying part of the case was that the patient recovered without a single bad symptom, would insist upon getting up on the seventh day, and was about as usual in a fortnight. Since then her health has been good, the catamenia regular and without pain, and she is now in the third month of pregnancy. She has left the neighbourhood, but will return to it to be under my care for her confinement, and I hope, by inducing labour before the full period, to deliver her of a living child.—*Lancet, Jan.* 11, 1868, *p.* 41.

98.—ON GRANULAR INFLAMMATION OF THE CERVICAL CANAL OF THE UTERUS.

By Dr. Fleetwood Churchill, President of the King and Queen's College of Physicians in Ireland.

[Granular ophthalmia is the best illustration of granular inflammation of the cervix which can be given. When examined carefully by sunlight, the lining membrane is generally found bright red, or, if much congested, of a purplish red colour; and we can distinguish small granulations scattered over or covering the surface.]

Sometimes we find a granular condition of the cervix, with or without erosion. I have generally observed more or less vaginitis; it may be general or in patches; but I have rarely seen the vagina perfectly free from inflammation. I am not prepared to say whether this inflamed condition of the cervical canal is always a consequence or continuation of inflammation of the mucous membrane of the vagina, or a portion of it; but I think it very probable. I have, at any rate, seen granular vaginitis along with the same affection in the cervical canal.

Another symptom is, I think, quite characteristic of this disease—viz., a wide open os uteri. Sometimes the finger may be passed as far as it can reach; in other cases only the point can be introduced; but, with very few exceptions indeed, it is much more patulous than natural. Now, in all these cases of patulous os uteri, if you carefully examine the mucous membrane you will find intense redness and granulation, except at one or two places, it may be, from which the mucous membrane has been stripped.

But you will naturally ask me, how we are to examine the inner surface of the canal. There are two means of doing so.

1. By the speculum with bright sunlight. The cervix uteri is always more voluminous than usual, and can hardly be embraced by the end of the speculum; but, when brought as centrally as possible, the pressure closes the wide os uteri, and it appears simply a prolonged slit. If one lip be separated from the other, by a slight hook—I use the one with which Mr. Spencer Wells raises the peritoneum in the operation of ovariotomy—we can quite easily see into the canal a quarter or half an inch; and, if so much appear inflamed and granular, we may fairly infer that the remaining portion is so likewise.

2. By the endoscope. I am indebted to my friend, Dr. Cruise, for my first view of the cavity of the uterus and for instruction how to use the instrument. He has added to my obligation to him by promising to attend here and give information as to its use to any one who may wish to understand it. I have since repeatedly used the instrument, and by it confirmed the diagnosis I had made from a sunlight examination of the lower portion of the canal.

In most cases the canal is open enough to admit of the tube being passed with great ease through it into the cavity of the uterus; in some cases it would admit a much larger tube. If, however, it should be too narrow, nothing is easier than to dilate it with a tangle tent or prepared sponge to any extent we please. Having passed the tube to the fundus uteri, sponged it out, and made our observations, we may turn its orifice to different portions of the cavity, and then, slowly withdrawing it, we can examine each success-

ive portion of the canal, and there will be no difficulty in distinguishing the inflamed from the healthy portions. The amount of information we obtain, or could obtain, through so small a tube (and I have no experience yet with the larger) is not great. I think the plate given by Dr. Cruise of the inflamed mucous membrane of the bladder gives a sufficiently correct idea of this granular inflammation of which I am speaking. Portions are bright or deep red, and rough; other parts are paler and smooth; except in the severe cases where we find no healthy portions of mucous membrane.

I have mentioned that I have seen a few cases in which the os uteri was not more open than usual, and in which I ascertained that granular inflammation existed. In such cases, I have observed that the introduction of the sound in the gentlest manner caused some bleeding, or, in other cases, gave actual pain. If, in such cases, the canal be dilated with a tangle tent, its granular condition may at once be ascertained; so that I regard these two symptoms—slight bleeding or pain when the sound is passed very gently—as conclusive. Reasoning *à priori* I think we should expect to find the canal narrowed or closed, as the ordinary result of granular inflammation; but the opposite is the case in the vast majority of instances. Nor am I able to offer any scientific explanation; all I know is the fact, that of this disease the most unvarying accompaniment is an os uteri unusually patulous.

I have been thus particular in describing the anatomico-pathological peculiarities of the disease, because, as one might expect, the symptoms (properly so-called) are less distinctive, but have much in common with other affections of the uterus. There may be, and generally is, the usual aching pain in the back, with, possibly, some feeling of weight or bearing down. The menstrual flow is almost always increased, sometimes excessive, or recurring at short intervals and from slight causes. I recollect only one case where the opposite extreme was observed. There is always more or less leucorrhœal discharge, thinner, and white or yellowish, if there be vaginitis; thick and tenacious when the disease is limited to the canal of the cervix. When the speculum is used, we may see this stringy discharge hanging out of the os uteri, and it is sometimes difficult to remove it. When the disease is of long standing, it interferes, more or less, with the general health, giving rise to languor, listlessness, loss of appetite, and imperfect digestion. In addition, it is not less potent than other disorders of the uterus in exciting reflex irritation, of which, perhaps, the most common is ovarian pain and irritability of the bladder; very distressing while they last, but not absolutely requiring any special treatment beyond a vaginal suppository of belladonna or morphia, and which disappear as the cure of the disease progresses.

Upon one uterine function, gestation, the influence of a severe attack of granular inflammation is apt to be disastrous. It does not always prevent conception; but, in a great number of cases it causes abortion. I have known a succession of miscarriages apparently result from this cause alone; and I think that, in all cases of the disease, no risk of impregnation should be incurred. It will be wise also, in any case of abortion, to examine into the condition of the canal after the lochial discharge has ceased.

Diagnosis.—After what I have already said, I need occupy very little time with the diagnosis. Our suspicions will probably be first excited, when making a digital examination, by finding the point of the finger pass more or less freely into the os uteri, and the inference from this will be turned into certainty by a sunlight or endoscope investigation. A little longer process will be necessary when the os is not patulous; the occurrence of pain when the wound is passed, or a drop of blood escaping afterwards, leave but little doubt, and that may be removed by dilating the os with tangle, and a subsequent careful examination.

Treatment.—The disease is curable enough, and the course of cure steady but slow. It must be for such cases that the different kinds of curette have been invented, to scrape off the granulations. This does not appear a very scientific procedure, and I am sure you can cure this disease as effectually *without it.* The endoscope affords us a very nice and precise mode of applying remedies to any part, either of the cavity of the uterus or the canal of the cervix, without the risk of doing more than we intend, so that we may

safely use stronger remedies than we dare in any other way. By this means, strong nitric acid, tincture of iodine, or lunar caustic, may be applied when we please, and to a greater or less surface as may be necessary, or if we wish to limit our treatment to the cervical canal, a smooth piece of deal may be dipped in the acid or the tincture, and freely applied. I have found it better to begin with a free application of strong nitric acid, so as to destroy the granular surface, and this may be repeated at intervals of three or four days, until we obtain a smooth surface. The vagina should be syringed with cold water, or camomile tea, once or twice a day. After this, I dip a small twist of cotton-wool in tincture of iodine, and apply it to the entire diseased surface, twice a week, continuing the syringing as before. As the cure progresses, the lining membrane becomes paler and smooth, and the os uteri closes somewhat, but it is not restored to its natural size for some time. I have found a strong astringent applied to the inner surface, in the way I have described, the best aid, and with this view I have used with benefit both the pernitrate and perchloride of iron, at the same time syringing the vagina with a strong astringent.

I should strongly advise "separation" until the treatment is finished, as conception might seriously interfere with our procedure,

There is a disease which I meet with occasionally, which may possibly have some relation to the foregoing, but of this I am not sure. I mean a vascular tumour at the os uteri, exactly like that we find at the meatus urinarius. It varies in size from a pin's head to a raspberry, and, unlike the small polypi found in the same situation, bleeds on the slightest touch. So far as I know, it gives rise to no symptoms but menorrhagia. It can seldom be recognised by the touch, because of its small size and softness, but it is easily detected by the speculum. The cure is simple—excision by scissors, or Dr. Braxton Hicks's small wire *écraseur*, or torsion by Wilde's snare, and subsequently repeated cauterisation.—*British Medical Journal, Jan.* 4, 1868, *p.* 2.

99—REPORT OF FORTY-ONE CASES OF UTERINE POLYPI.

By Dr. J. Braxton Hicks, F. R. S., Physician-Accoucheur to Guy's Hospital.

[In all the forty-one cases treated of in the following article, the polypi, which were of various forms and very different sizes, were all removed by the annealed wire-rope écraseur. No death occurred in the whole series, and the removal was not followed by hemorrhage, nor by symptoms which gave any anxiety except in two instances. One great point insisted on in all these cases, was the keeping of the patient in a state of quiescence for some time after the operation—generally at least a fortnight.]

Another point in the treatment of these and similar cases is the removal of secretions by warm vaginal douche twice at least a day. After these operations small clots of blood, or retained secretions, with perhaps, a fragment of dead tissue, are liable to set up decomposition rapidly, and the natural evacuation of them is hindered by the dorsal position of the patient. Hence a pint of warm water thrown up the vagina twice a day, with or without a little disinfectant, is very useful in preventing, to say the least, minor symptoms of irritation.

In the whole of these cases the polypus was removed by one method, namely, by "écrasement." And this mode of separation has an advantage over section by knife or scissors, independently of the less risk of bleeding —I mean that by the crushing process we have a line of condensed tissue, which doubtless forms a barrier to the ready absorption of unhealthy matter. This is the same, only mechanically induced, as is produced by the actual cautery (as by the galvano-cautery); but as there is in these cases but little advantage in the latter over the former, and as the application of the wire-rope écraseur is much easier, it seems to me that it is generally preferable.

So far as my own experience goes, I believe that the instrument which I described in a former volume of these Reports will be found the most uni-

versally applicable of any kind. Although I do not wish to insist upon this point with undue strength, because an instrument to which one may have become used is handled with more facility and adaptibility than any other, yet inasmuch as it has been tested, not only in these forty-one cases, but also in some twenty more, under my colleague, Dr. Oldham, in the hospital, and has always, with one exception of enormous fibroid, where the chain also would not do, been equal to the task of removal with ease and rapidity, I think I may, without undue confidence, make the above statement.

Under these circumstances it may be well to give a few practical remarks on polypi, and on the way I have adapted the instrument for their removal.

Polypi come before us practically under four conditions—

A. *External to the os uteri.*

1. When sufficiently small to be included in the speculum.

2. When a loop of the écraseur can, without much difficulty be passed round the polypus by the fingers or hand only.

3. When their size is so great that they occupy the vagina so completely that the hand cannot reach the pedicle.

B. *Within the os.*

4. When they are within the canal of the cervix, or within the body of the uterus.

The *first* class are easily manageable; they are readily seen, readily caught and snared, and easily severed. A medium-sized instrument, one with a rope of ten to fifteen strands, is sufficient; and the smallest kinds of all may be removed by the smallest instrument, with three strands to a rope. It is best not to underate the tenacity of the compressed tissue, and it will always be satisfactory to have more power than is really wanted.

As a rule, these smaller and generally more vascular polypi bleed most after division, partly because of the relatively greater amount of blood-vessels, partly because the lines of écrasement is narrower, and because the force required to crush is less; therefore it is well to have a styptic at hand to stop any bleeding, not that it is ever really much, but that the operation will be more complete to the appearance of bystanders; and the employment of a styptic to the base and surrounding parts is beneficial to get rid of abrasions, and the irritation caused by the presence of these growths.

Now, in the *second* class, we are obliged to dispense with the speculum, and trust to our hands only. We thus lose two advantages; the first, the assistance of our sight to quickly seize the growth; the second, the protection which the speculum gives to the vagina from the contact of the fingers and the instrument for removal.

But there are some cases in which, although from its small size the polypus would readily fall into the speculum, still the external parts may be so rigid and undilatable as to cause so much resistance to the speculum, that the instrument, and finger to guide, can be passed less painfully than the speculum. A few of the cases recited were treated in this way; they occurred, as may be surmised, in single women, somewhat advanced in years, where much hemorrhage had not occurred to lessen the tone of the system.

In the second class of cases, then, we may pass a noose of the écraseur slowly through the vulva, in such a manner as to give the least pain and compression; the tip of the fore, and, if possible, that of the middle finger also being placed just within the loop at the part opposite the eye of the instrument. These fingers should carry the loop to the back of the vagina, while the instrument should be carried towards the front. The loop can then readily be placed round the polypus by very simple adjustment. As soon as the loop has passed the equator, or greatest diameter, it is to be made smaller by pulling down the end, if still unfastened, or by screwing it down, if it is. By this process, the loop glides very easily to the base, the instrument, of course, being simultaneously pushed up, as far as possible (and *generally* this can be done more quickly than the description can be read); *and then* the ends having been properly secured, the screw can be worked,

and the pedicle crushed through. The rope requisite for this class of polypi should be formed of from twenty to thirty or forty strands. Better too strong than too weak. It will also be more readily introduced if it has a certain degree of stiffness.

The force used to crush the pedicle is sufficient to prevent hemorrhage; at least in all the cases I have had, no such contingency has troubled me. It would be well, however, to have the speculum and a styptic at hand in case of such an event occurring.

After the severance of the growth we have to remove it from the vagina. This is, generally speaking, easily done in this class of cases, by passing two fingers above it, and hooking it down; still, sometimes, in the larger sort, or where there is a contracted outlet, it is requisite to have a vulsellum forceps, or the whalebone fillet at hand.

The *third* class requires the largest sized instrument for their removal, not so much for its strength, perhaps, as for its length, because it is requisite the base should be reached, for we generally find that in this class the pedicle is by no means so very large. The growth has generally been for some time outside the uterus, which is pushed upwards, sometimes as far as the umbilicus, sitting as it were on the top of the much larger polypus, or pushed aside. To reach to the neck of these long growths requires a considerable length of shaft in the instrument. If we do not reach the base, the point at which the section is effected is sometimes so large as to make it impossible to divide it. This is especially the case with fibroid polypi; even the chain écraseur is unable to pass through them, the chain breaking, or remaining imbedded in the growth; but no such strain need in this class of cases be put on the instrument. We can get over the difficulty by passing the loop of rope past the equator of the polypus; then draw it smaller, pushing at the same time the instrument higher up the vagina; by this simultaneous movement the rope slips up to the origin of the growth.

The best mode to effect this movement is as follows: Take the écraseur, with the rope attached to the traversing hook, the other end free, and make a loop of a size about sufficient to pass round the polypus. Then take two slightly curved canulæ, and through each of them pass a small wire; when this appears at the top, pass the end round the rope and back again through the canula, till it appears again at the lower end; the two ends then to be fastened off. Each canula is to be placed on the loop in such a position that it is about the same distance from the other as from the shaft of the écraseur instrument. The loop is then passed into the vagina posteriorly, assisted by the canulæ as handles. When the loop is wholly within, the écraseur is also passed in, but upwards in front, the finger assisting to adjust it over the polypus. The canulæ are then pushed carefully upwards, till the root is reached. Then the écraseur is also pushed up in front, the free end of the rope at the same time being drawn down gently, so as to lessen the size of the loop. When the instrument is thus adjusted, fasten the free end of the rope off securely. Then remove the canulæ by unfastening the wires and pulling down one end of each. This pulls the wires off the rope; the canulæ are then pulled down. Upon turning the screw the pedicle will be cut through.

Some instruments have been made to work with an unlimited quantity of rope, but this is really unnecessary. It is very rare to find a polypus whose base is more than three inches across. This will give a circumference of nine inches. If we draw the rope tight before it is fastened off, we might deduct from this an inch, so that any instrument which can bring the hook down over eight inches is sufficiently long.

Formerly I advised the canulæ to be passed up together behind the growth, and then brought forwards, running their little noose along the rope which they thus carried round the neck, but I find the plan just recommended more simple and manageable. The curving of the canula is necessary in these large polypi, because it adapts itself to the ovoid of the polypus, without injuring the vagina (a male catheter cut off at the end, and stiffened by a stylet a slight degree shorter, would answer instead of a metal canula).

When we have separated these polypi, they require some degree of man-

agement to remove them from the vagina. A pair of midwifery forceps, the whalebone fillet, will generally succeed. The vulsellum forceps al will assist, according to circumstances, but they require much care in th use. On the removal of polypi of this class I have not seen any bleeding follow from écrasement.

The *fourth* class of polypi are those which give most trouble, and req most care, both during operation and after—I mean those which have escaped from the uterus. When they are of large size, they are gener attached rather high up towards the fundus uteri. It has been considered rule by some that as soon as a polypus appears through the os, or as soo it is possible anyhow to diagnose it, it should be removed. Doubtless this is an excellent rule so soon as the growth has become really a polypus; but supposing the base is still nearly as large as the equator when we proceed to remove it, we find either that we have to cut through a great width of tissue, rendering absorption of deleterious matter more probable, and leaving a portion of the growth behind to form a source of future trouble;—or that during écrasement we draw together, or pucker up the wall of the uterus, leaving a chance open for the supervention of immediate and future mischief.

The difficulty of removing these forms very easily in their imperfect state is, of course, greater than in the true prolapsus form, because of the trouble one has to keep the ligature on; but the danger is still greater than the difficulty of removing. Those that are really polypi are very easily removed (although the difficulty has been dwelt upon by some more than it need be), so soon as the os is large enough to permit their exit when severed, if the same plan of introducing the loop be adopted as above described, substituting for the vulva the os uteri. Sometimes the growth is attached to the side of the uterus; this will be found out by the sound, and would require that the canulæ should be carried up together to the fundus, separated and brought down across the neck of the growth. But the smaller sort, springing from the upper cervix, do not distend the cervix enough to permit the loop of rope to pass up between them and the walls of the cervix, and therefore give some trouble. This, however, is not very difficult to manage. Use a rather stiff rope; make a loop as nearly the size as possible; introduce the loop into the cervix in a slanting manner, so that the part of the loop most distant from the écraseur shall pass up between the os and the polypus behind. When about half has entered, pass the head of the écraseur within, telling the patient to bear down; this generally drives the growth into the loop, which then can be tightened. If the rope is very limp, it gives some trouble; in fact, this kind of case generally is the most trying for flexible wire.

Should this plan not answer, the loop might be carried up with one canula as above directed, or a single stout wire might be substituted for the flexible rope. However, I have always managed with the latter, although with some little trouble. Further enlargement of the cervix by the tent would give much greater facility for action.

For the severance of all tissues by écrasement it is very important to remember the excessive resistance which is given by the condensed tissue, particularly the less friable kinds, as areola tissue. The chain écraseur having angles succeeds in fraying out some fibres, by which gradually the tissue yields; with the single wire there is no such assistance, it acts simply by crushing, and this more particularly if both the sides come down together; when one side only comes down, there is a certain amount of torsion which lacerates and assists; this is increased by the alternate bringing down of each side. In the rope of many secondary strands we have an intermediate condition, the fraying power of which is much increased if the rope be made rather irregularly. I have found a further assistance by winding round it two small flattened wires of the same material (annealed steel), crossing their spiral. The edges of this wire, at so many points, produce sufficient fraying, and allow us to use the flexible rope in any position required.

These practical points having been attended to, I believe the kind of instrument with which these polypi have been removed will be found sufficient

in almost every case, and remarkably easy to apply. Some have complained that the rope breaks. Upon inquiry I have found that the rope has been put to work for which it was not intended, or that it was of so small a size that it was no wonder it gave way. Occasionally the annealing has been carried too far, so that the resistance of the wire to tension was reduced. A very common error in makers has been to make the eye of the instrument with sharp edges, as if on purpose to cut the rope; or they have left the hook with so sharp a bearing that it would certainly cut any rope. The eye, again, has been made looking upwards, whereas it ought to be set somewhat obliquely, so as to make the bending of the chain less acute, when the line of section is at right angles to the shaft.

In the cases here brought forward, as also in those of my colleagues, I have not seen the rope break more than once. The way to avoid it is to use a rope of as large a size as the case can be managed with; for I have found that the pedicles of the smaller polypi are as resisting as, and sometimes even more so than, those of the larger kinds.

There is one practical point which it is well to remember after the removal of these bleeding growths, particularly if the patient be at or approaching the climacteric; and this is, that on the bleeding ceasing there is not infrequently a condition of hyperæmia, which leads to headaches, flushed face, with general discomfort, and perhaps might cause more serious troubles. It would be best to place the patient on a light unstimulating dietary, with gentle laxatives, and, if necessary, now and then a sharper purgative.

[Dr. Hicks relates 42 cases of various kinds of polypi, which we have not room for.]—*Guy's Hospital Reports*, 1863, *p.* 129.

100.—ON POLYPUS UTERI.

By Dr. Robert Dyce, F.R.S.E., Professor of Midwifery, University of Aberdeen.

[The forty cases of polypus uteri which form the subject of the following article have all been sucessfully treated within the last few years. All the tumours were of either the purely fibrous or of the fibro-cellular variety, but by far the greater number were of the first class. The most ordinary size of them was that of an orange, but some were much larger, and others considerably smaller.]

The following table exhibits the various methods employed, and the number removed by each plan:—

Excision,	18
Excision with enucleation (double origin),	1
Ligature alone,	9
Ligature with excision,	3
Torsion,	4
Enucleation,	3
Ergot alone,	1
Ergot with sponge tents,	1
	40

The instruments used for excision were a strong vulsellum, and equally strong curved scissors, which cut principally from near the point, which latter is rounded. The scissors are ten inches long, and slightly bent; the vulsellum of the same size, but nearly straight. The distance from the handles to the joint, in both, is 7 inches.

For ligature, the double straight canula of Neisson, improved by Gooch and Levret, was used, and latterly an improvement was added, by which the ligature was passed around a winch, and thus more readily tightened; the material used was strong whip-cord. I once used wire, intending to cut off the tumour at once, but it did not succeed, although the process was shortened. The cord was tightened morning and evening, and the vagina

syringed at the same time with tepid water. Usually in from four to se— days the neck was cut through. I never but once saw any constitutio— symptoms follow, but the length of time required, and especially the care quisite to prevent injury by the patient impaling herself, and the conseq— necessity of keeping her on the side during the period of its application— duced me to substitute the scissors in all my future cases.

For torsion, the vulsellum was alone sufficient; and it is far preferab— two fingers, which some recommend, because once a sufficient hold is ob— tained, a few turns will readily separate the tumour.

For enucleation, a scratch with the finger-nail, has generally answered, and once begun, it is easily completed, provided the tumour is within reach. The rent made in the investing membrane while fixing the vulsellum, is another and ready spot for this purpose, but sometimes a slight snip with the scissors may be required. The vulsellum is here indispensable, both for dragging down the tumour as well as fixing it. This mode of operating is so speedy and so bloodless, that I always now first attempt enucleation; but in my experience, there are very few tumours so loosely invested as to admit of this mode of operating; still I think it should always be tried.

My first case was accidental. The tumour was very large, and partly intra-uterine. I excised what I believed to be the neck, as it allowed of its descent to some extent, but it was found to have another and broader attachment higher up within the uterus, within a bridge of strong fibrous tissue between the two attachments. With the first incision of this second attachment, my finger entered a vacuity formed by the tumour and its investing membrane; from this point the separation was cautiously made, and the mass was easily and quickly removed. This part of the tumour was fully larger than a turkey's egg.

The mode of using ergot will be best understood by a detail of the cases in which it was employed. The principle which first led me to employ it was, that, believing, as I do, that during every severe uterine pain in labour the circulation through the vessels of the uterus by which the placenta is supplied is entirely arrested, it occurred to me that if I could first create uterine contraction and then continue this contraction for a length of time *continuously*, I would arrest the circulation for such a length of time as to prevent the tumour receiving sufficient or any nourishment; that the consequence would be the death of some part of the tumour; and that, once it had begun, it would go on dying and decomposing, and thus eventually the whole mass be brought away. It is on this principle of continuous action that ergot destroys the life of the child in labour, for so long as there are intermissions of ease, I never saw danger from it, but so sure as the pains are constant and without any intermission the child will die, unless immediately born. It is from the same cause that, in impetuous labour, called "tetanic," where *no* ergot is given, children are lost, the too long continued action preventing circulation between fœtus and mother, and hence due arëation of the blood for its life and nourishment. This theory I acted on, and succeeded in removing from two patients a tumour of enormous size, in a very few days, which filled the whole uterus, and distended the abdomen to a size far larger than that of a woman at the full time of pregnancy.

I conclude with detailing a few cases illustrative of the various methods adopted.

Polypus Uteri removed by ligature, showing the danger of passing the ligature too high, thus including a portion of the uterus.—Elizabeth Ad—s, aged 42, unmarried, came under my care Nov. 10, 1853. Is exsanguined, weak, and unable to make the least exertion without fainting. Only a few months before her admission into the infirmary she was examined per vaginum.

On examination, the finger comes immediately in contact with a large round, smooth, and insensible tumour, protruding through the os uteri, which is very much dilated and thin. From the small vagina its attachment can with great difficulty be reached; but by the help of some relaxation, produced by chloroform, it was ascertained to be adherent below, about two inches within the cervix at its anterior aspect, but the extent of adhesion could not be

reached above—the tumour seeming to have no neck, and to spring from a broad base. On the 14th, a vulsellum was firmly fixed high up on the vaginal portion, and a strain made upon it, but without producing any sensible change. A ligature with Gooch's double canula was, with great difficulty, passed over the tumour. Immediately on its being tightened, she complained of more than usual pain, with a feeling of sinking and faintness; but as there had been so much difficulty in encircling the tumour, it was allowed to remain. She passed a restless night, with frequent attacks of faintness and sickness, with continued pain. The ligature, however, was tightened, the vagina syringed with tepid water, and an anodyne given. The tightening of the ligature daily, with the frequent washing the vagina to remove the putrid discharge, was continued for five days. The constitutional irritation, however, had now become so alarming, attended with exhaustion, loathing of food, great thirst, and rapid pulse, that, fearing a fatal result, the ligature was removed, and she was ordered strong beef tea, with brandy and wine. The following day, though still faint and low, she had not been sick, had less uneasiness, and had taken a good deal of the stimulants. The tumour was much diminished, and the discharge continued profuse. On the second day after, or seventh from the application of the ligature, while at stool, the remains of the tumour, as large as the closed fist, came away in a putrid state. From this time she gradually recovered, though her convalescence, was much retarded by an attack of diarrhœa.

This case affords an instructive lesson. In my anxiety to encircle the whole mass, which was sessile, I had included a portion of the uterine wall in the ligature, no doubt by the weight of the tumour inverting a portion of the uterus. This seemed evident from the severity of the pain, and feeling of faintness immediately induced when the ligature was tightened.

I should have removed it at once and fixed it lower down, as the success of the operation would still have been secured, without endangering the life of my patient. The late Dr. W. Hunter lost more than one patient by including a portion of the uterus in the ligature, which he ascertained by examination after death; and Gooch is very strong on this point, and expressly says that we are not to be anxious to include all the stalk, as he found it sufficient to get over the body of the tumour to insure the success of the operation; that the stalk will drop off in a few days, like the umbilical cord in a child, or be decomposed and pass away in the discharges. And I have found, in more than one instance, that even though there is no neck or stalk to the tumour, but growing from a broad base, that if the ligature can be made to include but the greater part of the tumour, the remaining portion will, in a few days, slough away, and a cure be effected. The next case illustrates this.

Large Polypus removed by Ligature, a portion of which had only been included in the Ligature.—Isabella W., an unmarried farm servant, aged 45, was admitted into the Infirmary, 24th March, 1856, with an enormous tumour distending the vagina, and pressing out the perineum, as in the last stage of labour. From its size, it was impossible to pass the finger over the tumour, or to reach the os uteri. Above the pubes a well-defined tumour was felt, and pressure on the vaginal tumour was communicated to that felt in the abdomen. She has been losing blood in more or less quantity for upwards of two years, and though for some time under treatment, had never been examined until a few days ago. The symptoms, both constitutional and mechanical, were very urgent and distressing. Seldom could any urine be passed without the use of a long flexible catheter, and the pushing up the tumour, nor could the rectum be emptied without injections. Two days after her admission, a ligature was passed around what was supposed to be the bulk of the tumour, with more ease than was anticipated, by Gooch's double canula; but on tightening it again the same evening, the ligature broke There had been no pain from the operation; no hemorrhage, but complete retention of urine, and some little manipulation was required to reach the bladder by the catheter. On renewing the strangulation two days after, considerably more difficulty was experienced, from the tumour having swollen,

consequent upon the first tying; it was not hence possible to include much of it in the ligature. The ligature was tightened morning and evening, and tepid water thrown up the vagina each time. On the third from this second attempt, the discharge became copious, brown, and fœtid proofs of its decomposition. From this time all uneasiness passed away, no longer required the catheter, and the bowels acted of themselves. On ninth day, finding by examination, that the os could be distinctly traced with the adherent tumour passing within the cervix, it was determined to excise what remained, but by slight traction of the canula, and some pinching by the finger, the whole came away.

The tumour was now greatly reduced in size, and much resembled an hour-glass in shape; the central narrow part, which was about equidistant from both ends, was tightly encircled by the ligature. The fœtid discharge continued for a few days; and she was discharged cured in the third week from her admission.

Polypus Uteri removed by Ligature and Excision.—Elizabeth T., unmarried, aged 47, housemaid, full habit of body, but extremely sallow in complexion from loss of blood, was admitted 7th March, 1854, with the ordinary history and symptoms of polypus uteri of two and a half years' duration. The tumour is of considerable size, round, smooth, and insensible, filling up the vagina, and descending to within an inch of the external orifice. The vagina is relaxed, hence the finger can be easily passed around the tumour, and its neck felt passing within the os and cervix, to the latter of which it is attached at its back part fully two inches within. On the 12th, chloroform was administered, which produced great relaxation of the vagina. The vulsellum, when fixed, brought the tumour still nearer the perineum, so that the ligature was easily passed over it and tightened; when, finding that the peduncle was small (not thicker than two fingers), it was at once excised above the ligature, and the mass brought away; not a table-spoonful of blood was lost. The tampon was used for twelve hours. The tumour was as large as an ordinary orange. No bad symptom followed, and she was discharged on the 20th, cured.

With the experience I now have, this tumour might have been at once excised. By the vulsellum it was brought quite to the perineum, thus enabling the neck to be entirely embraced by the finger.

Polypus Uteri removed by Torsion.—Two cases were in connexion with pregnancy, and no doubt both of them had been the cause of abortion.

Mrs. S., mother of four children, miscarried at the fourth month, with not more than the ordinary loss of blood. The discharge had ceased, and the patient was on the sofa on the fourth day. A few days after, while dressing, she was alarmed by rather a copious discharge of blood from the vagina. She got sick and faint, but without the slightest pain. By the time I saw her she had recovered, having taken some brandy, and applied cold cloths to the abdomen. On a careful examination, nothing could be detected. The os was open, but barely admitted the point of the finger; nor did the uterus appear larger than it ought at this period of its involution. She was therefore kept in bed, and directed to continue the wet applications, and to avoid stimulants and animal food. These means were adopted lest the cause of the hemorrhage should be the presence of another ovum in utero. It could be no part of the aborted ovum, because the whole mass, fœtus, placenta, and decidua came off "entire." For the next ten days the bloody discharge, though very moderate, was nearly continuous. Tinct. cannabis gtt. xv. every four hours was first tried, then ergot in 5 gr. doses every three hours. By these means occasional uneasiness, then regular pains in her back, were produced. On examining now, the os was found more open, so as easily to admit the finger, and a smooth something was felt pressing through it. A vulsellum was cautiously carried along the finger through the os, and the tumour grasped. Torsion was made, and a firm polypus, the size of a walnut, was removed in a few seconds: from that moment the hemorrhage ceased.

A second case occurred shortly after, in August 1858, under similar

circumstances. The patient, Mrs. W., had borne three children, and now had miscarried between the fifth and sixth month. The child cried, but soon died. The loss of blood was not more than usual, and had nearly ceased, but in a few days it returned with some violence, and, notwithstanding the ordinary means, it continued to recur at intervals for several weeks. For some reason she would not permit an examination until the exhaustion and prostration made it imperative. At this time I was asked to see her along with Dr. Ogston, whose patient she was. The os was found open, so as freely to admit the point of the finger, but nothing could at first be detected; at length, by firmly pushing down the uterus from the abdomen, something was felt to touch the tip of the finger. A vulsellum was cautiously guided within the uterus on to the tumour, and with difficulty grasped. Finding no pain produced, the instrument was more firmly pressed and twisted. In a few seconds the tumour was brought away; it was firm and cartilaginous, and fully equalled a walnut. The hemorrhage ceased from that moment, and in due time she menstruated.

In this case it required very strong pressure above the pubes to bring the tumour within reach of the finger, so as to be felt, and no less care in carrying the vulsellum so far within the uterus, over so small a tumour, as to avoid injuring the uterus itself. The safe method in all cases, whatever the size of the tumour, is to press the closed vulsellum or forceps (but I prefer the former) gently and steadily up on to the tumour, and allow the instrument to expand of itself as it embraces the tumour, and not to attempt to grasp or try its hold until a sufficiency is within the blades, which can be judged of by the expansion of the handle; because once fixed, there it should remain. If this is not attended to, from the limited space and the almost impossibility of liberating both blades to get another hold, it becomes very embarrassing. —*Edinburgh Medical Journal, Dec.*, 1867, *p.* 503.

101.—THE SPONTANEOUS ELIMINATION OF UTERINE TUMOURS.

By Dr. Alfred H. M'Clintock, late Master of the Lying-in Hospital, Dublin.

[To study any disease aright we should commence by studying the course which it takes when left to itself, and uninfluenced by the active interposition of art.]

The study of a large number of recorded histories, together with the results of my own clinical observation, lead me to believe that there are *five* different modes by which nature may effect the cure of a tumour of the womb. To reduce the number cannot be done, I think, without confounding cases essentially different, whilst to increase it beyond five would be devoid of any real utility. These five modes of elimination are as follows, viz.:—

1. Interstitial absorption.
2. Simple detachment, or separation.
3. Calcareous transformation, or petrifaction.
4. Sloughing, or disintegration.
5. Expulsion by uterine contractions.

I have arranged them in this order, believing that it represents—approximately at all events—the relative degree of risk attaching to each process, absorption being the least dangerous, expulsion the most so. Were the cases to be arranged according to frequency, the order would be somewhat different. *First* and most frequent would be the cases of sloughing; then the cases of detachment; after them the cases of expulsion; next those of calcareous degeneration; and lastly those of interstitial absorption. I do not pretend to affirm that this order is strictly correct, for it is not deduced from any minute statistical calculation; still I think it will be found pretty near to the truth.

1. The *absorption* of a solid uterine tumour, even with the aid of medicine and other agencies likely to favour the process, is considered by some an impos-

sibility. It is true, as a general rule, that heterologous formations are removable by absorption. But is it correct to regard fibrous tumour of the uterus as such? Upon this point I can cite no higher authority than that Virchow:—"We find for example," he says, "that the so extremely common form of uterine tumour, which has been designated fibrous or fibroid, has in every respect the same structure that the walls of the 'hypertrophied' uterus have, inasmuch as it consists not only of fibrous connective tissue and vessels, but also of muscular fibre cells. The tumour may, as is well known, become so large as not only to embarrass the uterus in all its functions in an extreme degree, but also to exercise, through pressure, the most injurious influence upon the neighbouring parts. In spite of this, it must always be considered an homologous structure."—*Cellular Pathology*, p. 443.

But again, in cases where a fibrous tumour (or what was supposed to be such) was absorbed, the correctness of the diagnosis has been disputed, and it is urged that the tumour was only an inflammatory or œdematous swelling, or a chronic hypertrophy, or some tumefaction whose removal we know to be quite probable. Objections of this kind may no doubt be advanced, and the possibility of an error of diagnosis in any particular instance, can very seldom be completely excluded. Still there remains an amount of evidence on this question, from which we cannot escape drawing the conclusion, that nature is capable, on some rare occasions, of entirely removing a solid—it is to be presumed a fibrous—growth by the process of interstitial absorption. This position derives corroboration from the cases—and they are not a few—in which the same result was brought about by the use of therapeutic agents. One such case occurred to myself, and has been published. I do not here refer merely to cases—common enough—in which the tumour undergoes an apparent reduction of its bulk after treatment, menstruation, or delivery, and which reduction is to be explained by the removal of congestion or œdema. I am speaking now of the actual absorption of the growth itself.

A very striking example of the complete removal of a fibrous tumour by absorption is recorded by Dr. Matthews Duncan, and what enhances the value of this case (besides the thorough competency of the observer) is Dr. Duncan's own acknowledgment that he has been, as it were, forced against his judgment, by the evidence of a single case, to admit the possibility of the complete removal of a large fibrous tumour by absorption. He writes:—"The tumour was as large as the fœtal head at the end of pregnancy. It was as easily and as perfectly diagnosed as any case could be. There was no doubt ever thrown upon the nature of the case by any of the experienced practitioners who examined it. It had every character and symptom of a fibrous tumour. The patient was long in the most aggravated state of anæmia. *Now*, there is as certainly no uterine tumour, as there was certainly one formerly. The only method of escape for me, in the evidence of this case in favour of complete absorption, is the supposition that the tumour may have become spontaneously enucleated, separated and discharged, without the consciousness of the patient. This alternative, I confess, considering the cleanly habits and truthful character of my patient, seems more unlikely than the other."

Dr. Routh tells us in his Lettsomian Lectures that he "has met with at least two distinct cases of large fibroid, which, he should say, filled the pelvis, and materially interfered with the functions in that cavity, where the tumours have gradually diminished to the size of small apples." (p. 38.)

It is worthy of notice, and Dr. Routh pointedly alludes to the fact, that in both these instances, as well as in others, the atrophy of the tumour took place contemporaneously with the change of life, which would seem to imply that it resulted from arrested nutrition.

Whether the structure of the tumour undergoes any degree of softening preparatory to its removal by the absorbents, it is impossible to say. Dr. *West thinks* that it does, and a case recorded by the late Dr. Rigby rather

supports this view, as he states, in the course of its history, that the tumour became *softer* and smaller.

The processes of transformation and absorption going forward with such activity in the uterus after parturition may, perhaps, be extended to a growth standing in close vital relation to the organ, and thus lead to its partial or complete dispersion. Although parturition is likely to produce in the tumour other effects than its simple absorption, still experience shows that this latter is an occasional consequence, and the most fortunate one for the patient. "If," says Scanzoni, "we consider the great vascularity, the hyperæmia, the infiltration, and the softening which these tumours present during gestation, it will be seen that if there is any time at which the conditions are favourable to absorption it surely is the puerperal state" (p. 237 of American edition). The same high authority reports a case where "a fibrous body of the size of a man's hand, the diagnosis of which was perfectly sure, disappeared during confinement, in a manner so complete that six weeks after parturition we could no longer discern a trace of this tumour, which had existed for eleven years." (*op. cit.*)

2. *Detachment.*—From the meaning of the term detachment (or severance), it is obvious that only such tumours as are pediculated—polypi in fact—can be the subjects of this mode of elimination; but it matters not whether they spring from the mucous or the serous surface of the womb. There are three ways in which this separation may be effected, and numerous examples of each could be adduced. First, the pedicle may give way from simple atrophy and attenuation; second, when the pedicle is small it may break or snap asunder in the act of extrusion from the uterine cavity, or from the weight of the suspended tumour; and lastly, the pedicle may be destroyed through the constriction exerted upon it by the os uteri or ostium vaginæ. If the detached tumour remain in the genital canal, it will, of course, speedily decompose and become putrid.

3. *Calcification.*—A much rarer event than either of those we have been considering, is the transformation of a fibrous tumour into a calcareous mass which ceases to grow, and may be partly or entirely discharged. It is a degeneration from animal to inorganic substance. The propriety of regarding this change as a mode of elimination may be disputed. The tumour certainly remains in most cases, but it ceases to hold a vital connexion with the economy, and the symptoms which it had previously produced abate or altogether disappear.

In a very few instances the structure of these calcified or petrified fibroids has a near resemblance to bone, but such cases are quite exceptional. In the vast majority no true bone is formed, but earthy particles, chiefly salts of lime, "are deposited and accumulated in the organic basis of the tumour, so as to make a mass which is sometimes friable and porous, sometimes hard and dense as marble" (Simon). By what process this remarkable conversion or transformation is brought about we cannot tell. Inflammatory action would seem to have no part in it. The appearance of the tumour and of its nidus indicates this. Nay, more, guided by the analogy of the cretaceous transformation of tubercle, and of the so-called ossification of arteries in the aged, we should rather regard it as the result of defective nutrition. "This much at least can, with certainty, be affirmed (writes Virchow, p. 365), that we are as yet acquainted with no stage in these changes which is at all akin to inflammation." Although we must confess our ignorance of the precise way or mode by which this calcification, or, to use the term proposed by Virchow, "petrifaction," of a fibroid is produced, still experience has shown us the circumstances and conditions which favour its production. It is chiefly met with in women advanced in life. It is confined nearly altogether to subperitoneal, or extramural fibroids; and generally to such as are pediculated, but not invariably so, as Dr. Turner would lead us to suppose. In the Museum of the Royal College of Surgeons (Ireland) is a very fine preparation, presented by the late Sir Philip Crampton, which strikingly exemplifies these points. The patient was an elderly woman, who married late in life, and never bore children. The disease was supposed dur-

ing life to be ovarian dropsy, and did not appear to be the cause of death. The uterus contains several fibroids, the largest which is about the size of a fœtal head, and has the hardness and density of bone. This tumour lies almost immediately under the peritoneum, but is still imbedded in the uterus, and not in the slightest degree pediculated. In direct apposition with it is a large interstitial fibrous tumour, which contains merely a trace of calcareous deposit.

Again, we find that this process of petrifaction may be limited to the surface of the tumour, or pervade its structure. It "has no necessary connexion with the size of the tumour, and is not commensurate with its growth; but it appears (says Dr. Turner) to be in intimate relation to the size of the peduncle, and to the changes which take place, by obliteration or atrophy, of the vessels which pass from the substance of the uterus through the peduncle to the tumour."

A tumour that has undergone this calcareous transformation ceases to be productive of symptoms, except such as may arise from its mechanical influence. On rare occasions the calcified tumour has fallen from its nidus, and been discharged *per vaginam*.

It does not always prove quite so innocuous, however. Cases have been reported in which these petrifactions, by infringing on a neighbouring organ, have caused ulceration and other ill effects. Dr. Turner cites a case where a large calcified tumour, growing from the back of the uterus, caused death by compression and rupture of a fold of the ileum, consequent upon a fall on the pavement.

Dr. Matthews Duncan exhibited, at the Edinburgh Obstetric Society, a calcified fibrous tumour of the uterus, from a patient who had died of peritonitis, and two holes were found in the peritoneum, apparently made by the tilting upwards of two thick scales of the tumour.

Through the kindness of Dr. Fleming, I once had an opportunity of seeing a most singular—indeed, I may say, unique case, in which a calcified tumour, springing from the anterior wall of the uterus, had made its way by ulceration into the bladder, producing all the symptoms of vesical calculus in their most intensely aggravated form. Some fragments of the tumour had been detached, and were found in the cavity of the bladder.

4. *Disintegration or Sloughing* is another change that may take place in a uterine tumour, and lead to its partial or complete extirpation. This process generally gives rise to symptoms of a grave kind, and in many instances the patient has actually sunk under the wasting effects of the discharge and the constitutional irritation. Suppuration of fibrous tumours is described by some authors, but I see no necessity for making a separate category of such cases. In many of them the discharge was plainly due to softening of the tumour and irritation of the uterine cavity.

The vitality of the tumour having been destroyed—how, we shall presently inquire—it may come away *en masse*, or in broken, irregular fragments, quite putrid, at intervals of days, weeks, or months. All this while there is little or no pain, but a constant puriform discharge of a highly offensive character, and sometimes mixed with blood, is going on. In a few instances the tumour is too dense to melt away, and too large to be expelled spontaneously. Here art must interpose, or the life of the patient will inevitably be lost. Dr. Hall Davis met with two cases in the Middlesex Hospital, which strikingly exemplify the good effects of this timely interference. Very alarming symptoms, viz., fever, delirium, vomiting, &c., were present in each case; but the lives of both patients were saved by dilating the os, and removing the putrid *debris* of the tumours. I may here venture on observing that it was the study of such cases as these that led me soon after its first announcement, to form an unfavourable opinion of the operation of gouging uterine fibroids.

5. *Expulsion*.—The fifth and last mode of elimination I term expulsion, under which title are comprehended all those cases where uterine contractions—parturient efforts, in fact—form the leading symptom as well as the efficient cause of the separation of the tumour, which is still in vital con-

nexion with the womb when the contractions commence. In the group of cases last considered, expulsive pains were very rarely present, and when they did occur, it was *in consequence of the death of the tumour*, which then acted as a foreign body, and excited parturient efforts on the part of the womb.

As the uterus is endowed with very great contractile power, we might, *a priori*, expect solid growths to be got rid of occasionally by the exercise of this faculty. Experience amply confirms this, but at the same time teaches us what we otherwise should not be quite so prepared for, namely, that the danger arising from this mode of elimination is greater, perhaps, than from any of the preceding.

A good many cases are reported where this species of parturition has been set up, and the tumour has thereby been partially or entirely removed. A large proportion of these cases occurred in the puerperal state, which may go far to account for the large mortality amongst them. But setting aside the puerperal cases, the danger would seem to be greater from *expulsion* than from *sloughing*. This more unfavourable result admits, perhaps, of some explanation. The expulsive process is the more acute: it is often accompanied by prolonged pains of a very severe kind; and lastly, the violent contractions of the diseased organ must powerfully tend to excite inflammation, or other morbid action in it.—*Dublin Quarterly Journal, Feb.* 1868, *p.* 20.

102.—ON DR. ATLEE'S PRACTICE IN PHILADELPHIA, U. S., IN CASES OF FIBROUS TUMOUR OF THE UTERUS.

By T. Spencer Wells, Esq., London.

[The following is taken from an article entitled "Notes of an Autumn Holiday in America." Dr. Atlee is very celebrated for his success in ovariotomy, and is now preparing a work on the subject.]

His cases of fibroid tumour of the uterus will not be less interesting than his cases of ovariotomy. In his early practice, he had, in common with myself and others, excised large fibroid tumours, believing them to be ovarian; but had, after more extensive experience, become convinced that they were uterine. The results, however, had not encouraged him to repeat such operations, unless the patient was in imminent danger and not to be saved otherwise. He has also become much less willing than formerly to incise these tumours by the vagina. He has had some cases in which the combined use of ergot and incision has been followed by the death and expulsion of large tumours or of their disintegrated remnants, and by the complete recovery of the patient; but the risk of uterine phlebitis and pyæmic fever has proved to be so great, that he has become gradually less willing to interfere unless driven to do so by hemorrhage or some other condition endangering life. And he has become more hopeful of these tumours becoming indolent as age advances, and at length undergoing a process of atrophy or spontaneous cure. As I had been led to entertain similar opinions after a much smaller experience than that of Dr. Atlee, I heard this with no common interest. As an internal remedy, he gives muriate of ammonia in ten-grain doses two or three times a day, for many weeks or months consecutively; and thinks he has seen more benefit follow its use, than after iodine, bromine, mercury, or any other medicine which he had tried. He promised to try the chloride of calcium, in the doses which have appeared to me to be so useful, by leading to atheroma or calcification of the nutrient vessels of these growths.—*British Medical Journal, Jan.* 18, 1868, *p.* 49.

103.—CANCER OF THE UTERUS.

[The following is from a report on the practice of Dr. Atlee of Philadelphia, by Mr. Spencer Wells, who has been lately making a kind of medical tour in America.]

In the treatment of cancer of the uterus, both cancroid and carcinoma,

Dr. Atlee has great faith in the long continued use of arsenic internally, in small doses long continued, combined with the local use of a very strong solution of iodine in glycerine. A drachm of iodine and a drachm of iodide of potassium are dissolved in two drachms of glycerine, and this is applied by a brush or cotton two or three times a week all over the cervix uteri and to any part of the growth which can be seen. I saw one case of undoubted carcinoma where the patient and her husband (a medical man) fully confirmed all that Dr. Atlee said of the remarkable improvement which had followed this treatment in her case.—*British Med. Journal, Jan.* 18, 1868, *p.* 49.

104.—ON THE DIAGNOSIS OF TUMOURS OF THE BREAST.

By Thomas Bryant, Esq., Assistant Surgeon, Guy's Hospital.

What are the symptoms by which a cancer of the breast is to be made out? Cancer is essentially of a progressive character; it never remains long in one tissue, but sooner or later involves all with which it comes in contact; and it is through this pathological peculiarity that it produces symptoms in the skin over it, and parts beneath it, of great diagnostic importance.

How Cancer affects the Skin.—The skin is affected slowly, but in a very characteristic way. It first becomes more or less fixed to the growth; and, instead of rolling freely over its surface, as it invariably does in a simple tumour, it seems tied or drawn down to the parts beneath. A dimpling of the skin next makes its appearance; this symptom passing into a complete puckering, at which stage of the disease the integument will have become completely fixed and immoveable. As the disease progresses, the skin becomes infiltrated with the cancerous products, and is made a part of the diseased mass; and, as time progresses, so-called ulceration of the integument may make its appearance—this ulcerating surface always assuming a characteristic aspect. In some cases, the skin becomes infiltrated with isolated tubercles, varying in size from that of a millet-seed to tubercles of larger dimensions. When any of these conditions of skin exist with a tumour infiltrating the mammary gland wholly or in part, and more particularly the latter or tubercular form of infiltration, the cancerous nature of the disease is tolerably clear.

On the Mobility or Immobility of the Tumour.—As cancer affects the skin by infiltration, so practically it affects the parts beneath; and, as a consequence, we find symptoms produced of great value in a diagnostic point of view; for a cancerous tumour, instead of moving freely over the pectoral muscle, as it does in innocent affections and in health, soon becomes, as the disease progresses, somewhat fixed and at last immoveable; the breast gland, pectoral muscle, and probably skin, all becoming, by general infiltration, involved in the same destructive influence at the last stage of the disease.

Infiltration of the Absorbent Glands.—Should you find enlargement and induration of the axillary or clavicular glands associated with a doubtful tumour of the breast, you must regard such symptoms as strongly indicative of the disease being cancerous; for cancers rarely exist any lengthened period without causing such symptoms. It is true, that you do find glandular enlargements associated with inflammatory affections of the mamma; but, under such circumstances, the axillary glands are usually more tender on manipulation than they are in cancer, and the enlargement is more rapid; the cancerous infiltration, as a rule, appearing, in its early stage, as a painless chronic infiltration.

Neuralgic Pains down the Arms, &c.—This symptom is one of great value in determining the nature of a tumour of the breast; for it is rarely present in any other affection of that organ than the cancerous. It indicates the probability that the deep cervical or axillary glands are infiltrated with the disease, and are pressing on the nerves, causing pain, although these glands may not be felt. Indeed, neuralgic pains in any part of the body, associated

with cancerous disease, or after its removal, should be always regarded with suspicion; for they are often indications of secondary cancerous deposit having taken place either in the nervous centres or along the nerve's course.

Retraction of the Nipple.—I must again warn you not to place much confidence in this symptom as pathognomonic of cancer; for I have already told you that it frequently exists in other affections of the breast. It is a purely mechanical symptom, and rather reveals the manner in which the gland is involved in the disease, than the character of the affection; this opinion receiving support from the fact that the nipple may become retracted in the early stage of cancerous disease of the breast, and at a later stage reassume its prominent position.

Discharge from the Nipple.—In all cases of general disease of the breast-gland, inflammatory, cystic, or cancerous, there may be a discharge from the nipple; in the inflammatory, it may be watery or purulent; in the cancerous, it is generally sanguineous, and little in quantity; in the true cystic it is viscid, more or less blood-stained, but profuse. It does not exist in all cases of cancer, and is not typical of the disease. As a means of diagnosis in the true cystic disease, it is of great importance.

Cachexia.—I have no belief in the existence of a special cancerous cachexia. A cachexia may be present in cancer, as in any exhausting and wasting disease; but that of cancer differs in no single point from that of any other affection. When a cachexia exists, it indicates the presence of some affection which is undermining the patient's strength; it may be cancer, but it may be any other form of disease.

Cystic Tumours of the Breast.—Time will not allow me to enter fully into this important subject. You must rest satisfied, therefore, with the few remarks I feel bound to make. I must beg you to remember that cysts are accidental developments in the majority of tumours of the breast; that they may exist in the simple, as well as in the cancerous affections; and that, as a general rule, they may be disregarded in a diagnostic point of view—the diagnosis of tumours containing cysts, with one single exception, resting upon the same points as the diagnosis of tumours without cysts, to which your attention has been already drawn. That exception refers, however, to *genuine cystic disease of the breast-gland*—a disease of the gland-ducts. It is one of an innocent nature; it affects the patient solely through local influences. It is to be recognized by the gradual and generally painless growth of the tumour; by the evidently cystic nature of the disease from the beginning; by the frequent presence of several cystic centres in the same gland; by the healthy character of the integument over the tumour; by its free mobility upon the parts beneath; by the total absence of all indications of induration of the absorbent glands, and the general good health of the patient from the first; but, above all, it is characterised by the fact that a *free* discharge of a clear or blood-stained viscid secretion from the nipple is always present, and that this discharge can be readily increased by steady pressure upon the part; the cyst or cysts in many instances being completely emptied by these means. When the cysts are full, great pain is often experienced; when empty, the tumour is comparatively painless; this discharge of a viscid fluid from the nipple in such cases fairly determining the nature of the disease.

I have thus, gentlemen, gone over as carefully as time will allow the principal points by which the diagnosis of a case of tumour of the breast is to be determined; and I trust I have convinced you that the majority of the difficulties generally experienced in the diagnosis of such cases will be found to vanish by making a systematic use of our eyes, our ears, and hands, in the investigation of the subject. In certain cases, it is true, difficulties may be experienced; but, in the majority, a fair degree of certainty is to be attained by the adoption of such a systematic mode of investigation as I have explained and illustrated.

By way of a general summary of the whole subject, the following conclusions may be drawn up, contrasting the cancerous with the innocent affection.

1. *Cancer* generally attacks patients during the functional decline of the mammary gland—that is, after forty years of age; but *innocent* affections, as a rule, occur during the functional activity of the gland's life.

2. A *cancer* in the breast of a single woman is generally observed at earlier age than in that of a married woman, the functional activity of gland lasting longer in the latter case.

3. *Cancer* always infiltrates the gland, wholly or in part; but simple tumours, as a rule, seldom involve it to any great extent, with the exception of the true cystic disease.

4. *Cancer* generally affects the breast in a single centre, and from that rapidly developes. Innocent growths, cystic or otherwise, often appear as multiple tumours in connexion with one or both breasts.

5. *Cancer* generally runs its course within a period of three years; *but innocent* tumours may be developing for many years without causing much distress, affecting the patient only from local causes.

6. *Cancer* does not exist for any lengthened period without involving the skin by infiltration or ulceration; but, in *simple tumours* of the breast, the skin remains uninvolved to a late period of the disease—until, indeed, it is ruptured by over-distension.

7. *Cancerous tumours* soon become more or less fixed to the parts beneath, and immoveable; but *simple tumours* are, with rare exceptions, moveable throughout the whole period of their existence.

8. In *cancers*, the axillary absorbent glands become indurated at an early period of the disease; in *innocent tumours*, they are rarely affected.

9. Neuralgic pains down the side and arm are common in *cancer;* in innocent affections, they are rarely present.

10. Retraction of the nipple is found in both simple and malignant diseases of the breast, and consequently is of slight diagnostic value.

11. A discharge from the nipple exists in all affections of the true breast-gland. In cancer, it is sanguineous and scanty; in *true cystic disease*, it is viscid and abundant, and can be readily increased by pressure on the cysts; in *inflammatory*, it is watery or purulent.

12. In cases of tumour of the breast, when the skin has ruptured or become ulcerated, the margin of the opening or sore reveals the character of the disease; in *simple* cases, presenting the appearance of being ruptured, cut out, or punched; whilst in *cancerous* diseases there is no such distinct margin, the edges of the wound appearing thickened, indurated, and everted.—*British Medical Journal, Nov.* 9, 1867, p. 417.

105.—REMOVAL OF THE BREAST—EMPLOYMENT OF TORSION.

By EDWARD HAMILTON, Esq., one of the Surgeons to Steevens' Hospital, Dublin.

K. B., married, aged 34, has had two children; presented a florid complexion, and very healthy aspect; she complained of a tumour in the right breast of three months' duration; the nipple was retracted, hard, and wart-like; the surface of the skin for some distance was thickened, and of a dusky hue; she complained of occasional lancinating pain; there was no enlargement of the axillary glands; the case thus seemed a most favourable one for operation, which was accordingly performed in the usual way, removing a large segment of skin; the vessels, which were numerous and large, were caught and twisted with Syme's torsion forceps, an instrument which I had an opportunity of seeing, through the kindness of Professor Tyrrell, of the Mater Misericordiæ Hospital. In all the operations recently performed at Steevens' Hospital, this mode of arresting bleeding has been exclusively adopted; even in a case of amputation of the thigh last week, by Mr. Colles, the femoral trunk was the only vessel secured by ligature, all the others were *sealed in* the most expeditious and efficient manner by torsion, and in no *single instance* has there been the least appearance of secondary hemorrhage.

No pocket-case should be without this instrument, as almost superseding the ligature in the treatment of bleeding from recent wounds. The wound was sponged with chloride of zinc, after the plan of Mr. De Morgan, using a weak solution, a practice which my experience leads me to regard most favourably: it arrests the weeping from small blood-vessels, cleanses the wound from clots, and produces an instantaneous glaze, very conducive to immediate union; the tumour, on examination, presented all the characters of scirrhus cancer.—*Medical Press and Circular, April* 1, 1868, p. 280.

106.—ON FÆCAL FISTULA FOLLOWING OVARIOTOMY.

By T. Spencer Wells, Esq., Surgeon in Ordinary to Her Majesty's Household.

[The patient, whose case is related in the following article, was 57 years of age. She had been tapped three times, and had suffered from several attacks of circumscribed peritonitis. A large multilocular cyst of the left ovary was removed on the 10th of March, 1864. The pedicle was transfixed, each half tied separately, the whole enveloped by a third ligature, and the tied end, after separation of the tumour, was returned into the abdomen with the ligatures, the ends of which were cut off close to the knots.]

A portion of the cyst adhered so firmly in the left iliac fossa that it could not be separated, and it was left adherent, after transfixing and trying it, leaving the ends of the ligatures hanging out of the lower angle of the wound. The patient recovered, and went to Leeds on the 14th of April, five weeks after the operation, the ligatures still keeping the lower part of the wound open, and a little discharge daily escaping beside them.

[After some little trouble the ligature came away, the discharge gradually lessened, and the patient considered herself to be well. There was a very slight oozing of pus, however, from the lower part of the wound.]

She remained well during the winter and early spring, but in May, 1865, Mr. Teale wrote to tell me that for some weeks past there had been "at intervals a considerable increase of discharge from the sinus, attended with uneasiness, but not with severe pain. The odour of the discharge is offensive—not putrid, but faint or albuminous. I do not think there is any lodgment of matter. It seems to escape freely as it is secreted. Deep in the left iliac region is a general state of solidity of the parts, as contrasted with the opposite side."

It should be remembered that although the ligature which had been left hanging out through the wound in the abdominal wall had come away in May, 1864, there was no proof that the ligatures tied on the pedicle after transfixion, and cut off short, had come away. Mr. Teale thought they might be present, and keeping up irritation. He adds, "To day I examined the sinus with an elastic catheter, and at the depth of 4½ inches encountered a solid resistance. Having introduced a hollow elastic tube open at the end, I passed through it a fine wire stilet, hooked at the end, and tried to angle for the retained ligatures, but without success. These proceedings were conducted in the most gentle manner and did not cause the least distress." On the 25th of May, Mr. Teale again wrote "She has been slightly feverish and frequently troubled with diarrhœa, and I have not thought it right to attempt any mechanical treatment. The discharge is less in quantity, but I think it has shown a little fæcal tinge."

After this the discharge became more abundant and more decidely fæcal, varying in quantity from day to day. She often complained of a feeling of painful distension at the lower part of the abdomen. This was generally followed by a gush of acrid irritating discharge, and then by relief. The amount of fæcal matter in the discharge varied considerably. If the bowels were not relaxed, there was little or none. Latterly, however, as she became confined to bed, she had frequent attacks of diarrhœa, and then fluid fæces escaped in considerable quantity from the fistula. But no solid fæces

ever passed. She gradually became weaker, and she died Dec. 20, 1865, about 20 months after ovariotomy.

I am indebted to Mr. T. P. Teale, junr., for a report of the *post-mortem* examination. "The fistulous opening on the surface of the abdomen was large enough to admit the tip of the little finger. Within the abdomen it was so dilated as to admit a middle finger at least. On opening the abdomen we found the edge of the omentum adherent to the wall at the level of the wound, —a coil of small intestines sealing the wound above the fistula, which latter was at the lower extremity of the wound. The omentum and sub-peritoneal tissues were excessively loaded with adipose tissues. A small part of the small intestine, the sigmoid flexure, and the rectum were matted together around the fistula and the left corner of the uterus. Close to the left side of the uterus was a mass, almost spongy, and pediculated, which projected towards the rectum. In the centre of the mass was a large suppurating cavity which communicated with the fistula, and with the rectum, by two large openings. The cavity extended for some distance between the uterus and the rectum. It passed towards the right side behind the lower part of the uterus; downwards, by the side of the rectum; and forwards as far as the femoral ring. No trace of any ligature could be found. The right ovary was healthy. The liver was greatly enlarged and much altered by fatty degeneration."

I think this case tells against the practice of tying the pedicle of an ovarian tumour, whether the ends of the ligature are left hanging out between the lips of the wound, or are cut off short and returned. The formation of a sort of canal or sinus by the adhesion together of folds of omentum or coils of intestine, in such a manner as to enclose the ligature and shut it off from the general peritoneal cavity, occurs, I believe, very generally when the ends of the ligature are not cut off. If the patient recover, one might expect more or less obstruction of intestine to follow such adhesions; although I am not aware of any case where such obstruction has been actually proved. When the ends of the ligature are cut off and the pedicle returned, we know from repeated *post-mortem* examinations that a similar adhesion of neighbouring intestine takes place around the end of the pedicle; and that, in some cases, pus has been circumscribed in this manner—until at length it has found an outlet, either through the abdominal wall, the vagina, or intestine. The occasional observation of cases of this kind (when at distant intervals I have been driven to adopt one or other of the *intra*-peritoneal methods of dealing with the pedicle) has led me more and more to the conviction that the clamp, or some other *extra*-peritoneal method, is not only more successful as regards the immediate result of the operation, but even more so if we look to the subsequent health of the patient.

Patients who recover after the *extra*-peritoneal treatment of the pedicle, as a rule, soon regain and maintain perfect health. So do many of those who recover after the *intra*-peritoneal treatment. But some of them, sooner or later, suffer from chronic suppuration, hæmatocele, or fæcal fistula; or, perhaps without any definite local ailment, are many months before they become strong and well.—*Glasgow Medical Journal, Feb.*, 1868, *p.* 379.

107.—A NOTE ON MENORRHAGIA.

Case under the care of Dr. MURRAY at the Great Northern Hospital, London.

Troublesome cases of menorrhagia treated with success are always worthy of record, for there are few disorders more trying to the medical practitioner. The result of treatment in the following instance, which was lately under Dr. Murray's care at this hospital, was very favourable. The patient was a woman of highly scrofulous habit. She had had many children, and her uterus was large, heavy, and indurated, with the mucous membrane of its *interior* granular, and bleeding at the least touch. The loss of blood was *severe* at the periods, and continued during their intervals, though not to so

great an extent. She had, however, sustained so much loss, that complete amaurosis had occurred, and her health was otherwise most seriously damaged,—the nervous system being generally shattered.

Dr. Murray, after having tried plugging, and the various styptics usually employed, even injecting the uterus with a solution of iron, met with great success in adopting the course which was originally, we believe, suggested by Dr. Henry Bennett,—namely, plugging the uterus itself.

Three or four small pieces of cotton-wool were used, to each of which were fastened long pieces of silk for the purpose of withdrawal. These little plugs having been first dipped in a solution of the sesquichloride of iron, were carefully pushed through the speculum into the uterine cavity, by means of the sound, and allowed to remain *in situ* from twenty-four to forty-eight hours, the iron preventing any unpleasant consequences from decomposition. This treatment was pursued for about ten days, after which time menstruation took place in a normal manner, and there followed no hemorrhage of any kind.

Dr. Murray is disposed to think that the delay which often takes place before local remedies are resorted to in some cases of menorrhagia, is frequently attended with the most mischievous results. The timely application of various forms of styptics to the uterine membrane will often save the patient from the serious and sometimes irreparable consequences of a long-continued drain from the system.—*Lancet, Jan.* 4, 1868, *p.* 12.

ADDENDA.

108.—ON THE THERAPEUTIC ACTION OF ACONITE IN DILATED CONDITIONS OF THE BLOOD-VESSELS.

By Dr. Archibald Keith, Physician to the Royal Infirmary of Aberdeen.

[In accordance with the experiments of physiologists, the vasomotor system, when paralysed, is more susceptible of stimuli than in the natural condition, and in consequence only very small doses of medicines having an affinity for the affected organ are required to contract unduly dilated blood-vessels.]

One of the first medicines on which I experimented was *Aconite*, to the therapeutical application of which, in accordance with the foregoing theory, I shall devote this paper. I do not intend to touch upon all the remedial uses of this drug, but merely to show its great value in one morbid condition of the system, namely, the febrile state. Before doing so, however, it will be proper to notice some of the more prominent characteristics of aconite, as recognised by the latest authorities who have made it the subject of their observations.

In my last paper I stated that medicines, viewed in relation to their effect on the blood-vessels, might be divided into two classes—the first consisting of those which merely induce contraction without subsequent dilatation, the pure astringents, acting chemically on the tissues; and the second, much the larger class, comprising those whose primary action of contraction (stimulus of sympathetic) is followed, sooner or later, by a corresponding dilatation of the vessels (paralysis of sympathetic). Passing by the former for the present, we find that the second class, those possessing the afore-mentioned double property, may be subdivided into two sections:—1. The medicines whose primary stimulating action on the sympathetic (contracted vessels) is prolonged and decided, and whose influence must be partially or wholly removed before reaction takes place (dilated vessels and paralysis of sympathetic); 2. The medicines whose primary action of spasm is momentary, scarcely, if at all, perceptible, though intense, and is followed, during the continuance of their influence, by very manifest and decided dilatation of the blood-vessels (paralysis of sympathetic). Cold is an example or type of the one class, heat of the other. Perhaps the greater number of drugs belong to the latter class, manifesting considerable variety in the intensity and duration of their temporary stimulant action on the sympathetic, but resembling each other in the shortness and comparative imperceptibility of the stimulus. Aconite, on the other hand, is an example of the former class of medicines. The analogy between its poisonous action on the system and the effects of cold are very striking. The prolonged contraction of blood-vessels with the numbness characteristic of cold, when applied to the body, have their counterpart in the phenomena exhibited by aconite. Hence these two agents have hitherto occupied a place in the Materia Medica differing from most other remedies. I stated formerly that what were known as the physiological or toxic properties of medicines were in reality their secondary effects, being a reaction from a previous, often invisible, or, at least, unobserved state of spasm of the blood-vessels. In experimenting with drugs, observers had overlooked this primary condition, and had wholly concentrated their investigations upon the secondary condition of dilatation. The majority of agents belonging to the second class, mentioned a little time ago, their primary action is comparatively of short duration, and it is not surprising that

it should have escaped attention. With aconite and cold the case is reversed. The primary effect of these agents, that of contracting the blood-vessels and exciting the sympathetic, is so decided and prolonged that it has constituted their main feature in descriptions of their properties. Thus the recognised physiological properties of cold and aconite are due to their primary action, those of other remedies to their secondary. It is easy to see what confusion this has introduced into therapeutics. The double action of medicines has been entirely disregarded; in some the primary, in others, such as aconite, the secondary phenomena have been overlooked. Both these actions, however, must be taken into account before we can form a correct estimate of the therapeutical value or application of remedial agents.

The chief brunt of aconite falls on the nerves of the heart. When taken internally, although other organs of the body come under its influence, its main action is upon the centre of circulation. If we could suppose Dr. Richardson's ether spray directed on the heart, it would give a fair idea of the effect of aconite upon that organ. Its mode of operation seems to be to cause excitement of the cardiac plexus with consequent contraction of the blood-vessels supplying the tissue of the heart. There is a diminution of blood in, and a decrease of the vital properties of, the organ. Its pulsations become less frequent, and may fall so low as forty per minute. The surface of the body becomes cold; there are rigors, shiverings, cramps, denoting defective circulation in the system. The general condition of the patient in this state resembles the collapse of cholera. In fact, it is a true collapse, induced by a poison, as much as is that of cholera. The sympathetic system is in a state of excitement, and the blood-vessels in a state of contraction. If a moderate dose of the poison be taken, the symptoms induced are those of the chilly stage of a fever, a less advanced degree of collapse. These, then, are the recognised physiological effects of aconite on the circulation. The attention of observers seems to have been arrested at this stage. Any further phenomena, characteristic of the medicine, appear to have been totally overlooked, or, at least, considered unworthy of special notice. Whereas the truth is, that the subsequent manifestations of aconite on the circulation are equally important with, if not more so than, those just mentioned. In all cases of collapse, unless the influence be strong enough to extinguish life in that stage, there is of necessity a reaction to the opposite condition. So much is this recognised, that it is the practice in India to abstain from all interference with cases of cholera during the stage of collapse, and to wait for the reaction which is certain to occur in the majority of the patients. The chilly stage, also, of a fever is succeeded by the febrile stage. In like manner, the primary symptoms of aconite just detailed are followed by a corresponding reaction. In proportion to the stimulus exerted upon the cardiac nerves, more or less paralysis ensues. The blood-vessels of the heart are preternaturally dilated, more blood is sent to its tissue, and there is a consequent increase in its vital properties. The pulsations are increased in frequency and strength, the circulation is more active, and there is consequently a febrile condition present. Had our nomenclature been consistent, this aconite-fever should have been regarded as the physiological action of the drug, and brought it under the same category with other medicines whose secondary symptoms have been considered their proper physiological manifestation. If the duplicate phenomena, however, be admitted, the nomenclature is of little importance. Now, these febrile symptoms exhibited by aconite, though acknowledged by the profession, are nevertheless practically ignored. They are never mentioned as affording any indication of its therapeutical application. All our ideas of aconite have hitherto been confined to its primary action as much as they have been to the secondary action of other medicines. But, in possessing a double property, its use in disease is rendered more intelligible and accurate.

It is unnecessary, I think, to enter more minutely into the influence of aconite on the circulatory system. Its action is so well known, that any detailed account would be superfluous. As I intend to confine my remarks

to its remedial uses in the febrile state, it is equally unnecessary for me to enter upon its effects on other parts of the economy.

The secondary action of aconite being attended with dilatation of the cardiac blood-vessels and quickened circulation, it follows that, in accordance with the views formerly expressed, it should prove more or less remedial in conditions of the system where such dilatation existed—the dose, of course, being small enough to induce only the primary effects. The febrile state is that for which aconite is specially indicated. And herein is exemplified the advantage of recognising the double properties of medicines. Aconite, viewed solely as a depressor of the heart's action, that is to say, a stimulant of the cardiac plexus, has been strongly recommended by some physicians in various kinds of febrile disorder. But overlooking the secondary action of the drug, and guided only by its primary, they have prescribed it on the principle of the larger the dose the more powerful the effect. The experience of its use in this way is very diverse. Sometimes it has succeeded: but it has as often failed, and in many cases dangerous symptoms have occurred. Sir James Simpson says, "One medicine has of late been supposed to produce this sedative effect on the heart, without exerting any other appreciable therapeutic action. I allude to aconite, a strong tincture of which has been employed by various European surgeons in surgical fever; but it is difficult to regulate it in its effects, and in many persons it produces an almost dangerous and depressing effect even in very small doses." Aconite has therefore not come into general use as a febrifuge. But when the principles are applied, which I endeavoured to expound in my last paper, we obtain the true conception of its therapeutical application. The two following points may be noticed:—

1. In the first place, the secondary action of aconite being analogous to the febrile state, we infer that it will prove remedial in cases of simple uncomplicated fever only.

2. In the second place, the effect desired being to produce moderate contraction of the cardiac blood-vessels such as will not be followed by undue reaction, the dose of aconite must be correspondingly small; and on account of the extreme susceptibility of the paralyzed sympathetic to stimuli, care must be taken to regulate the dose accordingly.

The first proposition excludes aconite almost entirely from the domain of specific fevers (except, perhaps, rheumatic fever, of which I shall speak presently). I have used it extensively in typhus and enteric fevers, without the least advantage. Sometimes I found the pulse come down a little, but it invariably rose afterwards. In the majority of instances no impression whatever was produced on it. It is therefore vain to expect any good from the employment of aconite in such cases. I should think, also, that in other specific fevers, such as measles, scarlatina, small-pox or erysipelas, the same negative results would be obtained; but I have not tried it in these cases. Mr. Liston spoke highly of it in the last named disease, but his example does not seem to have been followed. Belladonna is so beneficial that I have not resorted to aconite when treating erysipelas. My experience of the latter medicine in the two continued fevers I have mentioned is decidedly unfavourable, and confirms the impression I had formed from its secondary action, that it would be useless in fevers depending on the presence of a specific poison in the blood. Then, again, even in inflammatory fever, for which it has been highly extolled, and with reason, no impression will be produced on the symptoms *if there be extensive local lesion*. In such a case, the medicine acting physiologically on the affected part is the remedy properly indicated, whether it be belladonna for meningitis, antimony for pneumonia, or cantharides for nephritis. Aconite may and does prove very serviceable in conjunction with these, but by itself it will probably fail. Its use must therefore be limited to simple fever without local complication.

Next, as to the dose. There can be no doubt that the conflicting testimony regarding aconite is in great measure due to the quantity prescribed. Forgetting the febrile reaction which takes place, the tendency among observers has been to use gradually increasing doses. Finding injurious effects pro-

duced, they went backwards: only, however, to a certain extent. In some cases even small doses were found capable of acting injuriously. The medicine was consequently abandoned altogether. It never seems to have struck any one that, by still further diminishing the dose, more accurate results would have been obtained. On the contrary, the idea has been taken up, as with many other medicines, that unless benefit were derived from the doses of the Pharmacopœia, it was contra-indicated. The old-fashioned phrase of "not agreeing with the patient." became the usual house of refuge. Now, I beg to call attention to the following fundamental axiom of therapeutics, which has never, so far as I am aware, been stated before. It is this:—*Medicines, whose recognised physiological action is in reality their secondary manifestation, when indicated by the pathology as proper remedies for a particular disease, are unsuitable for, and injurious in that disease, when given so as to develop the physiological symptoms.* We have a notorious example of this in quinine, which "disagrees" with so many people. The disagreement is merely the development of its secondary or physiological action. If it be the proper remedy for the patient's ailment, the dose ought to be reduced. If one grain disagree, less and less ought to be given till the proper quantity be found. Many a case of this kind have I met with, and many a case of dyspepsia have I cured with that very quinine which the patient fancied he could not take. What is called idiosyncrasy is, in nine cases out of ten, merely greater susceptibility to the action of medicines; and if the dose were regulated in accordance with the foregoing principles, these peculiar abnormities would be less frequent. In the case of aconite, the physiological action being primary, we are merely called upon to deal with an excess of that action as manifested in the experience of those who have used it. They find that, in febrile cases, even five minims of our tincture, or three minims of Fleming's, will often cause alarming symptoms. Such a dose would have little effect on a healthy subject. But in conformity with that morbid susceptibility which I have repeatedly insisted on, in the febrile condition it becomes excessive. Why, then, not diminish it? This is the very point where experimenters have failed. It seems singular that when they found five minims to depress the heart too much, they did not try four, three, two, or even one. If they had done so, the value of aconite would have been by this time universally acknowledged.

The preparation I used in my investigations was the common tincture of the British Pharmacopœia. Keeping in view the theory on which I was experimenting, I gave much less doses than the minimum of the prescribed standard. I found then, that to answer all the purposes for which aconite was indicated, it was not necessary to give more than one drop, nor less than a quarter of a drop. On three occasions I found even this latter quantity to act too powerfully. It may be satisfactory to give these cases, the symptoms being alike in each.

A gentleman, aged 30, who had a feverish catarrh, took, according to my prescription, one-quarter of a drop of tinct. of aconite, in water. In a few minutes faintness came on, the pulse became very feeble, and there was a feeling of loss of power in the left arm. Brandy was administered, and recovery gradually took place. This patient is liable to feverish colds, but he never takes more than one-eighth of a drop of aconite, which he finds invariably to act as a specific.

A lady, aged 28, in similar circumstances, took a quarter of a drop of the tincture. Soon afterwards her pulse fell very low, and this symptom was attended with powerlessness in the left arm.

A lady, aged 31, suffering from febrile catarrh, took tinct. aconiti gtt. ¼. In a few minutes similar symptoms came on as in the other two cases. On two succeeding occasions I prescribed the same quantity of aconite, with the same results.

With these exceptions, then, the remedial dose of aconite I have found to lie between the quarter and the full drop. It may be given in water simply every half-hour, every hour, or every two hours, according to circumstances —always, of course, upon the empty stomach. This quantity may, at first

sight, appear absurdly small, and to approximate the homœopathic infinitesimals. It is not, however, a great reduction from the minimum dose of the Pharmacopœia (5 minims), which is confessedly too energetic in many cases. There is a wide difference also between gtt. ¼ as a minimum dose, and the thousandth or millionth part of a drop given by Hahnemann, in accordance with his theory of potentization. I do not know the strength of the homœopathic preparations of aconite, nor the relation they bear to our tincture; but it is plain enough, from homœopathic publications, that the more intelligent and enlightened members of Hahnemann's school, are giving up the theory of their founder, and prescribing doses more consistent with reason, although still on the principle of *similia similibus curantur*. The spread of homœopathy is due to the powerful effect of small doses of such medicines as aconite. Separate it from infinitesimals, and there is little fault to find; but an illogical public reasons in this way:—"Aconite cut short my cold, *ergo*, Homœopathy is right." The more intelligent method is certainly to reason, as a gentleman did who, having been cured of a feverish cold in a few hours by some aconite given to him by a homœopathic friend, said to me:—"It convinced me that aconite was a very powerful medicine, but it did not convince me that homœopathy was true." Now, if the middle position were adopted, which I have indicated in these papers, the present homœopathic system would not have a leg to stand upon. The doses I recommend are what come short of reaction, whether it be 20 grs. of bromide of potassium, or one-fourth of a drop of tincture of aconite. The connexion between this, and infinitesimalism is certainly very slight.*

I come now briefly to indicate the applications of aconite as a remedy for febrile disorders, in so far as I have made it the subject of personal observation.

1. *Catarrhal Fever.*—We possess in aconite a medicine which quite supersedes the purgatives and antimonials with which patients are too often drenched, *usque ad nauseam*. By purgatives the dilatation of the cardiac blood-vessels, or paralysis of the cardiac plexus, is sought to be relieved, by creating dilatation of the abdominal blood-vessels, or paralysis of the solar plexus. With antimony it is the same; acting by preference on the stomach and lungs before the heart, it lowers the pulse only at the expense of the first named organ, and after a large dose. Aconite, on the other hand, acting directly on the heart, controls its force prior to any influence exerted on other organs, and therefore, in quantity insufficient to operate elsewhere, lowers the pulse without the weakening effect of purgatives, or the nausea and unpleasant consequences of antimony. It is scarcely possible to speak of it too highly as a remedy for febrile catarrh, in the doses recommended above. For this it acts like a specific. It will not cure the attendant coryza, or tonsillitis, or bronchitis; but a few oft-repeated doses will, almost to a certainty, bring down the pulse, rendering it softer and less frequent. Its operation is usually attended with more or less perspiration, and followed by the removal of the oppression and restlessness under which the patient laboured. In order to save time I have frequently administered the medicine myself, and have, before leaving the room, witnessed the good effects of a single dose. It is quite common for me to hear parties express themselves as greatly relieved a few minutes afterwards. I have frequently had occasion to use it myself, and it has always within ten minutes removed the distressing feelings. A second supply was never required. In the majority of cases three or four doses are sufficient. Indeed, if within twelve hours no improvement takes place, it is to be inferred that there is a specific poison at work; and hence aconite forms a diagnostic element. In not a few instances I

* I find that many of the enlightened and intelligent homœopathic practitioners are of the same mind. They are prepared to abandon Hahnemann's system, and to occupy a middle position such as I have indicated, did they receive encouragement from our side. Would it be too great a concession to give them that encouragement? We are fully aware that homœopathy contains an element of truth: shall we continue to reject that element merely because Hahnemann buried it in so much rubbish? *I cannot* believe so; and I think that the recognition of the double action of drugs might serve *as a basis on* which the two opposing schools could ultimately be reconciled—a consummation *devoutly to be wished.*

have predicted the outset of typhus or enteric fever from the failure of aconite to bring down the pulse.

It would be impossible, within the limits of a short paper, to give even an abstract of the numerous catarrhal cases treated in this way and cured. Their number is legion. I shall therefore give only a few as examples of the whole. They do not differ materially from each other, and it would be an unnecessary repetition to detail every one.

Case 1.—A. G., female, aged 50, had suffered from general restlessness and feverishness for several days. When I saw her, the pulse was 120, the skin hot, the tongue furred. She complained very much of "pain in her bones." I gave her at once gtt. ½ of tincture of aconite in water. In five minutes, before I left the room, she felt very much relieved, and expressed her astonishment at the rapid effect. Before morning (it was 8 p. m. when she first took the aconite), she was quite well, and able to be up.

Case 2.—H. M., female, aged 13, was seized with rigors and general coldness on a Sunday afternoon, followed in the evening by great feverishness, headache, and oppression. I prescribed gtt. ½ of tinct. aconiti every two hours, to be suspended if she felt better after one or two doses. She did not require to take it more than twice, and before morning she was well.

Case 3.—A. B., male aged 60, had smart coryza, with great restlessness and general oppression. He had been in a "burning fever" the whole of the previous night. He was ordered to take gtt. ½ of tinct. aconiti during the night, at short intervals. A few doses relieved the symptoms, but without perspiration, and in the morning he was well.

Case 4.—I was called at midnight to see a child, aged 2½ years, said to be attacked with croup. I found him in a strong fever, with flushed face, swelled tonsils, and great dyspnœa. The croupal symptoms had lasted for more than an hour, but the child had been feverish and unwell for several days. On the first appearance of the stridor and hoarseness the parents had very properly put the child into a warm bath, and applied a poultice to the chest. I ordered mustard to the throat, and gtt. ½ of tinct. aconiti every hour. I was so satisfied that this was more than laryngismus stridulus, that I told the parents to call me at 7 a. m., if the child was not better. I had some misgivings about not giving the usual antimonials, and determined that, should the aconite fail, as little time should be lost as possible. I was not sent for; and when I called in the morning I found the child quite well, free of stridor, and hoarseness, and fever. He was, in fact, amusing himself with his toys. I was told that the alarming symptoms had subsided about 4 a. m. Query. Would antimony or any other medicine cut short threatened croup so effectually as aconite did in this case?

Aconite is also very useful in the chill which precedes the fever. I have met with several cases of this kind. One, a young man, sat shivering over his fire for an hour before he took the medicine. In twenty minutes the shivering ceased, as also the malaise which accompanied it. I infer that the aconite prevented the usual reaction which would probably have taken place.

Notwithstanding what I have stated, aconite does sometimes fail, even in catarrhal fever. Success, however, is the rule; want of it the exception. The reason of the exception I have not been able to make out; but it ought to be regarded, perhaps, as a confirmation.

H. *Rheumatic Fever.*—Aconite has been recommended by many for acute rheumatism, but very conflicting accounts are given regarding it. By some it is extolled highly; by others as much depreciated. Lombard of Geneva, who used it extensively, gives the following conclusions:—

1. The alcoholic extract of aconitum napellus is endowed with a specific action against acute articular rheumatism.

2. It speedily dispels the pains and the swelling, and dissipates the effusions of synovia in the articulations affected by acute rheumatism.

3. The medicine does not act as a derivative on the intestinal canal or on the skin.

4. Given in large doses, it produces a strong stimulus on the brain, and appears to modify the circulation.

5. The alcoholic extract of aconite contains the active principles of the substance, at least as regards its anti-rheumatic properties.

Fleming and Neligan in our own country have followed Lombard in their appreciation of aconite in rheumatic fever. All these observers, however, used gradually increasing doses; and this is probably the reason why, as I explained a little ago, conflicting results have been obtained, preventing the general adoption of their practice. My experience of aconite in rheumatism is very favourable. The small doses are unattended with the risks of larger ones, and produce all their good effects. I always trust to aconite for the reduction of the pulse, and it seldom fails, although it does not operate so speedily or so effectually as in catarrhal fever. For there is undoubtedly a poison in the system, and its presence there must necessarily interfere with the free play of aconite on the circulation. But the rheumatic poison does not resist aconite so completely as other febrile miasmata, and consequently the remedial effect of the medicine is far from being antagonized. While the symptoms in catarrhal fever ought to be relieved within a few hours, twenty-four at the most, I am satisfied if, in rheumatic fever, the pulse come down in two or three days from the time of administering the aconite. The utmost I expect from this is, not the instant cure of the disease, as some have hoped, but the protection of the heart from complication, and, by the quietness of the circulation, giving full advantage to the excretory organs to eliminate the morbific element. This is all that aconite will do, and it is a great deal. It prevents us being driven by the entreaties of the patient to that most mischievous drug, opium, and so saves the sufferer weeks of misery. It shortens the duration of the fever, if it does not arrest it. It is an invaluable auxiliary to the alkaline and blister treatment, and by itself alone will often save the necessity of resorting to either. The following cases will serve as illustrations:—

Case 1.—J. F., aged 21, male, affected with acute rheumatism. Pulse 120, very strong and full; knees very much swollen, especially the left; ankles painful and red. Ordered gtt. ½ of tinct. aconiti every two hours. Next day the pulse had fallen to 80, and the swelling of the knees had almost entirely disappeared. This patient's illness lasted a week. He had been ill three days before I saw him; he was relieved of the more severe symptoms in twenty-four hours, and in two days more he was comparatively well. Cases of this kind are frequently met with, where aconite alone suffices to effect a cure.

Case 2.—E. A., female, aged 28, a servant, was admitted into the Infirmary on the 2nd of January. She had been ill for a week with rheumatic fever. When seen, all the joints of the limbs were swollen and painful, the two wrists and the right knee being most affected. Pulse 120, full and strong. No cardiac complication. She was ordered gtt. ½ of tinct. aconiti every two hours. The pulse fell at the end of two days, and her general distress was much alleviated, but the joints, especially the wrists, continued painful. The blister treatment was then adopted for the wrists and right knee with complete relief, and she rapidly recovered. The pulse never rose after it had been reduced. This case is a good illustration of the failure of aconite by itself to cure the disease, but that it is very beneficial in conjunction with other remedies. Perhaps if it had been prescribed at the outset of the fever, it might have been successful alone.

The next case is an example of the complete inefficacy of aconite in severe local complications.

Case 3.—A. R., female, aged 18, admitted to the Infirmary on 9th of January, suffering from rheumatic fever of several days duration. She had formerly had the same disease about two years ago, and was then under the care of Dr. Smith. The heart was affected at that time. On admission the *knees and* ankles were found most affected, and there was a double murmur *over the* aortic valve, and apparently a to-and-fro sound over the apex. A *few days* afterwards, pericarditis was fully developed, and ran its usual

course. From the first she took gtt. ¼ of tinct. aconiti every two hours, without the smallest impression on the pulse, and it was not till after smart blistering that it began to fall. This is the only case of the kind I have met with for some years, as very few of the patients suffering from rheumatic fever are attacked with cardiac complications under the improved treatment now in vogue. It was not a case where aconite might be expected to do good, and larger doses than what I gave might have proved too depressing.

In mild cases, then, of rheumatism, aconite will in most cases lower the pulse and very materially shorten the disease, without further medicinal agency. In more severe cases, while it will prove serviceable in controlling the heart's action, it will be necessary to give it in conjunction with the full alkaline or blister treatment. In carditis, it will probably prove ineffectual.

[In inflammatory fever aconite has no place when there is extensive local lesion. The medicine then indicated is the one acting physiologically on the affected part, such as ipecacuan and antimony in bronchitis and pneumonia. The true place of aconite is at the onset of local inflammation, or the congestive stage; it is then most beneficial. In the shock after severe labour. Dr. Reith always prescribes aconite, and can testify to its great value, not only as a febrifuge, but as an antagonist to the usual febrile reaction which then takes place.]—*Edinburgh Medical Journal, April*, 1868, *p.* 894.

109.- ON THE PHYSIOLOGICAL ACTION AND THERAPEUTICAL USES OF CONIUM.

By Dr. HARLEY, Assistant Physician to King's College Hospital.

Physiological Action.—Hemlock acts as a depressor of the muscular movements, but the effect is influenced by the state of the muscles, whether they are in action or at rest. If a vigorous adult man take five or six fluid drachms of the succus conii and then start on a long walk, in half or three-quarters of an hour he experiences a feeling of tiredness, especially in the knees and hamstrings. He may still continue to walk, feeling powerless, with some giddiness and feeling of heaviness over his eyes; in an hour the sense of fatigue has gone off, and in an another hour he is as active as ever. If five fluid drachms and a half be taken and he remain at rest, the eyes become first affected—the adjusting function is interfered with—then succeed drowsiness and dilatation of the pupils, then weakness of the legs; he becomes cold, pale, and tottering; the pulse is regular and of undiminished force and volume; there is a diminution of muscular power in every part of the muscular system, and almost paralysis of the hamstrings and levator palpebræ. These symptoms are at their maximum about two hours after taking the dose, and have quite disappeared in three hours and a half. The succus was administered to several persons in doses of from three fluid drachms to one fluid ounce with similar symptoms, but in doses short of three or four fluid drachms there is no appreciable symptom. The action is uniform and invariable in man and in all other animals. There is depression of the motor function of the third nerve, a lazy movement of the eyes, and sometimes strabismus, with imperfect adjustment of the refracting media of the eye. The effects are quite distinct from those of alcohol—the latter acts on the motor system only secondarily, and does not affect the action of conium. The full action of hemlock is asleep; it is to the corpora striata, the smaller nervous centres, and the whole of the motor tract, just what opium is to the brain—it tranquillises and renovates the whole muscular system. At first it appears to paralyse this, but it is really a tonic, for Dr. Harley administered conium in full doses for months, and found the condition of the body was improved. The action of conium is influenced by the muscular activity more than by the muscular power. The sedentary with abundance of strength are more affected than the delicate but active. A delicate child will often take as much as would reduce some strong men to a tottering condi-

tion. Hence the dose of conium must be proportioned to the degree of motor activity of the individual, whether child or adult; in fact, it might almost be said that by conium we may measure the bodily activity of the individual. Hemlock has no pure cerebral effects; the irritability of the spinal cord is diminished; there is no evidence of distinct interference with the sensory functions.

Mode of Action on the System.—In thirteen cases in which it was administered the secretion of urine was in no way altered. By acting on the mixed urine of three cases, both before and after taking the drug, with potash, there was in both mixtures a similar smell of conia; hence it is not eliminated by the kidneys, and could not be detected in the breath or fæces. It has no direct action on the sympathetic, but nutrition is improved. Pain in cancer is diminished, probably from relaxation of muscular fibres, as the division of neighbouring muscular fibres relieves irritable ulcers. A dose of conium which falls far short of physiological effects is of no more use than a dose of two grains of quinine in ague. He attributed the neglect of conium as useless, to the administration of too small doses.

Therapeutical Uses.—There was only time to refer to its beneficial action in the convulsive diseases of children. It is pre-eminently a children's remedy. He gave it to a child 1¾ years old suffering from laryngismus stridulus and convulsive cough on excitement; he began with twenty minims, then thirty minims, forty minims, and increased it up to two fluid drachms and a half. There was no appreciable effect till he took forty or fifty minims. There was one attack in the first five weeks (previously one in two weeks), and the child completely recovered. In another case there was a complete tetanic condition of the muscles of the feet, with hyperæsthesia, drawing in of thumbs, contraction of hamstrings, &c., in a child 2¾ years old; there were tetanic attacks every now and then, lasting from five minutes to several hours. It got better under the use of conium, but, having omitted it for a time, relapsed, but on resuming it completely recovered.—*Medical Times and Gazette, March* 21, 1868, *p.* 325.

110.—ON THE ACTION AND USES OF BELLADONNA.

By Dr. J. Harley, Assistant Physician, King's College Hospital.

[The following is an abstract of the second of the Gulstonian lectures delivered at the Royal College of Physicians.]

Dr. Harley administered physiological doses, short of toxical effect, to man, the horse, and the dog. The method usually adopted was subcutaneous injection, and the solution one of sulphate of atropia. To man this was administered in doses varying from $\frac{1}{100}$th to $\frac{1}{30}$th of a grain. The symptoms were more or less delayed and powerful according to the dose, but the sum of its action seemed to be as follows:—After a few minutes there was a rather sudden acceleration of the pulse from twenty beats to double its previous frequency: its volume and force were also augmented; the temperature was increased about 1° Fahr. externally and slightly internally, and there was heaving of the carotids, also giddiness, heaviness or somnolency, nervous delirium and startings; dryness of the tongue, palate, and throat, with huskiness of voice; a dry brown fur on the tongue, which was usually moist at the tip and edges; gradual and increasing dilatation of the pupils. In two or three hours the dryness of throat and tongue gives way, and there is a very acid viscid secretion, of a fishy odour; then the pulse falls, but the pupils are in their highest state of dilatation. There is no effect on the respiration; occasional sighs and prolonged yawns. After the pulse assumes its ordinary rate the giddiness passes off, and he seems the *same as* before; but he feels languid, and there is dulness of mind and dimi*nution of* vision. Headache during these symptoms or afterwards is rare *and exceptional.* Desire for food soon returns, but during the action of the deglutition is impossible. The same accelerating effect

on the pulse was observed on giving it by the alimentary canal, and with succus belladonnæ there were exactly the same symptoms only altered in rapidity.

Conditions interfering with its Action.—1. Children are remarkably insusceptible, and occasionally bear very large doses before cerebral symptoms and dryness of throat are produced. But dilatation of pupil occurs as rapidly in the young as in the old. 2. Among adults some are more susceptible than others; it is doubtful whether pregnancy does not fortify against it. 3. Fixed caustic alkalis decompose the active principle, but only after a time; if taken immediately on mixing, its power is unaffected.

Results of Action and Mode of Elimination.—The kidneys are very active in its elimination from the minute it enters, and in two, or at most three, hours, all is removed. This occurs at all ages and in all conditions of the kidney. It is easily detected by its action on the pupils, and its presence can be easily proved if only $\frac{1}{96}$ grain of sulphate of atropia has been given, as the urine is generally retained; this is useful in a Medico-legal point of view. During the operation of belladonna the amount of urea excreted is increased, and the amount of sulphates and phosphates still more so, while the chlorides are usually diminished. The urine is increased in quantity and specific gravity. There is no increase in the uric acid, but sometimes a diminution. Hence it will be seen that it has a very similar effect to febrile action, perhaps from the same influence upon the sympathetic system, to which it is a powerful stimulant; but if its action is continued sufficiently long, the vaso-motor nerves are paralysed, and dilatation of the vessels occurs. The excessive oxidation required to furnish the phosphoric and sulphuric acids is from the increased activity of oxidation in the lungs. It is a powerful diuretic, and has the same effect on the other glands, not excepting the salivary. The drying of the mucous membrane in the upper part of the respiratory passages, the browning of the tongue, &c., are difficult to explain. The conjunctivæ are sometimes slightly dry, often injected.

Therapeutical Uses.—A study of the physiological action of belladonna has led me to regard the plant in a new light as a curative agent. First and foremost, it is a direct and powerful stimulant to the sympathetic nervous system, or, in other words, to the heart. Secondly, it is a potent diuretic. Thirdly, by virtue of its stimulant action on the circulation, it is a means for increasing the oxidising processes within the body. Its influence as an anodyne is so fully acknowledged, that I shall omit consideration of this action upon the present occasion.

First, as a *cardiac stimulant.* It is remarkable that this, the primary and essential operation of belladonna, should have been so long neglected. This plant should stand at the head of all our stimulants; for there is no medicine in the whole materia medica which at all approaches belladonna in its simple, direct, immediate, and powerful influence in exalting the force and rapidity of the heart's action. In all conditions and diseases, therefore, in which there is a depression of the sympathetic nervous influence, such as syncope from asthenia, or shock; in the collapse of cholera; in failure of the heart's action from chloroform or other cardiac paralysers—the subcutaneous use of sulphate of atropia, in doses varying from the hundredth to the fortieth of a grain, is the appropriate and most hopeful means of resuscitation.

With a view more of ascertaining the influence of belladonna in progressive failure of the heart's action in inanition, than of hoping for a permanent good result, I injected the two-hundred-and-fortieth of a grain of sulphate of atropia into the arm of an infant ten weeks old, at a time when, excepting a few beats now and then, the pulse was imperceptible at the wrist, and the cardiac systoles only 80. Within four minutes, the pulse rose to 100, and each beat was quite perceptible at the wrist. In eight minutes, it had increased to 110, and was quite regular and distinct. The stimulant continued for the next three hours; and at the end of this time the pulse was 100, of good volume, and of sufficient force to bear compression without obliteration. The respiration remained unaltered, and the pupils dilated from one-twelfth to one-seventh of an inch. The stimulant effect upon the pulse continued

to within half an hour of the death of the child, five hours and a half after the injection of the atropia.

As a *diuretic*, belladonna may be used in cases of *suppression of urine*, whether accompanied by uræmic symptoms or not. As both the sluggish circulation and the torpid kidney are simultaneously aroused by the medicine, there is ground for expecting a restoration of the renal secretion.

In *acute nephritis*, we may hope for beneficial results from the use of belladonna, which, coming in contact with the irritated and congested organ, will doubtless calm the nervous irritation, and at the same time contract the dilated blood-vessels. I am at the present time busily employed in determining the effects of its operation in congested and inflammatory conditions of the kidney; and, so far as my experience goes, I am led to expect beneficial results in both states.

In *chronic albuminuria*, belladonna, I believe, will prove very serviceable, provided that the kidney has not passed into the degenerative stage bordering on fatty degeneration. In one case, Charles E., aged 35, who had been under my care for three months for an acute attack of nephritis, commencing with excessive œdema of the legs, and exudation of albumen, I administered a single dose of atropia with the following result. On the 10th of January, he had so far improved under the influence of astringent chalybeates, and hydragogue purgatives, that there remained but slight pitting of the integuments over the tibia; and the urine, when boiled and heated with nitric acid, gave only a small precipitate of albumen,—enough, however, to render the fluid completely opaque from the presence of small flocculi of albumen. At 8.30 p.m. on the day above mentioned, I injected the forty-eighth of a grain of sulphate of atropia beneath the skin; and he passed at that time urine A. The atropia produced full effects: and at 10.30 p.m., when these had passed off, he voided urine B with some difficulty and in small driblets. Urine A had a specific gravity of 1022.4, and contained exactly a grain of albumen in 1000 grain measures. Urine B was of specific gravity 1024.4, and contained only half the quantity of albumen present in urine A. Four days afterwards, the patient presented himself at the hospital, and reported himself quite well. The œdema of the legs was entirely gone. He passed urine in the prescribing-room; and repeated examination by my clerks and myself showed that the albumen had quite disappeared. The patient has not attended since, from which I infer that he continues well. He had presented himself regularly at the hospital the previous three months, and the urine was regularly examined. The albumen was observed to be slowly diminishing in quantity, but it never had been absent from the secretion. It appeared, in this case, that the kidneys had received a sudden impulse to healthy action.

In another case, that of John B., aged 25, who had been under my care for two years continuously for acute, passing into chronic albuminuria, the albumen, which had long been stationary, began to decrease rapidly in amount under the influence of the same treatment.

The effect of a powerful dose of atropia upon the kidneys in chronic albuminuria is well seen in the analyses before given. It will be observed that there was a decided diminution of the albumen during the operation of the medicine. The result by the operation of belladonna in these cases must be accepted as the best proof of the condition of the blood-vessels generally during that operation. It is quite clear that there is no impediment from contraction of the arteries on the one hand, or from dilatation of the capillaries on the other, to the flow of blood through the kidney. On the contrary, it appears that the vessels of the gland are aroused by the action of the drug into a healthy state of excitement; a condition highly favourable for the nutrition of the organ, and the removal of chronic disease. As a means of promoting oxidation of the blood, belladonna will doubtless prove of essential service in the uric and lactic acid diatheses. I have employed it in rheumatic fever with marked success. I inject the fiftieth or fortieth of a grain of the atropia salt into the integument over the affected joint, as soon as the first indication of inflammatory action arises in the part. The ano-

dyne action is so direct, speedy, and enduring, that the use of opium, which, excepting for its anodyne and hypnotic actions, is decidedly objectionable in this disease, is altogether unnecessary. The subcutaneous use of atropia in other acute diseases is a wide field for inquiry, and promises, as far as my observations extend, to be a most interesting and encouraging one.—*Medical Times and Gazette, March* 21, 1868, *p.* 325, *and British Medical Journal, April* 4, 1868, *p.* 320.

111.—ON THE PHYSIOLOGICAL ACTION AND THERAPEUTICAL USE OF HENBANE, ALONE AND IN COMBINATION WITH OPIUM, AND ON THE COMBINED OPERATION OF OPIUM AND BELLADONNA.

By Dr. HARLEY, Assistant Physician to King's College Hospital.

[The following article is an abstract of the third of the Gulstonian lecturers of the present year.]

The lecturer commenced by describing the effects of increasing doses of sulphate of hyoscyamia when used subcutaneously. The following may be taken as a summary:—When given to an adult, and in doses insufficient to produce dryness of the mouth, the only effects are giddiness, somnolency, and dilatation of the pupils, and a progressive retardation of the pulse to that condition in which it exists after a prolonged period of complete rest of mind and body, without diminution in its force and volume.

In doses sufficient to produce complete dryness of the tongue and hard and soft palates, there will generally be an acceleration of the pulse ten or twenty beats, with a slight increase in its volume and power. This acceleration will be observed from ten to twenty minutes after the injection. It does not usually continue for longer than twenty or thirty minutes, and rarely lasts for an hour; dryness of the mouth comes on about twenty minutes after the injection, and continues about an hour.

In most cases there will be great somnolency, attended with so much giddiness that the patient is either unable to walk without assistance or reels about as if drunk; the face becomes slightly flushed, and the conjunctivæ injected; the pupils dilate. After the lapse of about an hour the mouth suddenly moistens, and the pulse, which, from the time of its maximum acceleration, had been observed to fall some five or six beats every twenty or thirty minutes, now falls with unusual rapidity, until at the end of two hours from the injection, it numbers only sixty, fifty, or even forty-two beats, still, however, retaining its original volume and power; the giddiness and sleepiness slowly pass off, and at this time the pupils attain their maximum dilatation.

The effects are precisely the same when hyoscyamus, or its active principle, is given by the mouth. Compared with belladonna, hyoscyamus agrees with it in its effects upon the mouth and pupils. Its stimulant effect upon the sympathetic nervous system is only manifest in man in large doses, and even in this case it is comparatively transient and much less powerful than belladonna. The most prominent symptoms of the operation of hyoscyamus are excessive giddiness and somnolency, effects produced by atropia in only a very secondary degree. While atropia is chiefly distinguished by its effects upon the sympathetic nervous system, hyoscyamine is distinguished by its influence on the cerebrum.

It would appear that hyoscyamine in combination with opium produces the most powerful hypnotic action possible. Each increases the effect of the other. Quantities of morphia and hyoscyamine, which of themselves are insufficient to produce sleep, will, when combined, speedily induce that condition.

Like atropia, hyoscyamine is eliminated by the kidneys, and the lecturer stated that he had detected it in the urine twenty-two minutes after the subcutaneous injection of $\frac{1}{15}$th of a grain of sulphate of hyoscyamine.

Treating of its therapeutical use, the lecturer stated that he had found it

serviceable in certain cases of epilepsy and enuresis, and extremely valuable in irritable conditions of the brain and heart, and that it is especially useful in often determining and invariably increasing the hypnotic action of opium.

In treating of the combined operation of belladonna and opium, the lecturer having previously determined the separate effects of atropia and morphia upon the horse, the dog, and man, gave the results of their operation when simultaneously administered, or when the one remedy was allowed to precede the other by a variable time.

From numerous experiments upon the horse, which were made by Mr. Frederick Mavor, of Park-street, and himself, he concluded that the reverse of any antagonism exists with respect to this animal; and he plainly proved by the experiments adduced, that the medicines not only intensify, but very much prolong, each other's effect.

The experiments upon the dog also led to the same positive conclusion. Some of them were peculiarly instructive. In one case, a quantity of atropine, which had been proved to be incapable of producing sound or continuous sleep, was given to the animal two hours after the administration of a subcutaneous dose of opium, and at a time when the dozy condition induced by the latter had passed off, the pulse being 78, respirations 18, pupils one-sixth contracting to one-seventh. Within five minutes of the injection of the atropine the animal was in a complete state of narcotism, and remained so without the slightest motion for the next four hours, and could not be aroused by pinching or pricking the skin, or by poking the finger down upon the glottis. The atropine effects, meantime, were extremely developed, and were much prolonged, and the dog continued to sleep soundly for three hours more.

In man, precisely the same results were observed in all the cases treated with opium and belladonna, either simultaneously administered, or when one was given some time previous to the other.

The lecturer could come to no other conclusion than that, as far as a hypnotic influence was concerned, belladonna decidedly increased the effects of the opium, and, on the other hand, opium invariably intensified not one or two, but *all* the effects of belladonna.

One important fact, however, resulted from the numerous experiments which he had made upon the dog and upon man. In a large proportion of patients he found that the subcutaneous use of morphia was followed by faintness, nausea, increasing to vomiting and violent retching, with weak and often intermittent action of the heart, these distressing symptoms lasting for many hours. When, however, a small quantity of atropine (the $\frac{1}{75}$th of a grain) was administered with the morphia, these alarming effects never followed.

He explained this fact by attributing to the atropia such a powerful stimulation of the sympathetic nervous system as was able to overcome that derangement of the vagus nerve which opium so frequently produces. In other patients, in whom opium alone fails to induce sleep, the combination of opium and belladonna, whether given by skin or by stomach, procured the desired result.—*Medical Times and Gazette, April* 4, 1868, *p.* 376.

112.- ON THE USE OF "RICHARDSON'S SPRAY PRODUCER" IN RESTORING THE BUBBLE OF AIR TO ITS PROPER PLACE IN THE MEDICAL THERMOMETER, WHEN ACCIDENTALLY DISPLACED.

By Dr. J. W. Merriam, Valparaiso, Chili.

By careless manipulation, the self-registering principle of the Medical Thermometer may be destroyed if the bubble of air, which forms the distinctive feature of one class of these instruments, should become lost in the bulb.

This accident happened to me a few months ago, and the difficulty which I

found in re-adjusting the instrument, and my final success, led me to think that a statement of the *modus operandi* might be of service to others.

While lost in the bulb, the bubble of air generally appears like a minute, bright point, and may, at first, be easily mistaken for a defect in the glass; if, however, the thermometer be reversed, and shaken a few times, the bubble rises to the top of the mass of mercury in the bulb and expands so much as to be one or two lines in diameter. It is well to satisfy one's self in this way of the exact position of the recreant bubble before proceeding further.

My first expedient was to try and restore the bubble by sudden, short jerks of the instrument downwards. This was always sufficient to displace the bubble again, more or less, and even in some instances to cause it to re-enter the tube, but at so low a point in the column as to appear still only a mere speck on account of the weight of the column above it. After two hours' trial, I recognised the following to be the cause of the difficulty. At *ordinary* temperatures, the mercury is so much expanded that it rises too high in the tube to insure the success of the effort, and it occurred to me that, by lowering the temperature of the mass very considerably, the main obstacle would be removed. I accordingly threw a jet of *ether-spray* upon the bulb, and the mercury fell so low as to enable me, by means of a few short jerks to place the bubble where I wanted it, about five or six lines from the top of the column.

Great care must be taken not to allow the bubble to pass *above* the column of mercury, for it is doubtful whether, in that case, it could be again dislodged and placed lower in the column, where it would be serviceable.—*Valparaiso, March* 4, 1868.

113.—ON MEDICINAL INHALATIONS, WITH DESCRIPTION OF AN IMPROVED APPARATUS FOR THE PRODUCTION OF MEDICATED VAPOURS.

By Dr. James Adams, Glasgow.

[The following paper was read before the Glasgow Medico-Chirurgical Society. The combination of Bergson's and Siegle's several apparatus is the one now in general use for the purpose of medicinal inhalation.]

It consists in a small boiler heated with a spirit lamp, the capillary tubes being attached to the boiler. The steam produced in the boiler issues from one of the tubes with considerable force, and in a steady current, producing the same effect as a blast of compressed air, so that the medicated fluid is projected in a gentle warm mist or vapour, very much finer than could possibly be previously obtained by any other arrangement. Contrasting the vapour produced by this apparatus with that of the air-pump or bellows arrangement, I should liken the first to a fine Scotch mist, and the second to a plump of rain.

Experiment has shown what indeed was already very obvious, *i. e.*, that this very minute division, or pulverization, of the medicated fluids, is a matter of prime importance, as the finer the vapour the less the irritation to which it gives rise in passing the larynx, and the more effectually it penetrates to the minutest and furthest removed air-tubes. The patient requires no assistant in working the instrument; he inhales without fatigue or flurry, and at leisure; and the inhalations can be repeated and prolonged indefinitely and at pleasure.

I have during the last fifteen or eighteen months had very extensive trials of this apparatus; and I think that, with some modifications it has undergone at my hands, and to which I will immediately refer, it leaves little to be desired. I was led to a consideration of these modifications on account of the high cost of the instruments, ranging from 15s. to 50s., and the occasional delay of many days, and even weeks, before they could be obtained. These were serious obstacles to a fair trial, or to the general use of the instrument, and I was unable to stimulate either instrument-maker or druggist

to find a remedy. I at length got a smart tinsmith to work several patterns under my directions, and to produce an instrument equally effective with the most costly, and a respectable druggist to give him an order for a supply. These within the last few months, under the stimulus of the sulphur mania, have been sold in large numbers by several druggists at an average cost of 4s. One druggist tells me he alone has sold upwards of 200.

I felt interested in my attempts to improve the instrument, and you would be amused if I showed you all the patterns from first to last, and surprised if I were to go into a description of little details which required consideration or remedy before the instrument I now place on the table was produced. Suffice it that I feel warranted in saying that it is the most efficient, ready, and cheap instrument that can at the present moment be obtained. Let me state shortly some of the qualities wherein I think that it contrasts with, and is superior to, others.

It is compact and ready for use, and is so put together that there is no necessity for frequent and nice adjustments,—consequently the risk of troublesome disarrangements, or of accidental injury of the instrument, is exceedingly small.

The form of the boiler is distinctive and peculiar, and ensures several advantages. Thus, the position of the water inlet enables the boiler to be filled to the proper height and no more. Above the water line and inlet there is a reservoir for steam sufficiently large to maintain a continuous current of spray, and to project it to any distance that may reasonably be desired. This arrangement of the water inlet, steam chamber, and steam outlet, prevents a very annoying and even dangerous accident of frequent occurrence in instruments with the ordinary form of boiler, namely, the forcible projection of spirts of scalding water in the face of the patient, caused by the boiling liquid coming over with the steam. The heat from the spirit lamp is carried up through the centre of the boiler, thus reaching a larger heating surface of the boiler, generating steam more rapidly, keeping up a full supply of the steam, and at the same time *super-heating* and *drying* the steam so generated. The steam escapes by a short horizontal nozzle at the top of the boiler, and necessarily is subjected at the instant of its escape to the action of the flue of the lamp, thus ensuring such a dry condition of the steam that it quickly becomes dissolved or dissipated in the air, so lessening the risk of annoyance to the patient, and at the same time avoiding in a great measure the dilution of the medicated fluid with watery steam. By carrying the flue of the lamp through the centre of the boiler, the body of the instrument is not so hot but that a handle attached to the lower and cooler portion of the case can be grasped with comfort and safety even when in use. An alarming-looking and costly safety-valve, very liable to become stiff and unworkable, has been dispensed with, as I have found that a simple cork or india-rubber plug is equally efficient and more convenient. The box containing the phial of medicated fluid is soon warmed by the mode of its attachment to the case of the instrument, and there are other details of minor importance which, when combined make the apparatus more convenient in use.

Before parting with my reference to this instrument I may mention that I have experienced annoyance from finding my patterns and directions departed from occasionally by workmen who did not understand what I was aiming at. An old friend of mine, who had opportunities of knowing this fact, suggested a patent, which I of course rejected, my ideas on medical patents being at one with my friend Dr. Gairdner. I did not object, however, that he should do so; and associating with himself a respectable druggist, he has taken out a patent. I hope thus to ensure accuracy and good quality of workmanship with cheapness of manufacture. *This will certainly gratify me; but beyond this, I beg emphatically to say that I have no right of property in the instrument, and no interest whatever in its sale.*

Having said so much regarding instruments, I have little time to refer to the medicaments, which may conveniently, and with probable advantage, be employed in the form of vapour. Those with the use of which I am most

familiar are solutions of morphia, digitalis, stramonium, squill, tannin, alum, nitrate of silver, sulphate of zinc, chloroform, acetic acid, and sulphurous acid. I extend and combine this list of agents as seems to me desirable. Each drug has of course its special properties, and it would open too large a discussion to enter, however shortly, upon their consideration. I might say much to show why I have reason to be satisfied with the results I obtain from time to time; but I think it sufficient to indicate the fact that I am so satisfied, and that I believe that the inhalation of medicated vapours is likely to be a more familiar and a more important therapeutic agent in the hands of the physician in time to come.

Those who are inclined to pursue this subject will find ample details of the history and full directions for, and illustrations of, treatment by medicinal inhalations in the work of Dr. Biegel, of London—an admirable work in my opinion, and showing in its author the possession of sound common sense and professional ability.

At the last moment it has occurred to me that it might give point to the object of my communication, and open an easy inlet to the observations of the members, if, before concluding, I should make special reference to one agent, with the name and alleged miraculous virtues of which the public of Glasgow has been made very familiar during the last few months. I need scarce say that I allude to sulphurous acid gas, or, as it has been termed in the popular epistles through which it has gained notoriety, "the Great Sulphur Cure."

The statements in Dr. Dewar's original pamphlet regarding the value of sulphur as a great remedial agent, seemed to me at the time sufficiently moderate and circumstantial to warrant a candid examination: and during the last twelve months, and of course long before the recent excitement, I have given to it a fair trial in a spirit of patient observation. I may at once say that I have satisfied myself that it has its uses—that it is not an innocuous agent—one to be pooh-poohed and laid aside without trial, or to be contemptuously stifled in a cloud of "chaff" scattered over the columns of a newspaper. But, to qualify this allusion, I admit that it is very difficult to be amazed and yet temperate when reading the loose statements, crude deductions, and far from satisfactory cases recently published by Dr. Dewar; or, to refrain from disparaging the rhapsodical style of his enthusiastic follower, Dr. Pairman. I am well satisfied that they are both sincere men; and, believing as I do, that they have made statements in perfect good faith, and alleged as matters of fact, occurrences that are open to daily and familiar experiment and observation, I think it preferable to sift these statements before troubling myself about misty theories, or rather hypothetical conjectures—or in denouncing pretensions however apparently absurd. If these gentlemen are competent observers, and of sound judgment, then the occurrences they have recorded must have been repeated under like circumstances; and this recurrence of powerful and novel effects has no doubt been observed, and will be corroborated by the testimony of medical men whose position as competent observers is established. We have such observers among us this evening, and I hope they will express such a judgment as their experience warrants them in giving. As regards myself, I repeat that I have observed with patience and without prejudice, and as a result of my observations I felt warranted, at the commencement of the late sulphur mania—for it deserves no other name—in expressing my opinion that the agent was useful; and that Dr. Dewar deserved great credit for his earnest and persevering efforts in directing the attention of the profession to several novel and useful applications of the remedy; but that it was most preposterously overlauded—that its excessive popularity would have a short day—and that some injury and many grievous disappointments would remain *en souvenir*.

My experience of the remedy, in some of its principal applications to medicine in the way of inhalation, and without any reference to surgical ailments, may be shortly stated.

Individuals whose general health was good, and who have resorted to the sulphur inhalations on account of such slight forms of catarrh as usually

receive little or no medical treatment, except it may be a sweating powder, or a warm bath, have been the most liberal in their acknowledgments of benefit. It did really seem to me that some cases were relieved or shortened in their progress by the treatment.

In acute bronchitis I have seen a copious secretion of serous or watery fluid cast off from the air passages at an unusually early stage of the disease, and this effect was evidently induced by the inhalations, and was followed by marked relief.

In chronic coughs it has frequently acted powerfully in exciting forcibly expiration, and in inducing a more copious expectoration.

From these results I am of opinion that in catarrh, acute bronchitis, and chronic coughs, the remedy stimulates the minute exhaling vessels, the bronchial surface seems to be both sweated and purged, and the tough, viscid phlegm which collects in the bronchi is dislodged more freely and effectively than occurs under the use of ordinary expectorants, administered in the usual way. But I have found no notable difference nor any advantage in use in the sulphurous acid over common vinegar or solution of chlorine administered by inhalation in like cases.

In a few opportunities I have tried it in asthma, but have not found that the inhalations were tolerated or continued, partly on account of marked distress which they occasioned, and partly because no evident relief was obtained during the paroxysms.

In phthisis its effect seems to be altogether that of a topical expectorant, but I have not seen any decided lessening of the amount of expectoration.

Cases of inflammatory sore throat have not been benefitted, but in the hoarseness following the acute stage of a cold I have seen improvement and relief.

I have not seen, in any of its applications, the alleged "sedative" or "calmative" action of the remedy.

Troublesome and even alarming consequences are of occasional occurrence after prolonged inhalations of the dry sulphur fumes, or of the aqueous solution of sulphurous acid gas in its full strength,—such as great tumefaction of the tonsils and pendulous palate, husky voice,—difficulty in swallowing,—pain in larynx, spitting of blood, &c.; and I have seen all these effects occur under circumstances where no blame could be justly imputed on the score of carelessness.

It is not, therefore, a remedy to be administered indiscriminately or without precaution, and I deprecate the practice of fumigating a patient's room by throwing a quantity (no limit as to quantity) over a heated shovel or live coal, and filling an apartment (which may be large or small, well ventilated or otherwise) with the dry fumes of an indefinite quantity of sulphurous acid gas,—an agent of known powerful chemical properties, and alleged to possess very potent influence upon the living organism. As well tell a patient to take "a large dash of laudanum" in his gruel when going to bed, or "a good pinch of calomel" occasionally.

My remarks on the sulphur cure have gone to a greater length than I anticipated, and I will now only add that I have not seen any effects from its use that have led me to think that it has any *specific action, when inhaled in any disease*, or that it has any other effect than that of a local stimulant, tonic, and astringent. As such I believe it is likely to prove a useful adjuvant in the treatment of various maladies.—*Glasgow Medical Journal, March*, 1868, *p.* 401.

114.—ON THE ANÆSTHETIC ACTION OF BICHLORIDE OF METHYLENE.

By Dr. Benjamin W. Richardson, F. R. S.

I wish you to observe the action of the anæsthetic again, inasmuch as it is *typical of* all the group of the chlorides of the monocarbons. To see the *symptoms* well, we will narcotise slowly. A pigeon is again the subject: it

is the hundred-and-thirteenth time this bird has been narcotised by the agent, but it has never suffered in the least. By narcotising slowly, we develope all the stages or degrees of sleep—a first degree of slight excitement; a second degree (and in this case very short, not more than a second or two in duration) of rigidity; a third degree of relaxation and complete insensibility; and, pushing the inhalation a shade further, that extreme degree, fourth degree, of prostration, which is immediately preliminary to death. We now remove the animal; it will remain unconscious, probably, for half an hour or more, and will then suddenly recover.

I say this substance is a fair type of all its allies, and this is true; but it has certain different points of action, which mark it off. It narcotises more quickly in an open chamber than the chloride of methyl, because its vapour is so much heavier—42·5 to 25·25—and is therefore less readily diffused in the air and lost, and it narcotises more quickly than chloroform, or tetrachloride of carbon, because its vapour, quite heavy enough for inhalation, and lighter than the vapour of chloroform in the proportion of 42·5 to 59·75, and lighter than the vapour of tetrachloride of carbon in the proportion of 42·5 to 77, is more easily diffusible from the air vesicles of the lung into the blood than its heavier comrades, for reasons we shall see in the sequel.

Before I leave the bichloride of methylene, let me say a word in reference to its action on the human subject. I have now administered the bichloride for the performance of nine capital operations. In one case the insensibility was sustained for an hour and seven minutes; in no case was the insensibility less than thirty-five minutes. In all there was the same order of phenomena as that we have just seen—a brief first and second stage, a rapidly developed third stage, and a very prolonged and deep anæthesia. The same results have been obtained by my friend, Mr. Peter Marshall, one of the earliest students and practitioners in anæsthesia, and by Dr. Junker.

In one of my cases only was vomiting a symptom during the anæsthetic condition, and in that case the vomiting was not, I think, strictly attributable to the narcotic; it was also very slight, a single effort with eructation causing the loss of about a table-spoonful of fluid from the stomach.

The prolonged anæsthesia after free inhalation of the bichloride of methylene is a striking feature in its action, and has, I understand, been a cause of some anxiety. There is no occasion for anxiety from, nor for interference with, this sleep; it passes off quite naturally if time be allowed, and what is more, it passes off leaving no headache and no feeling of depression behind. In most cases of operation, the prolonged anæsthesia is indeed of advantage, the sleep being so quiet, so undisturbed, so gentle, and so easily sustained by the briefest renewal of the administration of the vapour. I had an opportunity of obtaining a comparison from one of my patients respecting the difference of effect between chloroform inhalation and inhalation of bichloride of methylene. This patient, some years ago, was narcotised with chloroform by Dr. Snow; she was narcotised with bichloride of methylene by myself, to enable Mr. Spencer Wells to perform the operation of ovariotomy. She was a very acute observer, and most precise in her definitions. A few days after the operation Mr. Wells was so good as to take me to her, when she explained her differences of sensation under chloroform and under the bichloride of methylene. Her sensations under chloroform as she began to inhale it were those of oppression of the breathing, loud noises in the head, ringing in the ears, and a feeling of absolute necessity to get up and resist the further administration; on recovery from the chloroform, she felt an indescribable sense of exhaustion, an intense nausea, and a headache which lasted for many hours. By the inhalation of the bichloride of methylene she escaped, she said, every one of these annoyances. She dropped into sleep as into natural sleep; she awoke as out of natural sleep, with not one painful symptom; in all, it was as though she had closed her eyes and opened them again, the intervening space of time during which the operation had been performed, and which, with after-sleep, had reached fifty-five minutes, having been altogether inappreciable to her mind.

With the facts I have carefully collected before me, I have no hesitation

now in stating that bichloride of methylene as a general anæsthetic is at least as safe as chloroform, and that it possesses certain advantages of its own which give it a better position amongst anæsthetics than chloroform. The only obstacle to its success is the difficulty of its manufacture and its consequent greater cost than the cost of chloroform; but I should trust that in time this obstacle will be overcome. Administered internally by the stomach in doses of from ten to twenty minims, bichloride of methylene is a good anodyne. To make our studies complete, we have let an animal—a rabbit—sleep to death in bichloride of methylene. The action of the heart and of the muscles of respiration ceased at the same moment. I will pass the dead animal round, and you will see that the lungs are of natural colour, and that the heart contains blood on both its sides. In the trough with the animal is some blood saturated with the bichloride. The colour of the blood is bright red, but the fluid has undergone a feeble coagulation. The coagulum is not of fibrin, for the blood was defibrinated at the time it was abstracted, it is a clot of albumen and red corpuscles. This is a peculiarity of action of all these chlorine compounds; the cause coagulation of defibrinated blood. In this direction the bichloride is less effective than chloroform, and chloroform is less effective than tetrachloride of carbon, but the tendency is distinctly indicated in the specimen under observation.—*Medical Times and Gazette, Dec.* 28, 1867, *p.* 693.

SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR; INCLUDING SEVERAL PAPERS NOT INSERTED IN THE BODY OF THE WORK.

AFFECTIONS OF THE SYSTEM GENERALLY.

AGUE.—*Strychnia.*—Five interesting cases are related illustrative of the action of quinine in certain types of ague. In the first case, quinine having entirely failed, one-sixteenth of a grain of strychnia was given four times a day. This "immediately checked the paroxysms, and the patient was able to return to his estate after having been only five days under treatment, and wrote a fortnight afterwards to say that he still continued free from fever." (Dr. J. P. Nash, H. M. Madras Army, p. 19.)

RHEUMATISM.—If one-fiftieth to one-fortieth of a grain of sulphate of atropia is injected in the neighbourhood of joints affected with rheumatism the pain is immediately relieved. (Dr. J. Harley, p. 268.)

Alcoholic Rheumatism.—This is quite a distinct form of disease from that which arises from exposure to cold and wet; neither is it gout: yet it has not hitherto been properly distinguished from either of these two diseases. It appears usually about the middle age of life, after the person has daily imbibed fermented alcoholic liquors, even in moderate quantities. There is an expression of dulness or stupidity in the countenance, a weakness of the intellect, a stiffness and sluggishness of the limbs, producing a peculiar hobbling gait. These symptoms increase with age. This complaint does not affect the general health. It may be seen in publicans, gentlemen's servants, agricultural labourers, and farmers, and is very common in the cider counties. (Mr. J. Higginbottom, p. 27.)

FUNGOID ORIGIN OF FEVERS AND CHOLERA.—The opinion is gradually gaining ground that the malarious fevers of hot climates, dysentery, yellow fever, and cholera, are all caused by the action of different species of fungus or of infusoria. The time is probably not far distant when further investigations will prove that enteric and typhus fevers, as well as scarlet fever, measles, and small-pox, have a like origin. In the late fatal epidemic of fever in the Mauritius, minute plants of fungus were found along the whole of the intestinal canal, and these corresponded with growths found in the waters of the Grand River. These spores were not found in the blood of patients, nor have they been found in the blood of fever and cholera patients in this country. (Mr. R. Hamilton, p. 17.)

Organic Germs.—It is not the gases which result from putrefaction which are sources of danger to life, but the organic germs which are so abundantly produced by that process. The presence of these may be detected with the microscope, and are found to consist of monads, vibrios, amœbæ, and other of the lowest forms of life. These may be seen in the vapour condensed by placing a bell-jar over putrid meat. M. Chalvet finds that when watery vapour near a suppurating surface is collected, it is found to be charged with irregular corpuscles resembling dried pus. Eiselt, of Prague, found small cells like pus cells in the air of a ward where epidemic ophthalmia was raging. (Dr. W. Procter, M. Chalvet, M. Eiselt, p. 28.)

PYÆMIA.—Dr. Richardson has separated the poison of pyæmia; it may, he says, be evaporated to the form of syrup or extract. It produces, when dried, a substance closely resembling the snake poison. It admits of being pulverised, and when it is introduced into the wound of a healthy animal produces precisely the same symptoms as those of the patient from which the poison was taken. (Dr. W. Procter, Dr. Richardson, p. 30.)

THE TYPHOID STATE IN DIFFERENT DISEASES.—In the typhoid state, in whatever disease it is present, it is probable that the pathology is always identical. For instance, in cases of contracted kidney, it is generally admitted that the cerebral symptoms—the typhoid state—often indistinguishable from those of typhus, are owing to a poisoned state of blood from retention of the products of the retrograde metamorphosis of the nitrogenous tissues, which it is the function of the kidney to eliminate from the body. When convulsions, a symptom characteristic of an extreme typhoid state, occur in scarlatina, the cause is retention of the renal excreta from inflammation of the kidney, and consequent blood-poisoning. When similar symptoms occur in typhus, there is always albumen in the urine. These considerations point to the appropriate treatment, viz., depuration of the blood, at the same time avoiding all measures which are likely to irritate the kidneys. (Dr. Murchison, British Medical Journal, Jan. 4, 1868, p. 6.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CONIUM.—*Therapeutic Action.*—Hemlock has no cerebral action at all. It is to the corpora striata, the smaller nervous centres, and the whole of the motor tract, just what opium is to the brain; it tranquillizes and renovates the whole nervous system. Its full action is asleep. The neglect of conium as useless is attributable to its administration in too small doses. It is pre-eminently a children's remedy, and is of service in laryngismus stridulus, and tetanic convulsions. (Dr. J. Harley, p. 265.)

HYPNOTIC.—*Henbane in Combination with Opium.*—Hyoscyamine in combination with opium produces the most powerful hypnotic action possible. Each increases the effect of the other. Quantities of morphia and hyoscyamine, which of themselves are insufficient to produce sleep, will, when combined, speedily induce that condition. (Dr. J. Harley, p. 269.)

PARALYSIS.—*Hypodermic Injection of Strychnia.*—Strychnia has long been acknowledged to be the best remedy we have in cases of paralysis, but the advantages to be derived from its use by hypodermic injection far exceed those from its use in the ordinary way. We may expect to find a beneficial result produced in a shorter time, and from much smaller doses, than when it is given by the mouth; and benefit may be expected even in cases which have been treated by its stomachic administration without benefit. It is well to commence with small doses, as the one-ninetieth of a grain, and if this is borne well, increased to one thirty-sixth of a grain, which is borne well by most patients. Some bear well one twenty-fourth of a grain. It should not be given when active irritation is going on about the clot or presumed cause of the lesion. About two days should elapse between each injection. The cases related are very interesting. One noteworthy symptom produced was a feeling of heat in the body generally. (Mr. C. Hunter, p. 33.)

TETANUS.—*Nicotine.*—A case of tetanus is related in which the effect of nicotine was very remarkable. The most noticeable was a diminution in the frequency of the heart's action. In four hours and three-quarters the pulse fell from 120 to 68. The patient also at the same time sweat profusely, whereas previously the skin had been hot and dry. There was also much less muscular rigidity than had previously existed, and he was enabled to pass his urine naturally, which he had not done for thirty hours. The case ended favourably under this treatment. In administering the nicotine it was found that one-twelfth of a drop was quite sufficient to produce a decided effect, *and at no* time was it increased to more than one-eighth, dissolved in alcohol *and water.* (Mr. R. Harrison, p. 43.)

Calabar Bean.—Dr. E. Watson reports on the use of Calabar bean in traumatic tetanus. He and two of his friends have treated six cases with it, and all terminated favourably. He considers that the Calabar bean produces the exact counter effect on the spinal marrow to the state in which tetanus originates. In its action it appears to resemble nicotine. (Dr. E. Watson, p. 45.)

In treating tetanus by Calabar bean we must be careful to begin the administration of it as early in the case as possible. If there is much difficulty in swallowing it should be injected hypodermically, as given in this manner it is both certain and rapid in its effects. Eight grains of the extract rubbed down in and mixed with an ounce of spirits of wine, form a solution or tincture every ten minims of which contain one sixth of a grain of the extract. (Dr. E. Watson, p. 48.)

Toothache.—*Carbolic Acid.*—To one drachm of collodium flexile (B. P. 1867) add two drachms of Calvert's carbolic acid, full strength. A gelatinous mass is precipitated. A small portion of this precipitate inserted into the cavity of an aching tooth gives immediate relief. (p. 51.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Acupressure.—The advantages of acupressure over the ligature consist in: 1, The ease of its application without the aid of an assistant. This is by no means to be despised in country practice. The spring artery forceps does not confer an equal advantage. 2, The control of the venous hemorrhage at the same time as the arterial. 3, The short time (forty-eight hours at the very longest) that a pin need be left in the wound in order to insure complete closure of the vessel. 4, The offensive moist seton, in the shape of the ligature hanging from the wound is gone, and this of itself is sufficient to recommend the use of the pin and wire. On the other hand it is not found in London that union by the first intention without the formation of a drop of pus occurs, as is the experience at Aberdeen. The author has employed acupressure in thirty-one cases, but not in one instance was there absence of pus. (Mr. C. Forster, p. 118.)

The Aberdeen Twist.—What is known as the Aberdeen twist consists in passing the pin by the side of the bleeding vessel, and after it has pierced a small portion of tissues, bringing the point out of the flesh, then turning the pin round a quarter of a circle, allowing its point to go over the vessel, and fixing the needle into the soft parts beyond. This method is very effectual and easy of application, and does not require the use of a wire. (Mr. C. Forster, p. 116.)

Arterial Hemorrhage.—*Dr. Taylor's Acupressor Arteriæ.*—Bleeding arteries may easily and effectually be closed by small prehensile forceps, which may be removed at pleasure with perfect ease, and so have all the advantages of acupressure without the disadvantage of compressing other tissues than the vessel itself. The instrument is made by bending a piece of steel wire in the middle upon itself so as to form one spiral turn, and then by crossing the two ends at a little distance from the spring, and adjusting them in length so that they form a small pair of forceps. One extremity should be bent upon itself so as to give, with the single end which meets it, a firmer hold upon the vessel. A fine piece of wire should be attached to the spring of the forceps for its withdrawal, which is accomplished by passing a slender silver tube down the wire and upon the forceps, the latter losing its hold of the vessel immediately its arms are compressed by the tube. These forceps might be manufactured by the gross at a very cheap rate. (Dr. W. Taylor, p. 120.)

Dr. Wolfe's Canula Artery Forceps.—This form of canula forceps will be understood at once by reference to the woodcuts. They are intended for the arrest of hemorrhage, in the same way as by acupressure. (Dr. J. R. Wolfe, p. 122.)

[In the article at page 164 the references (*a. b.*) to the woodcuts are wrongly placed—they should be reversed.]

Closure of Arteries by Torsion.—Prof. Syme, of Edinburgh, states that since he has arrested arterial hemorrhage (except from the largest vessels) by torsion he has not had a single case of secondary hemorrhage, either public or private. In one case lately he trusted to torsion alone for closing the popliteal artery, but does not advise this as a rule. The late Mr. Teale, of Leeds, used torsion equally as much as Mr. Syme, and it was always his plan to twist the end of the vessel quite off. (Prof. Syme, Mr. T. P. Teale, p. 114.)

By torsion alone almost all arterial hemorrhage can be arrested. No ligatures are required even in amputation of the thigh. Syme's torsion forceps are the best. (Mr. E. Hamilton, p. 254.)

Belladonna as a Cardiac Tonic.—There is no medicine in the whole materia medica which at all approaches belladonna in its simple, direct, immediate, and powerful influence in exalting the force and rapidity of the heart's action. In all conditions and diseases in which there is a depression of the sympathetic nervous influence, such as syncope from asthenia, or shock, in the collapse of cholera, belladonna, or the subcutaneous use of from the hundredth to the fortieth of a grain of sulphate of atropia forms the most appropriate treatment. (Dr. J. Harley, p. 267.)

Detection of Minute Quantities of Blood by the Micro-Spectroscope.—By means of the micro-spectroscope, a combination as its name indicates of the spectroscope and microscope, as small a quantity of blood can be detected as the thousandth of a grain of the dried fluid. The dark absorption bands peculiar to old blood can by this means be distinctly seen. (Dr. W. B. Herapath, British Medical Journal, March 7, 1868, p. 217.)

Dilated Blood-Vessels.—*Aconite.*—Of those medicines (excepting direct astringents) which act upon the blood-vessels, cold and heat may be taken as the types. Cold produces great contraction of the vessels, lasting a long time, and is succeeded by reaction when the cold is removed. Heat produces slight contraction, which soon passes off, and is succeeded by decided dilatation of the blood-vessels. Thus it will be seen that the recognised action of cold is its primary action, whilst that of heat is its secondary action. The action of aconite is very similar to that of cold. Its primary effect is upon the heart, causing contraction of the blood-vessels which supply its tissue, and a consequent decrease of the vital properties of the organ. A state of collapse is produced, somewhat similar to that of cholera. But the further phenomena of the drug, which are equally important if not more so than those just named, appear to have been totally overlooked. A reaction occurs, and the blood-vessels of the heart become preternaturally dilated, the circulation more active, and a state of fever is present. In medicinal use we want, however, only the primary action, and for this purpose we must give it in very small doses, as of a quarter of a drop to a drop of the tincture, repeated every half hour or hour. Two or three doses will cure a feverish cold, and sometimes the relief is immediate. When a blood-poison is present the effect is not so marked; nevertheless, in rheumatic fever, considerable relief is sometimes obtained. (Dr. Reith, p. 258.)

Hemorrhage from the Rectum.—If the blood comes before the motion, it probably arises from the lower part of the rectum. If it comes during the motion, it is probably caused by the feculent matter running across the plugged-up mouth of a wounded vessel, and displacing the plug. If it comes after the motion, it is evidently associated with tumor or something of the sort higher up the intestine. It is necessary to ascertain accurately the source of the blood, and for this purpose the patient must have a stool in your presence, and be directed to bear down thoroughly. If necessary, an enema may be given to facilitate the action of the bowels. In nine cases out of ten you will see at once the source of the bleeding. The hemorrhage may come from a small depressed ulcer seated near the anus, or it may come from *several little* raspberry-like prominences. As they are everted during defecation, it will be extremely easy to apply a ligature or the actual cautery

or nitric acid to the bleeding part, according to your own choice. (Mr. J. Hilton, p. 109.)

INTERMITTENT PULSE.—That which impresses me most in favour of the cerebral origin of intermittent pulse, is the mode in which it appears in the prime of life. I have never met with a case in which it has not been traceable to some form of cerebral excitement with succeeding depression—grief; shock from failures of enterprises in business; disappointment; violent outbursts of passion; remorse; degradation; and, most common of all in this madly striving age, overwork of brain. The disease is owing to a failure of the left ventricle to contract when full to the proper degree with arterial blood, it thus with a violent effort makes up for previous inaction. The disease is nervous, not connected with disease of the heart itself. Nor does it arise as is so often thought, from dyspepsia, although it is true that many dyspeptic persons have intermittent pulse. (Dr. B. W. Richardson, p. 59.)

PLUGGING THE POSTERIOR NARES.—To detach and withdraw the plug by which the posterior nares has been stopped is not always easy. It is best effected by passing the gum-elastic catheter, by the aid of which the operation is supposed to have been performed, along the string by which the plug has been drawn into its place, and the end of which must, until wanted, be fastened on the forehead by adhesive plaster. When the plug is reached by the catheter it can be detached by it and carried into the fauces, and so withdrawn. (Mr. J. I. Mackenzie, p. 114.)

WOUNDS OF THE PALMAR ARCH.—Arrest the circulation through the ulnar and radial arteries by acupressure. The pin may be removed in forty-eight hours. (Mr. C. Forster, p. 119.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

HOOPING COUGH.—The author states that he has found advantage, in cases of hooping cough, from evaporising and diffusing in the air of rooms occupied by the patients the vapour of carbolic acid. He has designed a simple apparatus for the purpose, which may be obtained of Mr. Salt, of Birmingham. (Mr. V. W. Blake, p. 76.)

MEDICINAL INHALATIONS.—Dr. Adams, of Glasgow, describes a very efficient apparatus which he has devised, by which a patient without any effort on his part may inhale a warm medicinal vapour for any length of time. It is compact and ready for use, and is so put together that there is no necessity for frequent and nice adjustments, consequently the risk of troublesome disarrangements, or of accidental injury of the instrument, is exceedingly small. It may be procured from Mr. P. Harrower, or Mr. D. P. Walker, chemists, Glasgow, price 5s. (Dr. J. Adams, p. 271.)

SCROFULOUS PNEUMONIA.—In this disease, which simulates as far as regards its physical signs various other diseases, the deposit occurs first at the base of one or both lungs, and seldom, at any period of the case occurs in the upper lobes. Crepitation as in ordinary pneumonia is the first physical sign which occurs, and it is very difficult to distinguish the case from one of pneumonia, but there are these differences: there is not the extreme weakness and prostration of pneumonia; the breathing is but little oppressed; the expectoration is not rusty, unless there has been decided hæmoptysis; and lastly, there is not in scrofulous pneumonia, the dulness on percussion which occurs in ordinary pneumonia. Pneumonia begins to clear up after a definite time, whereas in scrofulous pneumonia day after day passes and yet the physical signs remain, still few and indeterminate, and consisting only of crepitation with after a time, a little dulness. This long continuance of such physical signs is quite sufficient of itself to lead us to a correct conclusion on the nature of the disease. The amount of elevation of temperature in scrofulous pneumonia (102°, 103°) is usually less than in ordinary pneumonia (104°, 105°). The crepitation of scrofulous pneumonia is generally a little larger than that of ordinary pneumonia, and often accompanies expiration

as well as inspiration. As the physical signs in scrofulous pneumonia deepen in intensity, the fine rhonchus is heard over the whole lung, whereas in ordinary pneumonia it is only heard at the edge of the affected part, and is inaudible at that part where the disease first commenced. (Dr. S. Ringer, p. 68.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Disease of the Gums.—After washing the teeth night and morning with a soft tooth-brush, apply on the brush to the affected parts a little of the following lotion: Carbolic acid, one scruple; rectified spirits of wine, two drachms; distilled water, six ounces. (Mr. Hilditch, p. 125.)

Flatulent Distension of the Colon.—Creosote and carbolic acid are the most likely remedies to check flatulent distension of the colon from gaseous evolution; and when there is also diarrhœa a carbolate of lime may be given with marked benefit It may be obtained from Squire, of Oxford Street. (Dr. S. O. Habershon, p. 125.)

Gastro-Neuralgia.—*Arsenic.*—In cases of gastrodynia, in which the pain comes on at irregular intervals, and bears no relation to the time of meals, and especially when the source can be traced to mental anxiety, the remedy of most service is arsenic. Some interesting cases are related in illustration of this, in the two first of which the attributes of neuralgia were well marked, the pain occurred spontaneously, without inflammation, and was sudden in its coming and its going. In one case the pain was induced by food, but the failure of ordinary remedies for pain after food, and the fact of the case being one of phthisis, led to a trial of the arsenic, and this proved successful. There is a connection between ague and some cases of gastrodynia, and if we have a history of the previous existence of ague, along with the fact of the occurrence of the pain independently of the ingestion of food, arsenic is almost sure to cure the case. (Dr. A. Leared, p. 76.)

Dr. Norris, of Stourbridge, relates his own case as illustrative of the use of arsenic in neuralgia of the abdominal viscera. He had suffered from abdominal pain, coming on generally during digestion, and was in the habit of obtaining relief by a few drops of laudanum or a cigar. The pain left him under the use of arsenic, in doses of five or six drops twice a day. (Dr. Norris, p. 80.)

Inguinal Hernia.—*Position in the Reduction of.*—Direct the patient to stand up, and place yourself (also standing) behind him, and encircle his body with both your arms, grasping the tumour with both hands, and you will probably effect in a few moments what you would have failed to accomplish in as many hours with the patient lying down. This has been so successful that the author has never yet had a case which he could not reduce. Femoral hernia, he observes, cannot be reduced when the patient is erect. (Dr. J. B. Bond, of Yarmouth, Nova Scotia, p. 123.)

Obstinate Constipation.—*Faradisation of the Bowel.*—Cases of habitual constipation are generally extremely difficult of cure, every remedy after a time losing its effect. In such cases faradisation of the bowel ought to be employed, as it offers a good prospect of producing permanent good effects. For this purpose an insulated metallic sound must be introduced into the rectum, a moistened conducter connected with the positive pole being placed on the abdomen. The top of the metallic sound only is free for about an inch, and at the other extremity it should be connected with the negative pole of the primary faradic current. The current which should be of moderate power, may be allowed to pass for fifteen minutes at a time, and the process repeated twice a week. It is not well to discontinue aperients altogether at first (Dr. J. Althaus, p. 123.)

Pain after Food.—*Black Oxide of Manganese.*—When there is gastric pain, worse after ingestion of food, a sense of heat, epigastric tenderness, and redness of the tip of the tongue, the purified black oxide of manganese is a superior remedy to bismuth. In a few cases, however, the irri-

tability of the gastric mucous membrane is continued in that of the small intestines. The pain is then associated with a tendency to diarrhœa, and the astringency of bismuth becomes valuable. (Dr. A. Leared, p. 79.)

AFFECTIONS OF THE URINARY ORGANS.

ACUTE NEPHRITIS AND CHRONIC ALBUMINURIA.—From its power of contracting dilated blood-vessels belladonna holds out prospect of being of use in the above-named affections. In chronic albuminuria, of course no benefit can be expected when fatty degeneration has commenced. (Dr. J. Harley, p. 268.)

ALBUMEN IN THE URINE.—There is no better mode of testing for small quantities of albumen than that of allowing a small quantity of urine to trickle slowly down the side of a test-tube upon about half a drachm of fuming nitric acid. (Dr. A. Clark, p. 82.)

DIABETES.—Give half drachm doses of the etherial solution of peroxide of hydrogen, mixed in an ounce of distilled water, three times a day. We possess in peroxide of hydrogen a ready means of destroying, by oxidation, the sugar in the blood, and of also maintaining the animal heat, which in the treatment of diabetes is an important consideration. What is sold by Mr. Robbins as Dr. Richardson's ozonic ether is in reality a solution of peroxide of hydrogen in ether. A most interesting case is related showing the gratifying results following this treatment. Before commencing it the patient had been passing as much as five quarts of urine each night, and the thirst was intense. The last report states that the quantity was reduced to twenty-four ounces, and that the thirst was quite relieved. (Dr. J. Day, Geelong, Australia, p. 81.)

ENLARGED PROSTATE.—One man in every three who dies above the age of fifty-five has some enlargement of the prostate, but only one in seven has any symptoms of it, and probably only one in ten requires treatment for it. The size of the prostate does not influence the degree of obstruction to the urethra; it is the amount of enlargement of the middle lobe which is the matter of import. For ordinary use in prostatic cases the English gum-elastic catheter is by far the best. The point is generally not curved as it should be. The instrument should be well curved to the very point: with this object it may be kept on an over-bent stylet for a month; it will thus pass easily without a stylet, for the tendency to over-curve which the instrument possesses will ride it over the obstruction into the bladder. The general treatment of prostatic enlargement consists mainly in preventing local congestion, the most frequent cause of which is the undertaking long journeys, and exposure to cold. (Sir H. Thompson, p. 142.)

It is a peculiarity in the frequency of micturition which exists in hypertrophy of the prostate, in common with all serious urinary diseases, that it is more urgent at night than during the day. This is an aid in diagnosis, and should be made a point of inquiry in investigating urinary diseases. (Sir H. Thompson, p. 126.)

Diagnosis between Prostatitis and Calculus.—Pain in micturition from prostatitis is towards the end of the act of micturition, when the bladder begins to contract on the tender prostate; but when the pain is from an inflamed bladder, it is at the commencement of the act of micturition. Pain from calculus in the bladder also occurs towards the end of micturition, when the rough surface of the stone comes in contact with the mucous membrane. Pain from this cause, however, is very characteristic, it is increased by movement and is felt very acutely about the base of the glans penis. Pain from prostatitis also sometimes occurs in the penis, but is less severe, and is not much increased by riding in a rough-going vehicle, as is that from stone. (Sir H. Thompson, p. 126.)

There is one point of great service in the diagnosis between prostatitis and calculus, especially if we wish to avoid the passage of a sound. Tell the patient to pass a little urine so as to wash out the urethra, set that aside in a

vessel, and then let him empty the bladder into a second vessel. If the first only is thick from pus or mucus, whilst the second is clear, it is prostatitis; if the second is turbid, the cause of the turbidity is in the bladder, and may be stone. (Sir H. Thompson, p. 127.)

FREQUENCY OF MICTURITION.—Whenever the natural characters of the urine are altered before it reaches the bladder, the secretion produces irritation. Diluted or watery urine is often regarded as unirritating; on the contrary, it is not generally well retained by the bladder. In pyelitis, and in almost all organic changes of the kidney, in Bright's disease, and in diabetes, there is frequency of making water. The bladder is always irritated by the pale urine passed by hysterical patients. (Sir H. Thompson, p. 126.)

Precaution in Examining the Urine.—Always first flush the urethra and take the urine in a separate vessel, say a wine-glass, and let the rest of the urine be passed into another vessel. If this is not done we get any matter from prostatitis or urethritis mixed up with the whole quantity of urine. A case came under Sir H. Thompson's observation which had been treated by a physician for months as pyelitis, whereas when the urethra had been flushed the rest of the urine was quite healthy, proving that the pus came from some part of the urethra. (Sir H. Thompson, p. 127.)

LITHOTOMY AND LITHOTRITY.—It may be affirmed as a maxim that between puberty and the age of fifty lithotrity is the most suitable and the selected operation. Even above the last named age a majority of cases will be most suitably treated by this method; and if any between puberty and extreme old age be rejected or set aside for lithotomy, it must be for certain unfavourable conditions and indications which, if likely to increase the dangers of lithotrity, will most assuredly have a similar effect in lithotomy. In other terms, it may be said that all the cases of stone in the bladder in the adult, with complications, are now set aside for lithotomy; whilst all the simpler cases most promising of success are treated by lithotrity. (Sir W. Fergusson, p. 161.)

It is of the greatest importance to determine the nature and size of a stone; for now that we have two operations for its removal, if we crush the large stone and cut for the smaller one we shall have a greater mortality than if we performed lithotomy in all cases as was formerly done. The rule may be accepted that all stones, under puberty, with very few exceptions, are to be cut, unless they are very small and can be crushed, say, in one operation. All the cases above puberty are to be crushed, with certain rare exceptions. The first exception is in a case of oxalate-of-lime calculus, which is, let us say, an inch in diameter. Under an inch in diameter you may crush an oxalate-of-lime calculus. A stone which is two inches in diameter, either phosphatic or uric acid, had better be cut. No doubt a rather larger phosphatic stone may be crushed, for it is very friable. No doubt it is possible to crush any stone; but the number of sittings required and the amount of irritation produced, prevent its being done successfully. If there is a bad stricture of the urethra, or a diseased state of the bladder, it is better not to crush. Hypertrophy of the prostate need not interfere with lithotrity, it is only a question of delicate manipulation. (Sir H. Thompson, p. 159.)

LITHOTOMY.—The internal incision is of more consequence than the external, although all are agreed that the latter should be free. For some years Sir Henry Thompson states that he has operated as recommended by Erichsen (after the original operation by Dupuytren), by incising both lobes of the prostate, in order to obtain room when the stone is large, and has found the plan satisfactory. (Sir H. Thompson, p. 167.)

Celsian Operation for Stone in the Bladder.—The oldest description of any lithotomy operation we have is that of Celsus, who made a semilunar incision, the concavity of which embraced the rectum. This plan of operating has been lately revived by the author. He commences the operation with what he styles "the famous, but antiquated, if not despised, semilunar incision of *Celsus*;" but as regards the deep parts of the wound and prostate, opens the bladder as in the ordinary lateral operation. The great advantage of this free external wound is the ease with which the finger can be introduced

deeply into the wound, above the knuckle if necessary, whereas in the ordinary small incision, the finger is arrested at the web between it and the second finger. There is also more room for the introduction of instruments, whereas when the ordinary lateral incision is made the finger seems quite surrounded by the edges of the wound, and the instruments are introduced with less ease, and consequently cannot be as satisfactorily used as when the external wound is more ample. The most frequent cause of failure in lithotomy is too short an external incision. (Sir W. Fergusson, p. 161.)

Mr. Erichsen, in an article drawn out by that from which we have taken the preceding synopsis, says that he quite agrees with Sir W. Fergusson in the adoption by him of the crescentic external incision after Dupuytren, but makes the practical suggestion that it is better also to divide the prostate by a transverse incision, cutting into both lobes. This may be done with great certainty by means of a lithotome caché, which cuts as it is withdrawn. The advantage of this is that a large internal incision can be made if necessary without risking cutting beyond the prostate. The use of the rectangular staff is recommended in preference to the ordinary curved staff, whatever operation is performed. (Mr. J. E. Erichsen, p. 170.)

Medio-Bilateral Lithotomy.—Mr. Jackson, of Wolverhampton, uses a broad straight gorget, with a bilateral cutting edge, for the purpose of dividing the prostate bilaterally. This gorget terminates in a probe point, which is lodged in the deeply grooved curved staff previously introduced into the bladder. During the division of the prostate the fore-finger of the left hand is placed firmly in front of the prostate. (Mr. V. Jackson, p. 167.)

Mr. Hutchinson bears testimony to the advantages of median lithotomy with bilateral section of the prostate. The knife, or rather gorget, he uses, is similar to that of Mr. Jackson, but he uses a rectangular staff which, at the end of the groove, has a little thimble to receive and retain the probe point of the gorget. Instead of making the gorget move on a staff which is fixed, he passes the probe end of the former forwards into the thimble, and having thus locked them both firmly together, he then takes the staff and passes both together forwards and upwards into the bladder. The ease and certainty with which an operation of this kind is accomplished, together with the certainty of avoidance of injury to important parts recommend it to general adoption. (Mr. J. Hutchinson, p. 168.)

Sounding for Stone in the Bladder.—The instrument used for sounding for stone should have a small, short, curved beak, because it can be turned in every direction. It is very easy indeed to miss a small stone when an instrument with a large curve like a catheter is taken; indeed it is probable that more stones were missed with such instruments than were found. A big stone can be found with any sound, but the great object is to find the small stones. The sound may be provided with a little slide on the staff, which by proper manipulation enables you to ascertain very nearly the size of a stone. A phosphatic calculus when in the bladder gives a dull note when struck; whereas the uric acid one gives a hard ring. To determine whether there are more stones than one in the bladder seize one with a lithotrite, and then move it about as a sound for others. (Sir H. Thompson, p. 177.)

Stricture of the Urethra —*Diagnosis.*—When a case of stricture, or supposed stricture, is first examined, a fair sized instrument must be first taken, say No. 8 or 9. If there is a stricture you ascertain its position but do not expect the instrument to pass, whereas if there is no stricture it passes at once. When the instrument is passed through the first few inches of the urethra, it must be kept well on the floor of the canal in order to avoid the lacuna magna. The second place in which a mistake may be made is at the commencement of the membranous portion of the urethra, or rather, in the bulb, where the canal is wider and more distensible than in the membranous portion. It is the lower part of the bulb which bags out and is so distensible, and consequently it is here that the point of the instrument is liable to catch, and, if force is used, a false passage to be made; consequently the rule in this part is to use an instrument with the point pretty well turned up

and to keep it on the roof instead of on the floor of the passage. The last place at which there may be difficulty in passing an instrument is at the neck of the bladder. We used to hear a good deal about "stricture of the neck of the bladder"; there is no such thing. It is simply because there is sometimes difficulty in passing the neck of the bladder that it came to be regarded as a locality of stricture. In this case also a well curved instrument is the best thing to go in. (Sir H. Thompson, p. 133.)

There is no such thing as "impermeable" stricture. It is a contradiction in terms. Stricture is a *narrowing* of the canal; it is not an obliteration of it. A stricture always admits urine, more or less in quantity, to pass through it, and whenever urine passes outwards through a stricture, an instrument ought with care and perseverance to be got in. (Sir H. Thompson, p. 131.)

Flexible versus Solid Instruments for Dilatation of Stricture.—Flexible instruments are very much preferable to solid ones for dilating a stricture. They pass more easily, and consequently with less pain, and will insinuate themselves where a solid instrument would fail. This latter property is especially seen in the French instruments, which are so flexible that you can wind them round the finger, and possess a tapering point slightly bulbed at the end. This instrument passes very readily, almost without your knowing it, and it requires no knowledge of anatomy to use it. (Sir H. Thompson, p. 135.)

To pass a Bougie through a Narrow and Tortuous Stricture.—Do not rely upon mere groping to find the orifice. You should always adopt some method—any method you please, provided that it shall seem to you exhaustive of the different modes of exploring the urethra. Take a small instrument and slide it down first the roof of the canal, because that is the firmest part, and by following it you are most likely to carry the point in. The floor, on the contrary, is the softest, loosest, and most spongy part, and will be most likely to yield to the instrument and give way. If your first effort does not succeed, take the right side; if that does not do, take the left; if that does not do, take the floor. There is no other method so calculated to help through a difficult stricture. If you are very careful, you may make the attempt in this manner for twenty or thirty minutes without doing any damage. (Sir H. Thompson, p. 133.)

Injection of Oil in Stricture.—When you have a very narrow stricture to deal with, it is sometimes of use to throw half an ounce or an ounce of olive oil into the urethra, holding the meatus well round the syringe. By this plan the passage is distended, and you may sometimes succeed in passing an instrument when you have been unable in any other way. The oil must be allowed to escape during the introduction of the catheter, and the plan must not be tried at all if there is a false passage. (Sir H. Thompson, p. 139.)

Stricture of the Urethra with False Passage.—When you have a case of stricture with false passage to treat, remember that the false passage commences usually on the floor, and generally at the bulbous portion of the urethra. When the instrument is in the false passage you can tell at once that it is so by introducing the finger into the rectum when only the thin coats of the bowel will be felt between the finger and the instrument. If the instrument is in the right passage, you feel the whole thickness of the prostate, not always very considerable, between it and your finger, still quite enough to show that you are in the right path. If you find the catheter goes into a false passage withdraw it an inch or two, and in passing it again keep as close along the upper part of the urethra as you can, ascertaining by means of the finger that the instrument is not passing into the old route. (Sir H. Thompson, p. 135.)

Fastening the Catheter in.—Having succeeded in introducing a catheter with great difficulty, should it be removed or left in? It should be kept in for two or three days before removing it, and, if a metal instrument be used, it may then be exchanged for a gum-elastic one. (Sir H. Thompson, p. 139.)

Over-Dilatation of the Urethra.—It is certain from observations made by *ejecting* the bladder and urethra with wax, that even in subjects of large *the* portion of the urethra begirt by the triangular ligament rarely ex-

ceeds the dimensions of a No. 10 catheter. To dilate, therefore, beyond No. 10 or 11, is to dilate beyond the normal calibre of the canal; the urethra is kept in a state of irritation by this, and generally resents such treatment. (Dr. R. M. M'Donnell, p. 156.)

Contractile Stricture.—How are we to treat a stricture which after dilatation recontracts to its original size? In one of three ways. We may rupture, over-distend, or cut the unyielding and contractile fibres which constitute the stricture. Urethrotomy is perhaps the most generally employed, and internal oftener than external urethrotomy. But the rule is generally this, that the nearer the stricture to the meatus the safer and more useful is it to cut; the deeper seated the stricture the more desirable is it to over-distend. In performing internal urethrotomy the bistourie-caché must be passed half-an-inch beyond the stricture, and, the knife being unsheathed to the desired extent, the stricture is divided by drawing the instrument forwards. After this a catheter must be tied in for forty-eight hours, and after that an instrument passed every third day, then every week, and so on. The plan of over-distension is to be preferred to that of rupture in those cases in which it is not thought advisable to divide the stricture, and for this purpose the instrument devised by the writer answers every end. It consists of two blades separable to any extent at one part by means of a screw in the handle. This part of the instrument is placed in the stricture. Strictures situated in the bulbous part of the urethra are more suitably treated by over-distension than by cutting. (Sir H. Thompson, p. 140.)

Use of Caustics in Contractile Stricture.—Sir Henry Thompson, than whom there is no greater authority on subjects of this nature, condemns entirely the use of potash and other caustics in cases of contractile stricture. He believes them to be unnecessary, undesirable, and dangerous. (p. 139.)

Mr. Le Gros Clark states that for many years he has been in the habit of employing potassa fusa in the treatment of old and impenetrable strictures, with the greatest advantage, and without in a single instance causing dangerous consequences. A suitable instrument, with care in using it, together with a little patience, are requisite; but these are elements of success in every operation. (p. 152.)

Retention of Urine from Organic Stricture.—First try to pass the finest gum-elastic catheter, and if successful tie it in; if it cannot be passed try a No. 1 silver catheter. Efforts to pass instruments should not be persevered in too long, and great care must be exercised that no injury is done to the urethra. If unsuccessful, put the patient into a warm bed, apply hot fomentations to the parts, and give opium very freely so as to relieve the involuntary straining efforts which make the case worse, and afford no relief. By this means the dribbling of urine will probably become more free, and in two or three days it will be found that a catheter can be passed. If, however, these means fail, and a greatly distended bladder be felt above the pubes, we are necessitated to puncture the bladder. The plan of opening the urethra behind the stricture has been now nearly abandoned, as it is extremely difficult to hit that passage, and a cutting operation at all generally becomes unnecessary if the urethra can be allowed to lie fallow for a few days. Mr. Cock, of Guy's Hospital, has had more experience than any one else in puncturing the bladder from the rectum. The finger of the left hand must be passed beyond the prostate until it reaches the bladder behind it, and the trochar glided along the finger to the point determined to puncture. This point is ascertained best by fluctuation being very distinct when transmitted from above the pubes by the other hand placed on the abdomen. A catheter can generally be passed a few days afterwards, and the stricture permanently cured. (Sir H. Thompson, p. 148.)

Retention of Urine in a young and healthy man without Stricture.—The history of such cases generally is that the retention came on in consequence of the patient sitting on a cold stone or damp grass when overheated by exertion, or of his indulging in some emotional excitement. He will also tell you that he has had gonorrhœa at some recent period. The cause is inflammatory swelling of the prostate; its condition resembling that of the tonsils in

inflammatory sore throat. The old treatment was bleeding and the warm bath; this however is bad, and must not be relied on to procure relief. The best plan is to take a moderate sized gum catheter (one not larger than a No. 6, as a large one gives in these circumstances unnecessary pain) which has been kept for some time on a stylet, which has been overbent. In this manner there is generally no difficulty in relieving the patient. (Sir H. Thompson, p. 147.).

Vulcanised India-Rubber Catheters.—In cases of retention of urine without stricture, in old men, vulcanised india-rubber sounds are preferable to the ordinary catheters. They are so flexible and supple that they seem to find their own way, and catheterism, so to speak, performs itself, even in cases where there exists considerable enlargement of the prostate. (M. Richet, p. 157.)

Extravasation of Urine.—What happens when extravasation of urine occurs from rupture of the urethra behind a stricture? From the anatomical disposition of the fascia, the urine passes into the scrotum, up into the groin above Poupart's ligament, and towards the belly. The urine cannot pass backwards behind the scrotum neither can it get into the thighs. It does occasionally get as high as the chest. In such cases do not be afraid of the knife. On each side of the perineum make a good deep incision, which need not be limited to two or even three inches, because you are cutting into urine and not flesh. The incisions generally bleed rather freely. An incision should be made on each side of the penis, because if it is made in the middle line there is not sufficient communication for the incision on one side to relieve the other. When the bladder has been relieved in this way the urine continues to drain off by the incisions, and the stricture begins to improve so that in three or four days a catheter can be passed. (Sir H. Thompson, p. 150.)

Simple Urinary Fistula.—Whatever part of the canal a simple urinary fistula is connected with it almost always heals if the stricture is dilated, and no other treatment is required. Sometimes the external openings are numerous and the intervening tissue much indurated. These cases generally improve a good deal by dilating the stricture well, but sometimes they are not cured by that. The external openings of the fistula should then be well enlarged so that the urine may not be detained in the parts any longer than necessary. It may be necessary to excite inflammation in the track of the fistula by a hot wire, or by touching it with nitrate of silver. If the case is not cured by these means the patient must be taught to pass a catheter for himself, and told to use it every time he makes water. The catheter must not be tied in, for the urine by capillary attraction always passes between the outside of the instrument and the wall of the urethra, and so reaches the fistulous opening. (Sir H. Thompson, p. 151.)

SUPPRESSION OF URINE.—*Belladonna.*—Belladonna will probably prove a remedy of value in cases of suppression of urine, whether accompanied with uræmic symptoms or not. As both the sluggish circulation and the torpid kidney are simultaneously aroused by the medicine, there is ground for expecting a restoration of the renal secretion. (Dr. J. Harley, p. 268.)

External use of Digitalis.—When fresh digitalis leaves, bruised, are applied as a hot poultice the digitalis is rapidly absorbed, so much so as to reduce the rapidity of the pulse by twenty beats a minute, and sometimes more. This renders it a remedy of great value in cases of suppression of urine. In these cases the pressure on the renal circulation is relieved, and the urinary secretion rapidly re-established. Some very interesting cases illustrating this are related. (Mr. J. D. Brown, p. 172.)

URETHRAL DISCHARGES.—If a patient has a urethral discharge, excluding gonnorhœa, such discharge is generally from the prostate, where it may be connected with prostatic calculus. But Cowper's gland or duct may be the seat of abscess, which is to be felt in the perineum, or an abscess in the walls of the urethra or deep in the perineum may cause pus to escape from the urethra. Such discharge, however, is most commonly from the prostate where it may be connected with prostatic calculus; if so, the prostate will

be found tender on pressure, and by pressing afterwards on the urethra a little of the discharge may be worked up to the meatus and obtained for microscopic examination. The presence of a discharge of this kind causes a little albumen to exist in the urine, but no mistake in diagnosis should be made on this account. (Mr. J. Hilton, p. 110.)

URETHRAL HEMORRHAGE.—Blood from the urethra never enters the bladder; consequently blood in the bladder signifies hemorrhage from the kidney or bladder itself. (Mr. J. Hilton, p. 109.)

URINARY PIGMENTS.—If healthy urine be allowed to trickle drop by drop, down the side of a test-tube containing about half a drachm of fuming nitric acid, and inclined at an angle of from 50 to 60 degrees, a yellow ring will form at the line of contact of the two fluids, and over this a narrow ring of an almost ruby-red colour. When bile is present there is also a green ring, and in certain morbid states of the system there is a purplish, bluish, or absolutely blue ring. In nearly every case of tropical dysentery a decidedly blue ring is produced so long as the disease is progressive, and the ruby-red ring reappears when the patient begins to recover. In any case where a blue ring exists it may be taken for granted that the case is taking a downward course. The depth of colour may be taken as an indication of the amount and rapidity of the destruction of the colouring matter of the blood, in short of the amount of waste going on. (Dr. H. Veale, p. 82.)

FRACTURES, DISLOCATIONS, OPERATIONS, AND DISEASES OF BONES AND JOINTS.

ANTISEPTIC PRINCIPLE OF TREATMENT IN SURGERY.—The preparations of carbolic acid in use in surgery, may be said to be three, carbolic oil, carbolic lotion, and carbolic paste. The composition of the first is carbolic acid and boiled linseed or other fixed oil, in the proportion of one to five; that of the second, carbolic acid and water in the proportion of one to thirty, and that of the third, carbolic oil with whitening, in the proportions requisite for the consistence of soft putty. The results of the employment of these preparations in surgery are almost to be called wonderful, and promise to remove and prevent an immense amount of human suffering. In a case of removal of a parotid tumour the wound was sponged with carbolic lotion, the edges stitched together with silver sutures and a plaster of carbolic paste applied. On the third day the wound was quite healed. In a case where the knee-joint was laid open by a scythe, the wound was freely sponged out with carbolic oil, the edges brought together, and a pledget of lint soaked in the oil laid over the wound. On the fourth day the wound was perfectly healed. Cases of compound fracture, psoas and mammary abscess, are treated most successfully by the same plan, which promises to create quite a revolution in surgery. (Prof. Syme, p. 93.)

Antiseptic Applications to Wounds.—It is probable that further experience will decide that it is unnecessary to apply carbolic acid in caustic strength to the interior of wounds; and that it will be found that the use of a solution of the acid in water or oil of sufficient power to act simply as an antiseptic, will prove sufficient for the purpose intended. (Mr. W. Adams, p. 95.)

DISLOCATION OF THE HIP BACKWARDS.—*Reduction by Manipulation.*—The way to reduce dislocation of the hip backwards is not by abducting or rolling the limb outwards as has hitherto been practised, for if this is done the head of the bone is almost certain to roll under the acetabulum to its inner side. A better plan is the following: Bend the thigh on the abdomen, and then slowly move the limb into a straight line with the body, so that the head of the bone can be felt projecting in the buttock. It will be seen by examining the skeleton that the head of the bone is now in the groove above the outer side of the tuber ischii. Here it is opposite the least prominent part of the lower edge of the acetabulum, and if the femur is depressed whilst

in this position the head easily slips into the socket. To effect this, draw the limb forward from the abdomen, and then force it downwards or extend it. (Mr. G. W. Callender, p. 86.)

FRACTURE OF THE THIGH.—*Continuous Extension.*—The author describes fully the plan of treating fracture of the thigh by continuous extension by means of a weight and pulley. He says, "I have treated fractures of the thigh in all the various sites of fractures, and the result has been uniformly satisfactory." (Mr. L. Tait, p. 84.)

FRACTURE OF THE PATELLA.—*Treated by a Ring.*—Take a measure of the sound patella and have a ring made of iron (allowing for padding). which pad well with cotton wadding cut in strips and wrapped round the ring, over which a bandage can be applied. To each side of the ring sew strips of bandage. Then place a well-padded splint to the posterior aspect of the leg and thigh, which secure by a few turns of bandage at the lower and upper ends, the bandage being loose so as not to interfere with the circulation. Next bring the two fragments of bone into apposition, place the ring around the patella, and tie the strips of bandage over the splint, thus securely holding the ring in its place, and keeping the broken bone always in complete apposition, thereby giving the greatest possible chance for a bony union. (Dr. W. A. Gibson, Dr. P. Eve, p. 85.)

HARELIP.—By passing two or more curved needles, armed with silk sutures, from the raw edges immediately *under* the skin, carrying them through the whole thickness of the lip and tying them *inside*, we may effectually close the wound and run no risk whatever of those unsightly scars and marks which so generally result from the usual mode of applying harelip pins or sutures externally. (Mr. A. Duke, p. 108.)

At what Age should an Operation be undertaken for the cure of Harelip?—The author considers that the age of earliest infancy should be avoided, but that the operation should be performed before the period of dentition. Even when a child cannot suck on account of the deformity, it is better to feed it with the spoon for a short time, until the parts have become larger and more fleshy. Again, although the mortality from this operation is very small, yet it is less in children who are a few months old than in those newly born, and again, many infants with harelip die from causes unconnected with this deformity during the first few weeks of life, thus dispensing with the necessity of an operation. On this question Sir W. Fergusson, than whom no one has a larger experience, thus expresses himself: "The earlier the operation is performed the better; assuredly before teething. I decidedly prefer before the end of the first month in a healthy child, though the operation may be performed any time earlier to within a few hours of birth." One advantage of an early operation is that the child does not get into the habit of thrusting the tongue into the hiatus between the bones. (Mr. T. Smith, p. 106.)

PENETRATING WOUNDS OF JOINTS.—The principles upon which these serious injuries must be treated are as follows: the immediate closure of the wound, the adoption of perfect repose of the body, and immobility of the injured member, the local application of ice, and a moderate but nourishing diet. (Mr. J. Birkett, p. 89.)

TAPPING WITH THE AID OF SIPHON-POWER.—This may be easily applied in the following manner. Have a short piece of tube soldered on to the head of the canula about to be used, this piece being furnished on the outside with a screw; then let a similar piece, screwed so as to fit on to the screw of the canula, be inserted into the end of a piece of india-rubber piping about four feet long. This tube before the operation must be filled with water, and the contents retained by tying a knot on the extremity. When the trochar and the canula have been introduced and the trochar withdrawn, the tube being attached to the canula, the weight of the column of water contained in it will considerably facilitate the withdrawal of the fluid from the body. (Dr. W. Roberts, p. 105.)

TUMOURS.—Treatment by Electrolysis.—The electrolytic treatment of tumours, which is now upon its trial, acts in a threefold manner, viz., first,

through mechanical disintegration of the tissues by the nascent hydrogen; second, through chemical destruction by free alkalies, potash, soda, and lime, which are evolved at the negative pole of the battery; and lastly, through a modification of nutrition. The positive pole is never to be introduced into the tumour operated upon, but always placed on the adjacent skin, whilst the negative pole, which should terminate in a needle or number of needles, is introduced into the interior of the tumour. The action of the electricity is thus upon the internal parts of the tumour, and all the risk of surgical operations is done away with. When the tumour is large the current should be passed through it from fifteen to thirty minutes at a time and repeated every day. For smaller tumours a less time will suffice. The results so far have been very encouraging. Nævus, bronchocele, and other innocent tumours have been destroyed, or rather, wasted with success. The results in the treatment of malignant disease are of course not so satisfactory, but more experience is still required on this point. A battery suitable for the purpose may be obtained from Weiss, weighing 36 pounds, in a neat case, and only requiring a little attention every three or four months. (Dr. J. Althaus, p. 102.)

Dr. Althaus has now treated fifty-two non-malignant and eleven malignant tumours by electrolysis. He finds bronchocele, nævus, and the allied forms of tumour more favourable for this treatment than lipoma, enchondroma, and osseous tumours. Nævus is very successfully treated in this way, and in cases where the situation, size, or state of health of the patient counterindicate other plans of operation, this is especially called for. A case of bronchocele of large size, which many surgeons of eminence had pronounced incurable, is shrinking fast under the influence of the electrolytic treatment. The result of this treatment in cancer is not favourable. (Dr. J. Althaus, p. 102.)

Dr. Collis, of Dublin, does not use electricity for a short time daily, as recommended by Dr. Althaus, for the purpose of destroying tumours, but for lengthened periods. He employs a weak current, generated by batteries made of small cylinders or plates of wood covered with felt, and wrapped round with zinc and copper wire. These may be excited by salt and water, and carried about with the patient, tied up in gutta-percha or oiled silk. The tumour should be covered with a plate of zinc or copper which must be connected with the positive pole of the battery, whilst the negative pole is placed on the skin in some other part of the body. (Dr. M. H. Collis, p. 103.)

Vertical Amputation of a Finger.—In cases of vertical amputation of a finger by accident from machinery or otherwise, the healing may be much expedited by the following plan. Pass a strip of adhesive plaster firmly round the stump, having four loops of wire so placed round it that two of the loops shall be on the dorsal, and two on the palmar aspect, and having their eight free ends projecting beyond the stump. Then apply a larger piece of plaster over the other so as to give firmness, and twist the ends of the wires together so as to draw the skin forwards and partly cover the wound. (Dr. S. W. Poole, p. 202.)

Wounds.—*Dr. Richardson's Styptic Colloid.*—I have now, for more than a year, used Dr. Richardson's colloid styptic in a large number of cases of incised and lacerated wounds, some of formidable dimensions, with complete success in a large proportion of cases. In two-thirds of the cases so treated, I can with confidence assert that union by the first intention has been obtained; or that the reparative process has proceeded either without suppuration, even in bad cases, or with the suppurative process reduced to a very insignificant amount. And in no instance have I seen any injurious effects. Mr. W. Adams, p. 99.)

It appears to me, from the wounds I have treated with it, to be particularly applicable to clean-cut wounds, where the surfaces can be brought accurately together; but the styptic is certainly often successful in obtaining union without suppuration in lacerated and often contused wounds, checking at all times, and often preventing suppuration in a wound which otherwise treated would be nearly sure to suppurate. (Mr. P. Hopgood, p. 100.)

When all is solidified the dressing becomes a concrete, having a true organic hold or basis on the tissues, and rendering the wound almost as good as subcutaneous. (Dr. B. W. Richardson, p. 99.)

AFFECTIONS OF THE SKIN, ETC.

ACNE.—The following ointment is serviceable in all the forms of acne. Washed lard, 50 parts; sublimed sulphur, tannin, of each four parts; laurel-water, five parts. The proportions of sulphur and tannin may be increased according to circumstances, to six or eight parts. (Dr. Rodet, p. 197.)

BURNS.—*Richardson's Styptic Colloid.*—Burns, which are both troublesome, offensive, and painful in the dressing, are by the use of the "colloid" deprived of half their terrors. When profuse suppuration sets in, if painted freely over the surface, it at once removes the fœtor, protects the granulations, and the wound speedily heals. The first dressing is painful, but not the others; with moderate sized wounds the first dressing is also the last: Nothing further is required but daily painting with the "colloid." (Dr. J. Lowe, p. 24.)

Carbolic Acid in Scalds and Burns.—Mix carbolic acid with the ordinary oil and lime water emulsion in the proportion of one part to thirty. Linen rags should be saturated in this and spread on the scalded parts. These should be kept moist by smearing them over frequently with a feather dipped in the emulsion. The whole should be covered with oiled silk or gutta-percha tissue." (Dr. A. Wilson, p. 201.)

Apply closely over the whole surface of the burn two folds of lint dipped in a liniment, of one part of carbolic acid to six of olive oil. A double layer of tinfoil should be placed over the lint, and the whole secured by means of a bandage. The effects so far as they can be told from one case related by the author are very remarkable—viz., complete cessation of pain and healing without the formation of any pus. (Prof. Pirrie, p. 200.)

CYSTIC BRONCHOCELE.—The diagnosis of cystic bronchocele from the cysts formed in the areolar tissue of the neck, called "hydrocele of the neck," is easy. Tell the patient to swallow, and if it is bronchocele it will move up and down with the windpipe; if hydrocele of the neck, it will remain stationary. (Mr. T. B. Curling, p. 209.)

The cure of cystic bronchocele may be effected in three ways; by excision, seton, and injection. The real question however lies between the two latter. When iodine is used, about a drachm or a drachm and a half according to the size of the cyst may be thrown in and left there. This acts very satisfactorily in some cases, but is not so well adapted as the seton to the cure of cases where the cyst is much thickened. There is another advantage in the seton over injections—that it is free from the risk attending the rapid enlargement of the cyst, and the confinement of matter which has been known to prove fatal, as there is always an outlet for the escape of fluids. Upon the whole, the seton is the more valuable plan of treatment and the one most generally applicable to these cases. (Mr. T. B. Curling, p. 210.)

FOUL AND CHRONIC ULCERS.—*Richardson's Styptic Colloid.*—In old chronic ulcers, foul-smelling and filthy, and whether syphilitic or not, the odour may be at once destroyed and healthy granulations established by the use of Richardson's styptic colloid. (Dr. J. Lowe, p. 26.)

LUPUS.—The only internal remedies of undoubted value in lupus are arsenic and calomel in lupus of the face, and iodide of potassium in lupus of the limbs. Cod-liver oil, as also chloride of barium, produce often a degree of improvement but it is uncertain. Among remedies of no value we may rank steel, quinine, mineral acids, bitters, sedatives, change of air, and sea-bathing. With very few exceptions the external applications recommended in lupus are useless and impracticable. For instance, soothing or *cooling* dressings, and ointments of every kind are perfectly useless. The *effects* of iodide of potassium in lupoid ulceration of the limbs is very

marked, a drachm weekly is generally sufficient. No remedy so rapidly checks quickly spreading lupus as calomel; no harm ever ensues from its proper employment, and it should not be given so as to produce any action on the gums. As a local application perhaps no remedy is more useful than the acid nitrate of mercury. (Mr. J. L. Milton, p. 193.)

PRURIGO.—*Arsenic.*—The great internal remedy in prurigo is arsenic. Its value is chiefly seen in severe and long standing cases. Strychnia in doses of the sixtieth of a grain, and cod-liver oil are of use, as is also the Turkish bath when the patient is not unsuited for it by feeble heart or general debility. (Mr. J. L. Milton, p. 203.)

Bromide of Ammonium.—The author relates a case of prurigo in an elderly woman, which she attributed to mental anxiety and bad food. Various remedies had been tried without success, including the subcutaneous injection of morphia. Bromide of ammonium in doses of ten grains three times a day was then administered, and gradually increased to twenty. The symptoms gradually disappeared after this, and a perfect cure resulted in four to five months, although the disease had existed six years. (Dr. H. Purdon, p. 207.)

SMALL-POX.—*Carbolic Acid to prevent Pitting.*—The author recommends the application over the face of an ointment made of carbolic acid, two drachms; mutton suet, two ounces; and coloured with lamp-black. This should be spread thickly upon black cotton wadding, and applied over the face and forehead, holes being cut for eyes, nostrils, and mouth. This mask should be changed every second day, and the face gently washed with soap and warm water. In a case related, this quite prevented pitting, and the suppuration was much diminished in amount and duration. (Dr. H. Yates, of Kingston, Canada, p. 200.)

WARTS AND CANCER.—There seems to be truth in the opinion that there is some relationship between warts and epithelial cancer. The similarity between the two at their commencement, and the frequent occurrence of them in the same individual, both confirm the idea. The application of the acid nitrate of mercury is the best way of getting rid of warts. (Mr. J. Hutchinson, p. 211.)

VENEREAL AFFECTIONS.

BUBO.—In the treatment of threatened bubo every endeavour should be made to prevent suppuration altogether, or to put it off as long as possible, and to confine it within the narrowest limits. If any matter forms it should be evacuated at once by free incision, as this often prevents the necessity of subsequently opening troublesome sinuses. (Mr. Durham, p. 216.)

SYPHILIS.—*Iodide of Potassium.*—Iodide of potassium in large does (*i. e.*, from thirty to seventy-five grains daily) is almost a specific for the cure of large spreading tertiary, or late secondary, ulcerations of the skin, such as those so common and generally so intractable, affecting all parts of the body, and which often follow an eruption of rupia. For those late syphilitic eruptions which affect the nose so disastrously, and often so rapidly, these same doses are equally valuable. Iodism is exceedingly rare in presence of tertiary syphilis; and large doses do not, as a rule, occasion loss of flesh. If they cure the syphilis the patient usually gains weight under their influence. The iodide of potassium is however as undesirable in primary syphilis and in early secondary symptoms, as the iodide of mercury in *small* doses is valuable at that period of the malady. (Sir H. Thompson, p. 217.)

Iodide of Potassium with Iron.—It will be constantly found that iodide of potassium given in small or moderate doses, fails to arrest the ravages of ulcerative secondary or of tertiary syphilis. There is, however, a singular increase in the efficacy of this drug, produced by combining it with a salt of iron, or better still, a double salt of iron and quinine. In this combination it will rarely fail to stay the progress of the disease. (Dr. P. Eade, p. 218.)

AFFECTIONS OF THE EYE AND EAR.

CATARACT.—*Extraction by Linear Flap.*—The patient being completely narcotised, the lids kept apart by the stop speculum, and the eye fixed with a pair of forceps, enter a Von Grüfe's cataract knife about the junction of the middle with the upper third of the cornea, bring out the point at a counter-puncture similarly placed; the knife must be held parallel to the iris, and be carried upwards about two lines and a half, then turned forwards and the section completed by a gentle sawing movement, thus forming a very small elongated flap, situated well back in dense tissue with sloping sides and transverse centre, occupying the summit of the true cornea. The above is an outline of the mode of procedure in this operation, which is said to be more successful than the ordinary modes of extraction. (Dr. C. Taylor, *p.* 174.)

Linear Extraction of Cataract.—After lacerating the capsule, the scoop should be passed so as slightly to draw open the pupil, and then pressed with its edge against the outer margin of the lens, which immediately tilts over on its axis, and the scoop being thrust onwards, the cataract comes to lie in the hollow of the instrument, and may then be withdrawn from the eye. The scoop must never be passed behind the lens as it lies imbedded in the vitreous. It is a decided advantage to make the incision in the cornea well forwards; if it is made too near the sclerotic, there is much greater chance of prolapse of the iris and escape of the vitreous. The usual curved scoop is the best, but it is an advantage to have its end serrated, and the curve well fenestrated, as the lens fits more firmly into this open space, than it does with an ordinary spoon-shaped scoop. (Mr. C. Macnamara, p. 180.)

New Operation for Cataract.—That operation is the best which the most diminishes the risk of primary corneal suppuration and iritis. These objects are gained by a small size of the corneal wound, and the removal of the lens in its unopened capsule. The operation is performed as follows:—The patient is kept throughout the operation fully under the influence of chloroform, so fully indeed, that he shows no signs of reaction when the conjunctiva of the eye which is not to be operated on is seized with forceps. A small incision is then made at the corneo-sclerotic junction, the adjacent portion of iris is excised, the zonula is opened with a small hook close to the wound, the scoop is gently passed into the vitreous cavity just behind the lens, and the latter is slowly removed. The after-treatment may be much as after extractions, the eye may, however, be safely examined at a much earlier period, say 24 to 36 hours after the operation. (Mr. T. Windsor, p. 178.)

NEW AURISCOPE AND ENDOSCOPE COMBINED.—Mr. J. Hinton, of Guy's Hospital, recommends an instrument which is handy, of moderate cost, and combines both an auriscope and endoscope. It has also the peculiar advantage of enabling two persons to see at the same time. A woodcut and description will be found at p. 185.

REMOVAL OF FOREIGN BODIES FROM THE EAR.—Let the surgeon take six inches of fine wire and double into a loop; then, having the patient placed on his side, pass the loop into the ear as far as it will go, and turn it a little gently. At the first or second withdrawal the foreign body will come out in the loop. The wire being flexible gives no pain, and cannot possibly do damage. (Mr. J. Hutchinson, p. 185.)

MIDWIFERY, ETC.

CANCER OF THE BREAST.—Neuralgic pains down the arms is a symptom of great value in determining the nature of a tumour of the breast; for it is rarely present in any other affection of that organ than the cancerous. It indicates the probability that the deep cervical or axillary glands are implicated in the disease, and are pressing on the nerves, causing pain, although *these* glands may not be felt. (Mr. T. Bryant, p. 252.)

Dr. Richardson's styptic colloid is an excellent application to ulcerating cancer of the breast. The horrible effluvium from the wound ceases and

healthy granulations spring up. The wound may even close up altogether, and the enlarged axillary glands disappear. A very interesting case in illustration of this is given. (Dr. J. Lowe, p. 25.)

CANCER OF THE UTERUS.—Dr. Atlee, of Philadelphia, has great faith in the internal use of arsenic in cases of cancer of the uterus, combined with the local use of a very strong solution of iodine in glycerine. (Mr. T. S. Wells, p. 251.)

DEODORIZED SPONGE TENTS.—Let the tent be made upon a silvered knitting-needle as an axis, and after it is finished and the needle removed, stop up one end of the central canal with a drop of wax. Now carefully fill the canal with powdered permanganate of potash, and close the other end with wax. It is then ready for use, and on removal no unpleasant odour can be detected. It is well to keep tents of different and regular sizes, properly numbered according to the size. (Dr. J. H. Aveling, p. 235.)

Carbolized Sponge Tents.—Sponge suitable for tents may be generally got from the chemists in small pieces at a very small cost. These should be very thoroughly washed, then carefully dried, and cut into pieces two inches in length. The tents usually sold are far too long. The shape should be slightly fusiform. The sponge must now be tightly wrapped round with tape, spirally applied. An axis is required on which to roll the sponge whilst the tape is fastened round it, and nothing answers better for this purpose than a slender square sided instrument, called a "broach"; on a round wire, or other tool it constantly slips during the making The tent must then be dipped into some melted cocoa-butter, or oil of theobroma, containing from 24 to 30 grains of white wax, and a drachm and a half of pure carbolic acid for each ounce of oil. Tents containing carbolic acid as recommended do not emit any unpleasant odour when removed. The tape must not be removed when the sponge is dipped, the cellular structure of the latter being readily permeated by the oil through the tape. When the tent is quite solid the tape is to be cut and rolled off. The pure crystalline carbolic acid is the proper preparation to use. (Mr. R. Ellis, p. 231.)

FIBROUS TUMOURS OF THE UTERUS.—There are five ways in which nature may spontaneously cure a fibrous tumour of the uterus. First by interstitial absorption. This is very rare but still does undoubtedly occur; it is generally preceded by softening. Dr. Matthews Duncan relates a case in which a tumour existed as large as a fœtal head at the end of pregnancy, but yet at the time of his writing no such tumour existed. Secondly, the tumour may become detached spontaneously, when it of course decomposes and becomes putrid. This only occurs to polypi. Thirdly, a fibrous tumour may become transformed into a calcareous mass, which ceases to grow, and may be partly or entirely discharged. This is a much rarer event than either of the preceding. A tumour which has undergone this transformation ceases to be productive of symptoms except such as may arise from its mechanical influence. It may be discharged *per vaginam*. Fourthly, the tumour may disintegrate and slough away. This process generally gives rise to symptoms of a grave kind, and in many instances the patient has actually sunk under the wasting effects of the discharge and the constitutional irritation. Lastly, the tumour may be eliminated by parturient efforts, the tumour being still in vital connection with the womb when the contractions begin. It is singular that this is more dangerous to the life of the patient than even the fourth or sloughing process. (Dr. A. H. M'Clintock, p. 247.)

Dr. Atlee, of Philadelphia, who has had much experience both in cases of ovarian tumour, and in fibrous tumours of the uterus, states that in his early practice he had excised large fibrous tumours, but the result had not encouraged him to repeat such operations unless the patient was in imminent danger. He had also become much less willing than formerly to incise these tumonrs by the vagina. He has had some cases in which the continued use of ergot and incision has been followed by the death and expulsion of large tumours, or their disintegrated remnants, and by the complete recovery of the patient; but the risk of uterine phlebitis and pyæmic fever has proved to be so great that he has become gradually less willing to interfere, unless

driven to do so by hemorrhage or some other condition endangering life. And he has become more hopeful of these tumours becoming indolent as age advances, and at length undergoing a process of atrophy or spontaneous cure. He has been in the habit of giving ten grain doses of muriate of ammonia three times a day, and thinks benefit has followed its use. Mr. S. Wells gives chloride of calcium with the object of promoting calcification of the vessels of these growths. (Mr. T. S. Wells. p. 251.)

GRANULAR INFLAMMATION OF THE CERVICAL CANAL OF THE UTERUS.—In this affection the cervical canal presents the appearance seen in granular ophthalmia, but the tint of mucous membrane is often more approaching a deep purple. As the os is generally more open than usual the dark granulations may be pretty easily seen by means of a speculum with direct sunlight. When seen in the cervical canal it is generally present also in the uterine cavity itself, and this may be verified by the use of the endoscope, by the aid of which the healthy may be distinguished from the inflamed patches of mucous membrane. The disease is curable enough by the application of caustics to the granular portions of the mucous membrane. The details of this treatment will be found described in the article at p. 238. (Dr. F. Churchill. p. 237.)

INDUCTION OF PREMATURE LABOUR.—Undoubtedly the best way of inducing premature labour is that known as Cohen's. In this plan fluid is injected between the uterine walls and the membranes by means of a catheter. A great improvement on this is to use a tube with a single aperture at the end instead of with side apertures. The object of this is that the fluid may pass directly upwards to the fundus of the uterus. Experience shows that for sure action of injection, it is necessary that the fluid injected should approach as near as possible to the fundus of the uterus, this being the most sensitive to irritation. (Prof. J. Lazarewitch, p. 224.)

The following is the mode of application of the douche for the purpose of inducing premature labour. Place the patient in the usual obstetric position, with the hips drawn well over the edge of the bed; by passing a full sized Fergusson's speculum the os may then be brought into view and the nozzle of an ordinary syphon syringe inserted into it. A continuous stream of water must then be injected into the cavity of the uterus. Before using the syringe care must be taken to fill it completely with water, so as to exclude the admission of any air into the uterine sinuses. Tepid water answers very well, and it is unnecessary to use alternately hot and cold water. (Dr. T. Telford, p. 226.)

MENORRHAGIA.—*Plugging the Uterus.*—Supposing in a case of large heavy uterus with unhealthy granular mucous membrane, menorrhagia cannot be arrested either by plugging the vagina, or even injecting the uterus with a solution of iron, what is the proper treatment to adopt? Plug the uterine cavity itself by means of small pieces of cotton-wool, to which threads of silk are attached for facility of withdrawal. These pieces of cotton-wool must be dipped in a solution of sesquichloride of iron and introduced by means of a uterine sound, through a speculum. The plugs should be allowed to remain *in situ* for twenty-four or forty-eight hours, and on removal a number of fresh ones inserted. The probability is that the hemorrhage will be permanently checked, and the next menstrual period be normal. (Dr. Murray, p. 256.)

OVARIOTOMY.—*Extra versus Intra-Peritoneal Treatment of the Pedicle.*—The practice of tying the pedicle of an ovarian tumour, whether the ends of the ligature are left hanging out or are cut off short, is not so successful as that where the pedicle is brought outside the abdomen by means of a clamp. Sinus and abscess are the occasional results of the former plan, and not unfrequently a matting together of the intestines in the neighbourhood of the ligatures occurs, causing a liability to obstruction of the bowels. Patients who recover after the extra-peritoneal treatment of the pedicle, as a rule, soon regain and maintain perfect health. So do many of those who recover *after the intra*-peritoneal treatment; but some of them, sooner or later, suffer *from chronic* suppuration, hæmatocele, or fæcal fistula; or, perhaps, with-

out any definite local ailment, are many months before they become strong and well. (Mr. T. S. Wells, p. 255.)

Polypus Uteri.—The author gives a report of forty-one cases of uterine polypus, all of which were removed by the annealed steel wire-rope écraseur. He finds this mode preferable to that of excision, as there is no hemorrhage, and less tendency to purulent absorption. No death occurred in any of this series, and the removal was not in any case followed by hemorrhage. A point much insisted on is the necessity of guarding against the decomposition which is liable to occur in the clots of blood and uterine secretions. For this purpose a pint of warm water thrown up the vagina twice a day, with or without a little disinfectant, is very useful. There is one great advantage in the removal of polypi by "écrasement," that by the crushing process a line of condensed tissue results, which doubtless forms a barrier to the ready absorption of unhealthy matter. (Dr. J. B. Hicks, p. 239.)

In a paper illustrative of his treatment in forty cases of polypus uteri, all of which ended successfully, the author states that having seized the polypus with a vulsellum forceps he always first attempts enucleation. For this purpose a slight scratch with the finger-nail has generally answered, and once begun it is easily completed, providing the tumour is within reach; sometimes a slight snip with the scissors may be required. Many tumours may be twisted off by a few turns of the vulsellum, but most require excision. For this purpose a pair of strong curved scissors, ten inches long, are required. Of the forty cases, nine were removed by ligature applied by means of the double canula. The material used was whipcord, and it was tightened morning and evening. A point of great importance in the use of the ligature is care not to include any portion of the uterus. The late Dr. Wm. Hunter lost more than one patient in this way, as he ascertained by examination after death. It is better not to include all the stalk of the tumour, a the part left perishes, and passes away in the discharges. (Dr. R. Dyce, p. 243.)

Puerperal Fever.—An interesting case of puerperal pyæmia is related by Dr. Snow Beck in which after death the uterus was found large and flabby, with the uterine sinuses so patent as to admit of fluid being injected into them. These sinuses contained pus, the source of which was a portion of putrifying placenta adherent to the uterus. The case well illustrates the cause of one of the most fatal forms of puerperal fever. It appears very questionable whether puerperal fever ever occurs with a firm contracted state of uterus, and the opinion is gaining ground, though it is not undisputed, that purulent infection of the general system in all endogenetic cases occurs through the pervious state of the uterine sinuses. When purulent infection has occurred, the uterine cavity must be cleansed out by some disinfecting fluid, as a solution of iodine or of permanganate of potash, and the sulphates given internally. Dr. Barnes states that he has repeatedly seen puerperal fever after perfect contraction of the uterus, but Dr. Snow Beck says that the cases given in support of this statement were not real cases of puerperal fever. (Dr. S. Beck, p. 228.)

Puerperal fever may be divided into *two great classes* with a few minor kinds, namely, that produced by decomposition of a clot, or the secretions in the uterus, thence called endogenetic; and that derived from zymotic poisons, of which scarlatina and erysipelas are the most common. In my own practice I have found that about three-fourths of the puerperal fever I see is somehow mixed up with scarlet fever. Of all disinfecting fluids for application to the uterine and vaginal cavities the permanganate of potash is the preferable one, because it shows when it has done its work. (Dr. J. B. Hicks, p. 230.)

The taint conveyed from post-mortem examinations is a fertile source of puerperal fever. This is well illustrated by the statistics of the Vienna lying-in hospital, where, at one time, when the students passed directly from their dissections to the lying-in women, the mortality was one in every ten deliveries. When this arrangement was changed the per-centage of deaths fell to 1 in 74. (Dr. H. Davis, p. 231.)

Puerperal Fever and Erysipelas.—A circumstance is related, well illustrating the connection between puerperal fever and erysipelas. Some years ago a lying-in ward had been established at King's College Hospital. The utmost care had been taken in its construction and management, but in spite of every precaution the mortality had been for the last year or two excessively high. A short time ago numerous cases of erysipelas appeared in the surgical wards, and immediately afterwards the two most recently confined women were attacked with a very adynamic form of puerperal fever, which proved fatal in both instances. There could be no doubt that the cause of the disease was the same as that which was producing erysipelas in the surgical wards. (Dr. Playfair, p. 229.)

Vaginal Examinations.—In vaginal examinations at hospitals, where many women are examined in succession, great care should be taken to secure cleanliness and prevent infection, by anointing the finger before each investigation, with a compound of purified soft soap, glycerine, and carbolic acid, in lieu of grease or oil, and the subsequent use of a solution of Condy's fluid. ℞. Glycerini, spir. vini rect. āā ℥ ss., saponis mollis ℥j., acidi carbolici ʒ ss. (Med. Times and Gazette, Jan. 11, 1868, p. 34.)

The Vectis in Labour.—The vectis must be considered as an artificial hand. It may be made of the greatest use in protracted and difficult labour, not so much by using it as a powerful tractor, for which it is not adapted, but by gentle and sustained traction during a pain, and by keeping the head from receding between the pains. It is so readily applied, and may be carried so easily in the pocket that it seems astonishing it is not more employed than it is. (Mr. G. D. R. McCarthy, p. 224.)

MISCELLANEA.

Action of Belladonna combined with Opium.—If a small quantity of atropine (the $\frac{1}{96}$th of a grain) is combined with morphia when subcutaneously injected, the faintness, nausea, and tendency to retching which is occasionally produced by morphia alone, is never seen. The combination of opium with belladonna, whether given by the skin or by the stomach, is an excellent one, as the belladonna intensifies the action of the opium and also does away with many of its unpleasant effects. (Dr. J. Harley, p. 270.)

Bichloride of Methylene.—The author has now administered the bichloride of methylene for the performance of nine capital operations. In one case the insensibility was sustained for an hour and seven minutes; in no case was the insensibility less than thirty-five minutes. In all there was the following order of phenomena: a first stage, of slight excitement; a second of slight rigidity; and a third, very prolonged, of relaxation and complete insensibility. If the inhalation is carried too far, we have a fourth stage of prostration, which is immediately preliminary to death. The prolonged anæsthesia is the most striking feature of its action: but there is no occasion for anxiety from, nor for interference with, this sleep; it passes off quite naturally if time be allowed, and what is more, it passes off leaving no headache and no feeling of depression behind. Bichloride of methylene is at least as safe as chloroform; and it possesses certain advantages of its own which give it a better position amongst anæsthetics than chloroform. (Dr. B. W. Richardson, p. 274.)

Professor Nussbaum, of Munich, has been employing this anæsthetic, but does not consider it preferable to chloroform beyond being of a pleasanter odour. He found the effects produced in all the stages of its administration to be scarcely distinguishable from those produced by chloroform, except that complete return of consciousness occurred somewhat later. (Medical Times and Gazette, Jan. 25, 1868, p. 111.)

Disinfectants.—All the organic poisons so important in the causation of *disease* possess a more or less complex constitution, yet of very little sta-

bility, as evidenced by the fact of their decomposition at about 212°. Hence arises our hope of their direct destruction by oxidation. From this it follows that our principal disinfectants are oxidizing ones, which render the poison harmless by reducing it to its simplest form. Antiseptics are the second class of disinfectants, these place the matter in a state unfavourable for its destruction by oxidation or otherwise. (Dr. W. Procter, p. 28.)

Internal Antiseptics.—The sulphites are the most readily absorbed of our internal antiseptics, but carbolic acid is the most powerful. The great desideratum is a salt which shall combine the two. This desideratum the author has succeeded in obtaining in the production of the sulpho-carbolates. (Dr. A. E. Sansom, p. 23.)

Poisoning by Carbolic Acid.—Care must be taken in the external application of carbolic acid over large surfaces, as three cases of poisoning by it have occurred in the workhouse of Aston Union, near Birmingham. It was applied in a wholesale manner in mistake for sulphur lotion for the cure of itch. Two of the cases proved fatal. (Mr. E. S. Machin, Brit. Med. Journal, March 7, 1868, p. 220.)

Poisoning by Cyanide of Potassium.—The best antidote is a strong solution of sulphate of iron. As an accident from the cyanide is most likely to occur where photography is carried on, the remedy above-named is generally at hand. (Dr. Alfred S. Taylor, Guy's Hospital Reports, 1868, p. 239.)

Snake Poison.—Mr. Charles J. Smith, late Inspector-General of Hospitals, Madras Army, after making many experiments on dogs, and witnessing the effect of the antidote he recommends in the case of snake-bite in a man, considers that in the liquor ammoniæ diluted just sufficiently to enable the patient to swallow it, we possess an efficient remedy. A tourniquet ought to be placed upon the limb bitten, artificial respiration kept up if necessary, and the liquor ammoniæ freely administered. It is curious that neither prussic acid, opium, nor arsenic are poisonous to snakes, whilst tobacco is so fatal to them that a pinch of snuff is sufficient to poison them. (British Medical Journal, Feb. 22, 1868, p. 164.)

Protoxide of Nitrogen.—This gas has been pretty well tried as an anæsthetic at St. Bartholomew's and also at University College Hospitals. It acted pretty well, but there is a general feeling that it is not comparable with chloroform in many important particulars. Useful where a single tooth has to be extracted, most persons would prefer taking chloroform had they to lose three. (Lancet, May 2, 1863, p. 563.)

Septicides.—The author states that he has succeeded in producing a series of salts which combine the diffusibility of the sulphites with the potential efficacy of carbolic acid. These salts are the sulpho-carbolates. He does not agree with Dr. Polli in his idea that the sulphites do not act upon the organized germs of disease, but upon the pabulum in which these germs are propagated. He believes they act directly as septicides. (Dr. A. E. Sansom, p. 20.)

Subcutaneous Injection of Morphia.—*Dangerous Results from.*—We cannot feel quite comfortable in the constant use of subcutaneous injections after reading a case which occurred to Mr. Braine, of London. The patient fell back to all appearance dead, and was only brought round with great difficulty. It could not well have been from air entering a vein, for the injection was made just over the insertion of the deltoid. Great care should be taken that the syringe when charged with the solution to be injected is free from air, and is clear from minute floating particles. (Medical Times and Gazette, Jan. 4, 1867, p. 8.)

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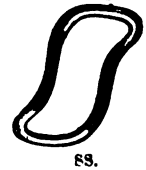

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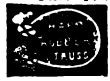

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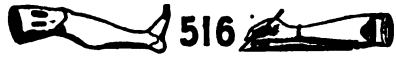

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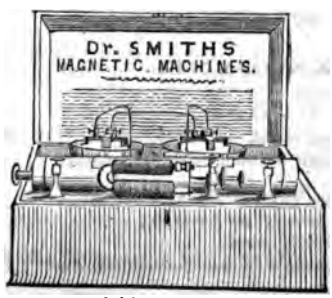

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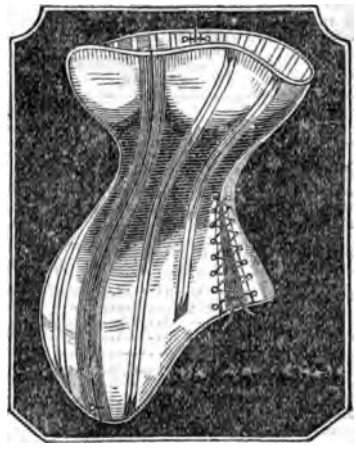

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Lime	.510		.152	
Potash	.211		none	
Ammonia	2.862		none	
Inorganic Constituents		8.988		.586
Water and Loss		21.747		3.009
		100.000		100.000

The extract is thus shown to contain 135 times as much of non-oleaginous organic substances as the oil, and 15 times as much of inorganic elements. These two classes of bodies together form 78.153 per cent. of the extract, but only 1.324 of the oil. In a table-spoonful of the oil, which comprises 240 grains, there are therefore present hardly 2½ grains of the above matter, while the extract is almost wholly composed of them.

PRICES.—Box containing 60 Dragées, equal to 1¼ pints of the best Cod-liver Oil, 75 cents. Box of 120 Dragées, equal to three pints of oil, $1 25. Box of 240 Dragées, equal to 6 pints of oil, $2.

Samples furnished free to physicians

For full account of the discovery of Cod-liver Extract and its remedial properties, apply for pamphlet, which will be forwarded to any address on application, by

WARD, SOUTHERLAND & CO., Wholesale Druggists,
128 and 130 William Street, New York,
Wholesale Agents for the United States.

Entered according to Act of Congress, in the year 1867, by DAVID W. FELT, in the Clerk's Office of the District Court of the Southern District of the State of New York.

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